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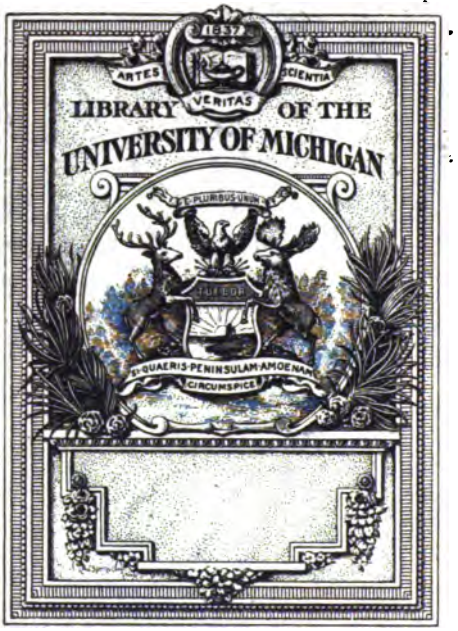
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THE

RETROSPECT

OF

PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M. D.,

LECTURER ON OBSTETRIC MEDICINE AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

J. BRAITHWAITE, M. D.

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CONTENTS OF VOL. XLIV.

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ARTICLE.		AUTHOR.	PAGE.
1	On Fever in the Zambesi	*Dr. Livingston*	17
2	On the Difference between *Curing* and *Treating* a Fever	*Dr. P. M. Latham*	18
3	On Typhus Fever	*Dr. Bell*	20
4	Fever in the Crimea	*Dr. Lyons*	22
5	Pathognomonic Sign of Scarlatina	*M. Bouchut*	23
6	On the Action of Sesquicarbonate of Ammonia in Scarlet Fever	*Dr. John M'Nab*	24
7	On the Use of Benzoate of Ammonia in Renal Dropsy succeeding Scarlatina	*Dr. J. Taylor*	24
8	On Arsenious Acid as a substitute for Quinine in Fever	*J. Turner, Esq.*	25
9	On Actea Racemosa in Acute Rheumatism	*Dr. F. R. Mac Donald*	25
10	Case of Acute Rheumatism, with unusual Complication	*Prof. Reid*	26
11	Medicinal Treatment of Gout	*M. Trousseau*	27

DISEASES OF THE NERVOUS SYSTEM.

12	On Acute Softening of the Medulla Spinalis	*Dr. Robert Mayne*	28
13	On Reflex Paraplegia	*Ed. Brit. Med. Journal*	29
14	Some Remarks on Paralysis	*Dr. C. E. Brown-Sequard*	34
15	Treatment of Delirium Tremens by large Doses of Tincture of Digitalis	*Dr. Francis E. Carey*	37
16	Digitalis in Delirium Tremens	*Dr. D. M. Williams*	38
17	Delirium Tremens treated by Large Doses of Tincture of Digitalis	*Dr. Thos. B. Peacock*	39

ORGANS OF CIRCULATION.

18	On Blood-letting in Apoplexy	*Dr. T. K. Chambers*	41

ORGANS OF RESPIRATION.

19	On the Uncertainty of the Sputum as a Diagnostic Feature in Pneumonia	*Dr. Thos. Williams*	44
20	Chlorate of Potash and Sugar in Phthisis	*Dr. James Turle*	48
21	New Remedy for Whooping-Cough	*M. Foster, Esq.*	49
22	On Arsenic Smoking in Asthma	*Dr. Fred. G. Julius*	50
23	Drill for Auscultation	*Dr. T. K. Chambers*	50

ORGANS OF DIGESTION.

ARTICLE.		AUTHOR.	PAGE.
24	Curious Cases of Furred Tongue on one Side, from various Causes	*John Hilton, Esq.*	53
25	On Diphtheria	*Dr. William Budd*	56
26	On the Treatment of Acute Dysentery by Ipecacuanha	*Dr. R. W. Cunningham*	57
27	On the Treatment of Diarrhœa by Nitric Acid and Opium	*Dr. Patrick J. Hynes*	59

URINARY ORGANS.

28	On the Diseases of the Kidney, accompanied by Albuminuria	*Dr. C. J. B. Williams*	60
29	On Uræmic Intoxication	*Dr. W. A. Hammond*	62
30	The Urine in Albuminuria	*Dr. Lionel S. Beale*	63
31	On a New Clinical Method of estimating the Quantity of Sugar in Diabetic Urine	*Dr. William Roberts*	64
32	New Test for Saccharine Urine	" " "	67
33	On Anasarca	*Dr. Charles Witt*	68

SURGERY.

AFFECTIONS OF BONES AND JOINTS, &c.

34	On Amputations	*Thos. P. Teale, Esq.*	70
35	On the Influence of Rest in the Treatment of Diseases of the Joints	*John Hilton, Esq.*	71
36	On Loose Cartilages in the Knee-joint	*W. Joseph Square, Esq.*	75
37	Excision of the Knee-joint:—Recovery with a False Joint, and a Useful Limb	*Prof. Fergusson*	79
38	Treatment of Housemaid's Knee by the Thread Suture	*Fred. C. Skey, Esq.*	79
39	Excision of a Portion of the Os Calcis for Caries	*Dr. J. K. Barton*	80
40	Pirogoff's Operation	*Prof. Fergusson*	82
41	On Dislocations of the Shoulder-joint	*Dr. N. R. Smith*	82
42	On the Treatment of Fracture of the Lower Extremity of the Radius	*Dr. Alex. Gordon*	86
43	Complete Restoration of the Tibia by the Periosteum, after its Removal	*M. Maisonneuve*	90
44	On Anterior and Posterior Curvatures of the Spine	*William Adams, Esq.*	91

ORGANS OF CIRCULATION.

45	Traumatic Aneurism of the Femoral Artery	*Dr. Thos. J. Rawson*	96
46	On the Treatment of Popliteal Aneurism by Compression	" "	97
47	Large Gluteal Aneurism; Ligature of the Gluteal Artery	*Prof. Syme*	97
48	The Treatment of Varicose Veins and Ulcers by Means of Blisters	—*Ure, Esq.*	99
49	On the Surgical Treatment of Vascular Tumours	*P. C. Price, Esq.*	100
50	On the Radical Cure of Varicose Veins	*John Wood, Esq.*	103
51	A Bullet Lodged in the Heart for Twenty Years	*Dr. Balch*	104

ALIMENTARY CANAL.

52	A Rare Instance of Conservative Surgery	*Dr. J. Nichols*	105
53	Ulcerated Throat	*Ellis Westcott, Esq.*	106

ARTICLE.		AUTHOR.	PAGE.
54	On the Mode of Operating for the Radical Cure of Hernia	*John Wood, Esq.*	106
55	On a New Operation for the Radical Cure of Hernia	*Prof. Chisholm*	107
56	Complicated Case of Oblique Congenital Inguinal Hernia	*Dr. Wm. J. Wilson*	109
57	Perforation of the Intestine	*M. M. Velpeau and Verneuil*	112
58	On some Diseases of the Rectum	*James Lane, Esq.*	114
59	On the Treatment of Certain Forms of Prolapsus Ani where the Ligature is not Admissible	*Henry Smith, Esq.*	117
60	On Flat Ulcers of the Rectum	*William Coulson, Esq.*	122
61	On Abdominal or Pelvic Abscess	*Fred. C. Skey, Esq.*	122

ORGANS OF URINE AND GENERATION.

62	On Incisions for Stricture Compared with Dilatation	*John Gay, Esq.*	124
63	On the Immediate Treatment of Stricture of the Urethra	*Barnard Holt, Esq.*	126
64	Treatment of Stricture of the Urethra by Sudden and Forcible Dilatation	*Christopher Heath, Esq.*	132
65	On Incision of Stricture of the Urethra	*Dr. James Arnott*	134
66	On Phymosis	*P. C. Price, Esq.*	139
67	Lithotomy,—Lateral Operation	*Prof. Fergusson*	140
68	Radical Cure of Varicocele	*Henry Lee, Esq.*	140
69	On the Radical Cure of Varicocele	*J. Redfern Davies, Esq.*	141
70	New Remedy for Irritable Conditions of the Bladder	*Henry Thompson, Esq.*	142

DISEASES OF THE SKIN.

71	On Tubercular Disease of the External Lymphatic Glands	*P. C. Price, Esq.*	143
72	On the Treatment, with Excision, of Wens and of some other Cysts	*Prof. Courty*	147
73	On the Prevention of Pitting in Small-pox, by the Application of Linimentum Aquæ Calcis and Cotton Wool	*Dr. Joseph Bell*	150
74	Observations on the Treatment of Deformities Resulting from Severe Burns	*Dr. J. K. Barton*	151
75	Treatment of Scabies by Benzine	*J. J. Godfrey, Esq.*	157
76	On the Treatment of Phthiriasis	*Dr. T. McCall Anderson*	158
77	On the Treatment of Eczema	*Prof. Hebra*	159

SYPHILITIC AFFECTIONS.

78	On Syphilis	*Henry Lee, Esq.*	161
79	Treatment of Gonorrhœa by Injections of Liquor Hydrargyri Pernitratis	*G. Borlase Childs, Esq.*	164
80	Subnitrate of Bismuth as an Injection in Acute Gonorrhœa	*Dr. Mouslon*	166
81	Gonorrhœa	*Dr. T. K. Chambers*	166
82	On the Cure of Gonorrhœa by Blisters	*H. Chalmers Miles, Esq.*	166
83	Chlorate of Potash in Gonorrhœa	*Dr. Irvin*	170
84	How is Syphilitic to be Distinguished from Non-Syphilitic Psoriasis	*Dr. T. M'Call Anderson*	170

DISEASES OF THE EYE AND EAR.

85	On the Relief of Symblepharon by Transplantation of Conjunctiva	*T. P. Teale, jun., Esq.*	
86	Presbyopia	*J. S. Wells, Esq.*	

ARTICLE.		AUTHOR.	PAGE.
87	Black Eye	"	173
88	On Catheterism of the Eustachian Tube, and on the Pharyngoscope	*Dr. Voltolini*	173

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

89	On the Induction of Premature Labour	*Dr. J. Y. Simpson*	178
90	Indications and Operations for the Induction of Premature Labour, and for the Acceleration of Labour	*Dr. Robert Barnes*	180
91	Induction of Premature Labour	*Dr. Arthur Farre*	184
92	On the Treatment of Cases of Abortion, in which the Membranes and Placenta are Retained	*Dr. W. O. Priestley*	185
93	Accidental Separation of the Placenta at the Seventh Month of Pregnancy	*G. E. Stanger, Esq.*	186
94	The Treatment of Placenta Prævia	*Dr. Robert Barnes*	188
95	Observations on Placenta Prævia	*Dr. Charles Clay*	193
96	On a Peculiar (Syphilitic?) Degeneration of the Decidua	*Prof. Virchow*	195
97	What Influence has Contraction in Stopping Uterine Hemorrhage	*Dr. James Adams*	195
98	On Three Cases of Puerperal Convulsions	*Dr. Robert Harper*	196
99	Use of Chloroform in Puerperal Convulsions	*Dr. Fred. Page*	198
100	On a Case of Epileptic Puerperal Convulsions	*Dr. F. W. Wilson*	199
101	On Supporting the Perineum in Labour	*Dr. Grailly Hewitt*	199
102	On Laceration of the Perineum	*Dr. James Gray*	200
103	On Supporting the Perineum in Labour	*Dr. E. W. Murphy*	201
104	On Rupture of the Perineum During Labour	*Dr. Thomas Skinner*	203
105	Is the Ergot of Rye, when Administered to the Mother during Labour, dangerous or not to the Life of the Child?	*Dr. Uvedale West*	204
106	On the Use of the Forceps in Tedious Labour	*Dr. G. Hamilton*	205
107	On a Modified Form of Obstetric Forceps, to be used in Cases where the Ordinary Ones are Inapplicable, owing to Unusual Elongation of the Fœtal Head	*Dr. Grailly Hewitt*	208
108	Some Practical Observations on the Timely Use of the Obstetric Forceps	*Dr. Edward Sinclair*	209
109	On the Obliquity of the Fœtal Head in the Mechanism of Parturition	*Dr. J. M. Duncan*	215
110	An Effort to Shorten the Duration and Diminish the Pain of the First Stage of Labour	*Dr. B. Fordyce Barker*	223
111	Spontaneous Evolution		226
112	Impeded Labour from Dorsal Displacement of the Arm	*J. J. Murray, Esq.*	226
113	On Uterine Hemorrhage	*Dr. G. Hamilton*	228
114	Extra-Uterine Gestation	*Dr. George Cheesman*	230
115	Improvement in the Speculum		233
116	On Vesico-Vaginal and Recto-Vaginal Fistulæ	*T. Spencer Wells, Esq.*	233
117	On a New and Successful Mode of Treating Vesico-Vaginal Fistula	*M. H. Collis, Esq.*	236
118	On Secondary Abscess with Disorganization of the Knee-Joint, following Scarlatina and Parturition	*W. Coulson, Esq.*	240
119	Operation for and Perfect Cure of Procidentia Uteri from Perineal Laceration of many Years' Standing	*Dr. D. Lloyd Roberts*	243
120	On Abdominal Collapse	*Dr. Robert Barnes*	245
121	On Diphtheritic Ulcer of the Uterus	*Robert Ellis, Esq.*	250
122	On Inflamed Ulcer of the Uterus	*Do.*	252
123	On Removal of the Cervix Uteri	*Dr. R. Greenhalgh*	255
24	On the Effects of Undue Application of Caustic Potash to the Uterus	*Dr. W. Whitehead*	256

ARTICLE.		AUTHOR.	PAGE.
125	On Ovariotomy and its Results	*Dr. Charles Clay*	256
126	On Ovariotomy	*T. Spencer Wells, Esq.*	263
127	Hysteria Masking Secondary Syphilis	*W. Coulson, Esq.*	265
128	On Irritation of the Urethra in Females	*John Hunter, Esq.*	265
129	Explanation of Hysterical Pains in the Hip or Knee-Joint	*John Hilton, Esq.*	266
130	On Leucorrhœa and Vaginitis	*Dr. Charles Walker*	267
131	Electricity in Amenorrhœa	*Dr. Althaus*	270
132	On the Local Treatment of Amenorrhœa	*Dr. J. Y. Simpson*	271
133	Case of Dysmenorrhœal Membrane	*Dr. Beatty*	274
134	On Dysmenorrhœa and Sterility	*Dr. John Coghlan*	275
135	On Inflammation of the Breast, with an Analysis of Seventy-two Cases	*T. W. Nunn, Esq.*	276
136	On Saccharine Fermentation in the Milk within the Female Breast	*Dr. George D. Gibb*	278
137	Weight of New-Born Infants	*Dr. E. Von Siebold*	279
138	Researches on Asphyxia with Observations on the Effects of the Hot Bath	*Dr. A. T. H. Waters*	280
139	The Atrophy of Children from Dirt Eating	*Dr. William Moore*	282
140	On Cerebral Disease Unattended by Cerebral Symptoms	*Dr. Charles West*	284

MISCELLANEOUS SUBJECTS.

141	On the Use of Alcoholic Stimulants in Hospital Practice	*Dr. W. T. Gairdner*	289
142	On the Action and Uses of Alcohol	*Dr. Daniel Hooper*	291
143	On Veratrum Viride	*Dr. Otterson*	296
144	On the Galvano-Caustic Apparatus	*Dr. T. J. Walker*	298
145	On the Influence of Ozone on Organic Substances	*Dr. T K. Hornidge*	300
146	Ferri Carbonis Effervescens; A New and Elegant Form of Chalybeate	*Dr. Thomas Skinner*	301
147	On the Chronometry of Life and Disease	*A. de Noe Walker, Esq.*	302
148	On Internal Obstruction by Bands	*John Gay, Esq.*	305
149	Crinoline Accidents	*Dr. Odling*	307
150	Experiments Relating to the Diuretic Action of Colchicum	*Dr. W. A. Hammond*	308
151	The Hot Bath: its Physiology, Use, and Abuse: especially in Connexion with the Treatment of Drowning	*Charles Hunter, Esq.*	311
152	Rules for Guidance in Chloroform Accidents	*Dr. Kidd*	313
153	On the Treatment of Gout	*M. Trousseau*	314
154	Formula for the Administration of Chloroform, Ether, Turpentine, Camphor, or Essential Oils	*M. Vee*	316
155	Essence of Beef	*Dr. Druitt*	316

SYNOPSIS.—COMMENTARY ON MIDWIFERY AND THE DISEASES OF WOMEN, ETC.—INDEX.—GENERAL INDEX.

PRACTICAL MEDICINE.

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON FEVER IN THE ZAMBESI.

By Dr. LIVINGSTONE.

[At the Epidemiological Society, Dr. McWilliam read the following interesting note of Dr. Livingstone on the fever of this district.]

In the typical cases given in Dr. McWilliam's "Medical History of the Niger Expedition," the gall bladder was found distended with black bile, and if my memory does not deceive me, most of the cases treated with quinine at an early period of the disease, either recovered or were subjected to the milder or intermittent form of fever. In 1850 I adopted the plan of giving quinine, mixed with a purgative, as the first step of the treatment, and was successful in the cases of two of my own children, and an English party whom we found at Lake Ngami, and of whom one had died before our arrival. I have lost the notes of my reason for adopting the practice, but I have been successful in every case I have met with since. The prescription employed is resin of jalap, calomel, of each eight grains, quinine c. rhubarb, of each four grains, mix well together, and when required make into pills, with spirit of cardamom. Dose from ten to twenty grains. The violent headache, pains in the back, &c., all are relieved in from four to six hours, and with the operation of the medicine there is an enormous discharge of black bile; the patient frequently calls it blood. If the operation is delayed, a dessert-spoonful of salts promotes the action. Quinine is then given till the ears ring, &c. We have tried to substitute other purgatives instead of resin of jalap, and calomel; but experiments have only produced the conviction that aught else is mere trifling. No strength is lost in our march up the river, of 600 miles on foot. An European would be stricken down in one day, and the next after the operation of the remedy would resume his march on foot. In some very severe attacks it was necessary for the patient to travel on a donkey, but after two or three days he would prefer to tramp it. We tried Warburg's tincture, which has a great reputation in India, but it causes profuse sweating and does not cure the disease; the strength is also impaired. We had a good supply, by the kindness of one of our nobility, but I am compelled to say that it did not answer our expectations. The daily use of quinine is no preventive. We have seen many cases occur when the person was on the verge of cinchonism. I employed the foregoing remedy with success on the west coast, but made no fuss about it more than make a general statement in the "Missionary Travels." I was not quite sure that our fever was identical with that Dr. McWilliam encountered in the Niger, but the melancholy fate of a party of six missionaries at Linyanti, where six out of nine Europeans and four native attendants perished in the short space of three months, makes me fear that it is the same complaint as that which destroyed the officers of Commodore Owen in the Zambesi, those of Captain Tuckey in the Congo, and the crews of the great Niger Expedition in that river. My companions, Dr. Kirk and Mr. C. Livingstone, entertain the same idea of the value of our pills as I do. We wrote a paper for one of the Medical Journals, but the above s[illegible] case makes us anxious that the remedy should become more exten[illegible] known than it has been, and I do not know a better plan for eff[illegible] than by communicating it to Dr. McWilliam. Those who [illegible] remedy will do well to remember that the above doses are [illegible] I cured myself and native companions in this way durin[illegible]

between 1852 and 1856, and that the remedy has no bad effects on the system, may be inferred from the fact that I have had no regular attack of fever since my return. I have had little illnesses, probably from exposure to malaria in its most intense forms, but nothing like what I formerly experienced; and I am of opinion that what we were all taught, not to give quinine till we had used the preliminary measure of relieving the bowels, was a mistake. [Query: Might the remedy not be applied to some of the fevers at home that arise in unhealthy localities?] Around every village in this country there is a large collection of human ordure during the dry season. This is swept into the rivers by the heavy rains, and you may guess the effect from hundreds of thousands of villagers. The natives here do not drink water as the natives do on the banks of the Thames, but make holes in the sand to draw from. Possibly this has as much to do with the origin of fever as it has at home.—*Med. Times and Gazette, June* 29, 1861, *p.* 684.

2—ON THE DIFFERENCE BETWEEN *CURING* AND *TREATING* A FEVER.

By Dr. Peter Mere Latham.

Let us consider fever in its largest sense, and see first what it is to *cure* it, and then what it is to *treat* it. This difference will display the difference between *cure* and *treatment*, as applied to the management of all other diseases whatever.

Take cold, and heat, and perspiration, occurring interchangeably, whether regularly or irregularly, and in any manner or degree; and a frequent pulse, whether strong or weak, and pain in the head with wakefulness and delirium, or drowsiness and torpor; and the tongue dry and red and clean, or sordid and black; the breathing hurried, and perhaps the bronchi wheezing, the abdomen tympanitic and painful on pressure, and perhaps a sanious discharge running from the bowels; take these symptoms on any day early or late in the course of the disease; take them all in a heap, and regard them as one, and make them serve collectively for the single aim of the single remedy, and that remedy quinine. This is pre-eminently the cure of fever; and its success, which comes suddenly when it comes at all, must be a sort of triumph.

Again, take all these same phenomena which tell of disorder and disease in the vascular system and the nervous system, and in any or every organ of the body; take them, not as they appear on any one day, but as they ebb and flow and fluctuate every day and night for more than twenty days and nights in succession; and watch each, and try to interpret the meaning of each, and judge which is most perilous in itself, and which bears most perilously upon the whole disease; and which is most within reach of a remedy, and which is most urgent for relief; then choose and apply, not *the* one remedy, but remedies many and various, or few and simple, according to the aims proposed; and take care the right remedy of to-day be not the wrong remedy of to-morrow; pure air to breathe, and pure water to drink; affusions or ablutions of the whole body, cold, warm, or tepid; lotions or fomentations of this part or that; remedies for pain according to its nature and seat; expedients for preventing sores, and expedients for healing them; aperients, and astringents, and mercurial alteratives, according to their several needs; also opium, that great saver or destroyer of life as it is rightly or wrongly given in fever, in its various uses. These are the sort of implements which the physician has to work with when he has to *treat* a fever, and being thus vigilant and analytical about the bedside emergencies of fever, and dextrous in ministering to them, he saves many lives.

And this is pre-eminently the *treatment* of fever; but it may be, and often is, one part of it only. For, after all has been done that need, or can be done, in the way of thus noting particulars and ministering to them, lives cannot be saved without engaging observation and treatment in larger aims.

More in one case and less in another, more in one stage of the same case, and less in another; but in some stage of almost every case, we have to take the sum of the symptoms, and calculate whether, upon the whole, they mean strength or weakness, vital reaction or vital yielding; and to make choice accordingly, whether it be right to restrain, to lower, to deplete, or to support, to raise, to stimulate; whether to give calomel and antimonials, or to use some kind of bleeding, or to give wine, or brandy, or ethers.

This is the *treatment* of fever in another of its parts. Both together, that which is analytical and that which is summary, comprise the *whole treatment*. If it succeed, its success is after the lapse of many days, and the trial of many hopes and fears. And when success comes at last, it produces nothing like exultation and triumph, but rather quiet satisfaction and thankfulness.

Such is the difference between cure and treatment, when we come to follow them out in their operations, and to think what the one is and what the other, practically and in truth. The difference is so great, that if this vast class of diseases, called fevers, belonging to the whole world, and always existing and raging somewhere, could be taken at once out of the domain of *treatment* and transferred to the domain of *cure* now and forever; and if, instead of needing remedies, as many and various as are the times and places, and men and circumstances wherein they arise, they were found capable of being safely and successfully consigned to one and the same remedy always and everywhere, and in all persons, the practice of medicine would suffer the nature of a revolution. The thoughts, studies, habits, and feelings of medical men would be utterly changed.

So great a revolution is not likely to happen. But a man need not have grown old in the practice of medicine to bear witness to its having undergone considerable changes; and the diseases which have been the objects of them are especially those we have been speaking of; and the changes themselves have still respected *treatment* and *cure*. They have not, however, amounted to anything like a substitution of one for the other; but have only involved measures of more or less, in which treatment and cure have been mixed together, and that not always and everywhere, but partially and from time to time, yet for long times together, and in this place or that; yet it may be over many countries. Thus, febrile diseases which have a contagious or malarious origin, or which arise from causes so occult that they seem spontaneously engendered; also typhus and typhoid fevers, scarlatina, erysipelas, spontaneous febrile hemorrhage, and purpura, will for a time, even for a series of years, and over a great portion of the world, be capable of successful management by remedies addressed to the circumstances and conditions of individual cases—that is, simply by treatment. Again, some or all of these diseases in the same place, during another series of years, will yield less satisfactorily to the same remedial method. The indications will be as well chosen as before, and the medicines used will fulfil their immediate purpose, but the whole disease will not decline as easily or come to an end as soon or as completely as before. And then physicians will not be able to make up for the failure of their tried resources by seeking and finding new indications and new remedies rationally suited to them, but by using a special remedy which acts and cures nobody knows how or why—even the great specific cinchona. Then cure has become the needful auxiliary of treatment; and a special remedy given on the faith of a large experience has completed the imperfect work of remedies addressed to rational indication.

Now there are subjects upon which the most sober and practical minds cannot help speculating a little beyond what they know. Sure and great results—yet familiar and common, and procured at will and by certain means, but in an unaccountable manner—naturally set us thinking and forming notions how they come to pass; and then it is safest and best to fill up the gaps of our knowledge from analogy. When we are treating a disease, our aim may be to raise the pulse or to lower it, to cool the skin or to warm it, to purge the bowels or restrain them, to augment the secretion of this or that

gland or to diminish it or alter its quality; and as our remedies do their appointed work, we witness the whole disease brought to an end. But, when we give cinchona for an ague, we have no single aims; we give it for the whole disease, and the cure follows. Nevertheless, it is according to analogy to believe that it hits a mark of its own, which is more particular than any within the scope of our discernment. Thus cinchona, and all so-called specifics, may work after the same manner with other remedies, which are better understood. They may all equally produce their great results by intermediate operations; only in the one case the intermediate operation is seen, and unseen in the other; aimed at in the one, and unaimed at in the other.

From this view of the matter, it would turn out that our distinction between *cure* and *treatment* is not in the nature of things, but in the imperfection of our knowledge.

Further, from the same analogy, we may fairly believe that there is a certain morbid element, of which cinchona is the specific antidote; that it exists most pure and unmixed in ague, but that it may add itself as an accident to other elements whose property is to produce other forms of disease; and that wherever it exists, and in whatever combinations, its power is still predominant, and cinchona is still needed to bring the disease to an end.

These things may be as analogy would suggest. But in practice there is a mischief in conceiting ourselves wiser than we are. Granted that of the many morbid actions and sufferings which make up the disease, one only may be reached and swayed, or annulled, or curatively modified, by the one special remedy; granted, that thus the complex disease may cease by removal of some one main element, and by some intermediate operation of the remedy; but inasmuch as we know no such element of the disease, and see no such intermediate operation of the remedy, they must be to us as if they did not exist. We cannot construct aims and indications of practice out of hidden things, but must be content for the present to regard the whole disease as a single comprehensive indication, and so prescribe for it the single remedy and expect the cure. We shall perhaps know better some time; but no good will come from our pretending to know better now. Patience under imperfect knowledge is no proof of an unwise mind.—*British Med. Journal, Aug.* 31, 1861, *p.* 222.

3.—ON TYPHUS FEVER.

By Dr. Bell, Physician and Clinical Lecturer, Glasgow Royal Infirmary.

[Dr. Bell, in the series of papers of which the following extract is the conclusion, endeavours to prove that the typhus and typhoid forms of the disease are identical as to cause; that is, they both come from the same morbid poison. The doctrine which prevails as to the cause of fever (namely, that a morbid poison is absorbed into the system) is not of the slightest value in the treatment of the case. Dr. B. says:]

With regard to the theory of Liebig, that this morbid poison acts as a ferment, I beg to state that it is an hypothesis which is totally unsupported by a single apposite fact, but, on the contrary, is opposed to well-established principles. For example, if fermentation, or any process analogous to it, should be set up in the blood, such a process could never come to an end during life, in consequence of the everchanging character of the blood; the position of this fluid being perfectly dissimilar to that of the fixed quantity of saccharine fluid in the brewer's vat. Even here let a stream of this fluid be constantly escaping from the vessel, and a new portion be as constantly added to it—why, fermentation would never cease so long as these changes were in operation. This is precisely the case regarding the blood in the blood-vessels, and it appears to me perfectly inexplicable how that, with this *fact* so prominently patent to all, the theory of the distinguished chemist should have been entertained for a single moment by physicians; and it is still more astonishing that the theory should have had the effect of changing

the nomenclature of the whole class of diseases arising from morbid poisons. The term *Zymotic* is a standing reproach to the medical profession, because it shows how easily we have been captivated by a mere theory, and thereby so blinded as to overlook the well-established laws and facts connected with the process of fermentation. But, apart from considerations regarding the truth of this hypothesis, even supposing it to be correct, let me ask what advantage does it confer in the treatment of the disease? Does it suggest the shadow of an idea as to the management of a single case, or the removal of a single pathological condition which exists? Assuredly not. On the contrary, the adoption of this zymotic theory positively may do much harm; it tends to paralyze all investigation into the pathological states which actually occur. The notion that a process of fermentation must *be completed* leads the practitioner to let the disease run its course, without an effort to ward off or remove those lesions, both of function and texture, from which death results. We have seen that death in fever very seldom, if ever, arises from mere functional disturbance; but invariably, or very nearly so, from actual structural changes—viz., congestion or exudation into the brain, softening and degeneration of the muscular tissue of the heart, ulceration, and many other associated lesions of the small intestines, or from serious disease of the pulmonary organs. Let me inquire how fermentation produces such morbid results in solid tissues? As practical physicians we must discard all theory in the treatment of fever as a mere "will o' the wisp" which will lead us astray. We must base our treatment on the knowledge which we obtain in our post-mortem examinations, combined with careful clinical observations made during life. The facts so obtained must be our sole guide, and we must never neglect their valuable instruction. They teach us the following important lessons:—1st. That typhus produces an effect upon the whole frame; that the muscular system, the brain and nervous system, the circulatory system, the digestive or nutritive system, and the respiratory system, are all more or less deranged. 2nd. Besides this, observation teaches us that persons attacked by typhus are not uniformly influenced. 3rd. We see that all the different functions or systems are not uniformly attacked with equal severity, but that, in some cases, and during some epidemics, one series of lesions are more frequent or more severe than in others; that whilst, as a general law, disease of the mucous membrane of the intestines is the usual pathological manifestation of the disease, yet diseases of other organs and other tissues frequently supervene, and prove destructive to life.

Let me next point out very shortly the practical value of these clinical facts in the treatment of typhus:—1st. The fact that typhus produces an effect on the whole system, leads us to direct attention to the condition of every organ in the body. The muscular system becomes impaired; we therefore must take care to prevent exertion; we should confine the patient to bed; cause him at an early period to use the bed-pan and the urinal. Though he may be perfectly able to get up to perform these necessary evacuations, yet as the time may come when he will be unfit to do so, it is a good plan to accustom him to the use of these utensils whilst he has sense to guide him; and besides this, when care is taken to preserve the patient's strength in the early stages of the disease, the subsequent prostration will be modified.

2nd. The knowledge that the digestive system is impaired points out the necessity of great attention being devoted to the character of the food and drink, so as to give the patient aliment best calculated to undergo easy digestion. Again, the almost invariable implication of the mucous membrane of the small intestines in the disease, demands that every care should be taken to avoid the exhibition of any article of food or medicine which is likely to produce irritation of the alimentary canal. The knowledge of the existence of this intestinal lesion also guides us to the early employment of means calculated either to modify the extent of its development or remove its effect.

3rd. The fact that the brain is more or less affected points out to us the

necessity of taking steps to prevent all causes of excitement. The patient should not be annoyed with business matters; there should be no conversation allowed in the sick-room calculated to excite his mind; the head should be shaved and kept cool by cold applications; the bed-chamber should be kept quiet, and somewhat darkened. Those symptoms to which I have referred as indications of severe cerebral disturbance should be carefully attended to—such as sleeplessness, subsultus, cerebral breathing; because it is only at the earliest stages of these complications that we can hope to secure success from treatment.

4th. The knowledge that the respiratory system is also affected in fever, leads us to adopt measures to maintain a constant supply of pure air in the patient's apartment, avoiding all currents, and cautions us to make a careful examination of the chest at every visit.

5th. The fact that the heart becomes diseased in fever instructs us to make it a principal point in our attendance to ascertain frequently the state of its impulse and sounds, so as to be able to avail ourselves of this only safe key to the administration of stimulants.

6th. The evacuations must also receive a careful attention. The peculiar loose stool is most significant; and the important duty of attending to the state of the bladder must never be overlooked. These are points which should never be left to the nurse. The practitioner must always institute a personal examination.

But the clinical fact that fever affects the whole system is not of greater practical value than the other fact, that these different systems are neither equally nor uniformly disordered. The knowledge of this important circumstance leads us to examine every case with the greatest care, so as to be able to detect at the very earliest moment any tendency to the undue disturbance of any particular function or organ.

These facts instruct us to watch carefully, not only the peculiar manifestations in each individual attacked with fever, but also the tendencies which are exhibited in different epidemics to affect some organs more than others.

Whilst we know that, as a general law, the fever poison impairs the whole system, and that it produces its local manifestation on the mucous membrane of the small intestines, we also know that it attacks other tissues and organs secondarily, and that these secondary lesions are by no means either constant or of uniform intensity, but that at the same time they are frequently much more severe than the primary intestinal ones, and often prove more fatal.

It must follow from these considerations, that fever cannot be correctly treated by any *routine practice;* on the contrary, it is perfectly clear that whilst every attention must be given to the usual anatomical lesion in every case, yet each individual patient must be treated according to the character of his constitution, and the nature and extent of the various secondary complications to which I have directed attention in this series of contributions—papers which I regret to say abound with many imperfections. The only apology which I can plead is, that they have been written at considerable intervals, amidst the usual interruptions of professional life.

But whatever faults may be detected, I beg to remark that I have endeavoured to place before my readers the result of nearly twenty-five years' experience of a disease in which I have always taken a deep interest—a disease which I have had ample opportunities of studying under all circumstances, viz., in the rural district, in the manufacturing and mining village, in the overcrowded and densely populated city, in the private dwelling, and in the public hospital; in its sporadic forms, and in its epidemic outbreaks. —*Glasgow Medical Journal, April* 6, 1861, *p.* 57.

4.—*Fever in the Crimea.*—In a Treatise on Fever lately published, "it is shown by Dr. Lyons that considerably more than half of the whole deaths in that splendid army of over 300,000 men which France supplied in the late Russian war, was caused by fevers, or the diseases secondary to fevers;

whilst on the side of England, 10,000 men of the flower of the British army were carried off by the same class of affections in seven months during the first winter campaign. From the 1st of September, 1854, to April, 1855, 4,228 men perished from disease in the hospitals of Scutari alone, of whom but 359 died from wounds. Such, rather than balls and bayonets, are the true war plagues. The destroying angel of an army is fever, not gunpowder."—*Lancet, May* 21, 1861 *p.* 515.

5.—PATHOGNOMONIC SIGN OF SCARLATINA.

For some years past, M. Bouchut has been in the habit of pointing out in his wards a curious sign which assists in the discrimination of scarlatina from measles, erythema, erysipelas, &c. It consists in a vascular phenomenon, proportionate in intensity to the extreme contractibility of the capillaries: we refer to the enduring *white stripe* produced at will by drawing the back of the nail over the part of the skin in which the eruption exists. Pressure with the nail or any other hard substance upon the exanthematous surface produces a white streak which lasts one or two minutes, and sometimes more. Figures may thus be traced upon the skin, the lines of which are conspicuous from their whiteness. With a blunt probe or pen-holder, the diagnosis of the disease may be distinctly inscribed on the integument, and after a minute or two the word *scarlatina* disappears, when the uniform tinge of the eruption again invades the written surface.

This phenomenon is observable in scarlatina only. The scarlet hue of measles is not uniform, the eruption consisting of mottled patches, with very slight elevations separated by interstices of healthy skin. In measles the procedure we have described would produce an alternately red and white streak enduring a much shorter time than in scarlatina. In erysipelas, in the redness induced by a mustard poultice, in solar erythema, the white line we allude to is not visible; and without attributing to this sign an undue degree of importance, it may be said to supply one more element in the determination of the characters of the eruption of scarlatina.

Among the young patients in whom this symptom was noted this year, we observed one in whom scarlatina occasioned a delirium similar to that of meningitis, and who recovered, without any other prescription than mixtures with two ounces of syrup of mulberry. In this case, pressure with the finger left a distinct and lasting white line, especially on the second day of the eruption. The instances which place the fact beyond question may now be numbered by hundreds, and the phenomenon invariably appears when the eruption is complete. It is, moreover, not a little singular that when the exanthema has faded, and the skin has resumed its natural rosy hue, white lines may still be traced by friction with the finger, and last longer than on those parts of the skin to which the eruption did not extend.

M. Bouchut has sought for the cause of this phenomenon. How is it that in a cutaneous eruption, consequent upon universal capillary congestion, sudden and lasting discoloration can be induced by friction? He considers this to be due to a considerable increase of the contractile power of the capillaries, proportionate to the intensity of the disease, the regularity of the eruption, and the amount of vital power. The capillaries contract, and expel their contents; hence the *white stripe of scarlatina.* This excessive tonic contractility of the blood-vessels is further remarkable, inasmuch as it is peculiar to this disease; once brought into play by pressure, the vascular contraction seems to last one or two minutes—a circumstance which does not occur in any other morbid condition.—*Journal of Practical Medicine and Surgery.—Medical Circular, April* 24, 1861, *p.* 302.

6.—ON THE ACTION OF SESQUICARBONATE OF AMMONIA IN SCARLET FEVER.

By Dr. JOHN M'NAB, Bunessan, Oban.

[This remedy, no doubt, is very valuable in scarlet fever, often from the very commencement of the attack, not only given internally, but used as a gargle. In some severe cases Dr. M'Nab used it as follows:]

When given as a gargle, it was employed in the proportion of two drachms of the salt to six ounces of water—the throat to be gargled with the mixture every four hours. When ordered internally it was prescribed in five or ten-grain doses, according to the age and strength of the patient, and given three times a-day. This system of treatment, combined with the general therapeutical indications, was as a general rule attended with marked relief, particularly if the patient was of age sufficient to become impressed with the necessity of using the gargle, and provided the disease had not already involved the throat to such an extent as to interfere with deglutition, and produce organic change and disorganization.

The specific therapeutical effects of sesquicarbonate of ammonia in scarlet fever seem to depend upon its direct influence on the special blood-poison, from the presence of which in the system the disease arises; for, as scarlet fever is essentially the effect of a specific contagion on the system, and as the sesquicarbonate of ammonia changes or modifies the action of that blood-poison, so there is every reason to believe that the remedy acts specifically, by neutralizing the so-called materies morbi. But, apart from any specific influence it exerts, it seems to be of essential service in the treatment of the disease, in consequence of its diaphoretic and sudorific properties, and from the circumstance that it is a prompt and diffusible stimulant, especially of the nervous system, while it possesses the advantage of not accelerating the circulation. Anasarca, which is the most common and the most formidable of the sequelæ of scarlet fever, is not so frequently experienced in those cases where the sesquicarbonate is administered in the early stages of the disease, as where it has not been given at all, or given only when the disease has reached its acmé. This favorable result is probably due to the diaphoretic action of the remedy, which, when freely administered, relieves the kidneys of the undue action they would have to perform in eliminating the blood-poison through the urine. "But it need be no surprise at all that there are certain remedies found which operate on the human body with a power not yet explained or understood, since there are likewise so many diseases whose nature and causes are entirely unknown." Let us hope, however, that in the onward progress of scientific medicine, the action of remedies, their properties, and the cause of their efficiency in the cure of disease, will be better understood; and that disease itself, with its deleterious influence on the human race, will be stripped of its virulence, mitigated in its course, and governed by laws fixed and determinate as the periodic tides of the ocean.—*Edinburgh Medical Journal, October* 1861, *p.* 338.

7.—On the use of Benzoate of Ammonia on Renal Dropsy succeeding Scarlatina. By Dr. J. TAYLOR, Surrey-place, Old Kent-road.—It frequently happens that the sequelæ to scarlatina occasion more trouble to the physician than the original disorder, more especially in cases of anasarca with albuminous urine, the process of cure being often tedious, and not unfrequently unsatisfactory both to the patient and practitioner. If, therefore, any addition or improvement can be made by means of which such morbid conditions may become more amenable to the remedies employed, society will be benefited and the physician gratified. Having had many cases of the above nature during the past and the present year, I have prescribed the following draught (for a child six years old) with more than ordinary success. The remedy being not unpleasant, is readily taken by children:—Benzoate of

ammonia, five grains; spirit of nitrous ether, fifteen minims; syrup of tolu. one drachm; camphor mixture to one ounce; to be taken three times a-day. This, with a purge of the compound jalap powder, has answered admirably, the urine becoming free from albumen in a much shorter time than usual, and the patient's strength being afterwards recruited by the ammonia-citrate of iron.—*Lancet, Oct.* 12, 1861, *p.* 361.

8.—*On Arsenious Acid as a substitute for Quinine in Fever.* By J. TURNER, Esq., Surgeon to H. M. Brigade, Bombay Horse Artillery.—Mr. Turner has employed arsenious acid for twenty years in the treatment of intermittent fevers, and on account of the great drain upon the cinchona tree, its failure in India, and his strong opinion as to the equal if not greater value of arsenious acid in the above-named diseases, he now brings the results of his experience before the profession. He considers the fears of an inconvenience or danger arising from the remedy as much exaggerated, and instances the case of a child of nine months, to whom he gave twenty minims of the arsenite of potash within ten hours, repeating the dose on the following day, with the only effect of curing an obstinate quotidian intermittent. Mr. Turner's success was so marked, that in 1860 the Director-General stated that Mr. Turner should be thanked for "drawing attention to his successful treatment of intermittent fevers by large doses of arsenic, and steps should be taken by circular to urge an extended trial of this remedy, and reports requested." The course usually adopted by the author was to give the arsenite of potash as in the following prescription:—Arsenite of potash and compound tincture of cardamoms, of each half a drachm; gum mucilage, three drachms; camphor mixture or water, half an ounce: mix. To be given every second hour four or five times, the last to anticipate the expected paroxysm at least two hours.—*Lancet, Aug.* 3, 1861, *p.* 112.

9.—ON ACTEA RACEMOSA IN ACUTE RHEUMATISM.

By Dr. FRANCIS R. MAC DONALD, Inverary.

[Dr. Mac Donald thinks that this medicine is superior to all others in this tiresome disease. He says:]

This remedy is the tincture of the *Actea racemosa*, an American plant, (natural order, *Ranunculaceæ*). It was first brought under my notice by an accomplished physician, Dr. Voris, of New Rochelle, near New York, who was living for some time here two years ago. Dr. Voris spoke so strongly of the good effects of the Actea in rheumatism, that I immediately procured a supply. I have used it since in all the cases that have come under my notice. I was at first disposed to ascribe its beneficial effects less to its own virtues than to that coincidence of favourable circumstances which has frequently been observed to attend the use of a new medicine. Its uniform success has induced me to alter this opinion; and I believe that a similar success will be found to attend its employment in the vast majority of cases of acute rheumatism.

I have employed the Actea with equal benefit in all the acute forms of rheumatism. In the subacute variety already alluded to, it acts with remarkable readiness. There are not a few persons to be met with, either of a favouring diathesis, or living in rainy districts, who, in consequence of such an exposure as would elsewhere or otherwise lead to no greater inconvenience than a slight cold, are suddenly seized with rheumatic pains. In these cases the Actea is alike expeditious and satisfactory. But it is in the severest forms that its beneficial effects are best seen. Two well-developed cases came under my observation at the same time in last spring—one in a boy of nine, and the other in a man of forty. My supply of the Actea being exhausted, I was obliged to have recourse to the ancient treatment. In neither

case did the disorder yield until the Actea had been given. The dose that I give varies from 25 to 40 drops, administered three times a-day.

The tincture of the Actea racemosa may be had of Messrs. Duncan and Flockhart, of Edinburgh, at 6d. per ounce.—*Edinburgh Monthly Journal, Oct.* 1861, *p.* 335.

10.—CASE OF ACUTE RHEUMATISM, WITH UNUSUAL COMPLICATION.

By Prof. Reid.

[At a meeting of the Belfast Clinical and Pathological Society, Prof. Reid related the following case:]

A young gentleman, aged 16, enjoying a comfortable home, was attacked for the second time with acute rheumatism. It began in the left ancle, and successively involved the knees, the elbows, and the wrists. A careful examination of the heart did not detect any evidence of disease as the result of the attack he had suffered from six years previously.

The treatment adopted at first consisted of alkaline aperients, alkaline anodyne lotions, a combination of calomel, squills and opium; and enveloping some of the joints in cotton wool. For the first four days his bowels were very difficult to act on, so that he took a very large amount of Rochelle salts without their producing anything like a proportionate aperient effect I thought afterwards that this might have had some influence in producing a complication I had never met with before in acute rheumatism, and which has caused me to think the case worthy of record.

On the seventh day of his illness there appeared an ecchymosed patch or blotch, of the size of the hand, on the outer side of the right leg, not raised in the skin, nor removable by pressure; this was soon followed by similar patches on the arms, and by redness and great swelling of the left eye-lid, with effusion of blood beneath the conjunctiva. The right eye was soon involved in the same way; then the forehead and scalp swelled up into tumors the size of half an egg, producing, with his swollen face, a most disagreeable appearance.

The gums had become slightly swollen and coated from the mercury; on the day that the first discoloration was observed on the leg, the mercurial was at once discontinued, and an opiate and tonic treatment adopted. Salivation did not follow, nor did the gums bleed at any time. There was no cough, nor oppression in the chest. The stomach now became very irritable, rejecting everything, and I became alarmed as to the result, and requested Professor Ferguson to see him.

He, like myself, had never met with a similar complication in acute rheumatism.

He suggested ten minims of diluted sulphuric acid every two hours to relieve the vomiting, which it did immediately, after the failure of full doses of opium, ether, and the aromatic sp. of ammonia. Tonics, opiates, and nourishment were continued.

At the end of four days the swelling of the face and forehead began to subside, and the blood effused under the conjunctiva was slowly absorbed, and the discoloration of the skin gradually disappeared, as well as the affection of the joints. That these peculiar symptoms resulted from some change in the blood, permitting its transudation through the vessels, does not, I think, admit of any doubt. It then struck me, that possibly the alkaline solution, which had been taken so freely during the first few days of his disease, and which evidently had remained in the system, as it did not produce its usual aperient effect, was the cause of this altered condition of the blood, it being known that alkalies produce a more ready solubility of the fibrine, which, becoming more fluid and less coagulable, permits the blood to transude.

Believing that there is generally an excess of acid in the system in acute rheumatism, I have for many years past invariably administered alkaline

aperients freely in the treatment of this disease, without their being followed by any such scorbutic symptoms as appeared in this case; but, then, I had never known three or four ounces of Rochelle salts to be taken daily for several days without producing free and continuous purgation.

It is, therefore, possible that the introduction and retention in the system of so much alkali might have injured the quality of the blood. For a few days after convalescence had commenced the evacuations from the bowels were slimy and streaked with blood, but the latter never appeared in such a quantity as to indicate intestinal hemorrhage.

I was told, at my first visit, that his medical attendant in the former attack of acute rheumatism had stated that there was no heart complication. As I saw him within twelve hours after the commencement of the present one, I carefully examined the state of the heart, and found no evidence of present or past cardiac disease. Nor did a daily examination detect any till the morning of the fifth day, when a feeble murmur was heard at the apex, which continued feeble till the morning of the eleventh day of his rheumatism, when it was found to have become much louder.

For the first six days of the cardiac complication (during the two latter of which I had the advantage of Dr. Ferguson's opinion), no murmur could be heard through the solid whalebone stethoscope, though it was heard distinctly through the ordinary tubular one. On the seventh day it had become so loud that it could be heard through both, and continued to be so for five or six weeks, when it gradually subsided under the influence of blistering and free doses of hydriodate of potash.

The heart's action was easily excited for several months afterwards, and its impulse, on exertion or running, was so strong that it was often complained of.

This, however, gradually passed off, and a careful examination of the heart, made at the expiration of twelve months, from this last attack, failed to detect any evidence, or even suspicion, of disease, either at the valves or of the substance of the heart.

This young gentleman had scarcely become convalescent before his younger brother was attacked also with acute rheumatism for the first time.

He was treated, at the commencement, with alkalies and moderate doses of mercury, care being taken that free purgation should take place.

He also suffered from a cardiac complication, which pursued much the same course as that in his brother, and left no permanent injury in the heart.

Fortunately, he was not attacked with scorbutic discoloration of the skin, nor with the swelling of the face or irritability of the stomach which had excited so much uneasiness and alarm during the treatment of his brother. His brother's complication should not, however, cause us to abandon the administration of alkalies during the treatment of acute rheumatism, but only inculcates the necessity of our insuring their non-retention in too large amount in the system by a combination of purgatives with them. Indeed the recent experiments of Dr. Richardson, which proved that he could at will produce inflammation in the interior of the heart, by the injection of lactic acid into the peritoneal cavities of dogs, placed the alkaline treatment of acute rheumatism on a much more legitimate basis, by giving additional weight to the inference, that the cause of the disease and its cardiac complications was an excess of acid in the system.—*Dublin Hospital Gazette, June* 15, 1861, *p.* 177.

11.—*Medicinal Treatment of Gout.*—The combination which M. Trousseau conceives to be the most efficient is that of sulphate of quinine, colchicum, and digitalis, proposed by M. Becquerel in the following proportions:—Quinæ sulphatis, 22 grains; ext. semin. colchici, 8 grains; ext. digitalis, 4 grains. M. Divide in pilulas decem. Two or three of these pills should be exhibited in the course of the twenty-four hours, for two, three, or four successive days. M. Trousseau has prescribed these pills himself and witnessed their exhibition by others, with sometimes wonderful success. He has found the

excruciating pain of a genuine acute paroxysm yield in seven or eight hours, and the attack itself subside in two or three days. These are the pills the professor prescribes in the incipient stage of anomalous gout.—*Dublin Hospital Gazette, June* 1, 1861, *p.* 175.

DISEASES OF THE NERVOUS SYSTEM.

12.—ON ACUTE SOFTENING OF THE MEDULLA SPINALIS.

By Dr. ROBERT MAYNE, Physician to the Adelaide Hospital.

One of the most remarkable circumstances connected with the history of tumours of the brain, is the fact, that they are very frequently latent in the early stages. Many cases of chronic tumours of the brain have been presented to the Pathological Society which attained considerable dimensions, without having caused, during their early growth, any serious disturbance of the patient's general health, or having given rise to any symptoms calculated to create a suspicion of their existence.

It seems to be a law, in the development of these tumours, that if the growth be slow, and the increase gradual, so as to allow the surrounding brain to accommodate itself to the presence and pressure of the tumour, such tumour may arrive at a large size without creating anything like special symptoms. At length, however, when the tumour has reached a certain bulk, the surrounding brain becomes softened, or inflamed, or congested, or compressed, and when we find symptoms of cerebral disease setting in with great severity, and running a rapid course, symptoms which are, however, referable rather to the condition of the brain than to that of the tumour.

For what reason it may be I cannot very well say, but the history of tumours in the *spinal marrow* is not as well known as the history of tumours in the brain. There can be no doubt that the former class of tumours is infinitely more rare than the latter, and this, added to the difficulty which the pathologist always experiences, in removing the spinal cord for examination, may probably account for our imperfect knowledge of such cases.

The following case is one of some interest, illustrating, as it does, many circumstances connected with the pathology of the spinal marrow.

A. B., twenty-one years of age, had always enjoyed good health previous to his last illness, with the exception of one brief rheumatic attack in his eighteenth year. He had been at work constantly and regularly up to the period of his late attack. According to his own account, he was returning home after a hard day's work on the evening of the 10th of December last, when "all at once" he was seized with an acute pain "shooting down the left thigh and leg, and towards the ankle and foot." He was compelled to stop abruptly in the street, feeling unable to proceed; a short time after, he made an effort to walk on and he then experienced a similar "darting pain" throughout the entire length of the right limb, so that he could scarcely move. With much difficulty he managed to reach his home and get to bed, when he experienced a severe *rigor*. Subsequently he fell asleep and obtained some rest, but on awakening in the morning he was paralyzed in both lower limbs. The bladder was paralyzed from the commencement. He was received into St. Vincent's Hospital on the 12th of December, and placed under the care of Dr. Quinlan, who has obligingly furnished me with an abstract of his notes of the case whilst it remained under his care. All power of voluntary motion in the lower extremities was gone, and there seemed to be complete destruction of the excito-motory phenomena as well. The tactile sensibility of the paralyzed limbs was much impaired, but not quite destroyed. There was complete retention of urine. Doctor Quinlan considered the case to be one of acute softening of the spinal marrow, and, indeed, all the phenomena of the case seemed to confirm this opinion. The integuments over the sacral region soon became livid and sloughed, and the

urine quickly became ammoniacal. By degrees the paralysis extended upwards, and at length the left upper extremity lost its power of motion, in great measure, and its tactile sensibility partially. All that medical skill could devise was done for the patient's relief, but with little avail. After being about a month in hospital, sloughing of the integuments set in wherever the body was subject to the least pressure. At about this period, the friends of the patient removed him from St. Vincent's Hospital, and shortly afterwards I found him in the hospital of the South Dublin Union. It is needless to recount the symptoms which he then presented, as they were the ordinary symptoms of complete paraplegia. There was fearful sloughing of the integuments over the sacral region and hips. The trochanters at each side, and the osseous structure of the sacrum, were laid as bare as if they had been dissected, yet in the midst of this frightful destruction of parts, with this process of death going on in the living body, the patient seemed not to suffer any acute pain, and appeared perfectly unconscious of his terrible condition. All power being lost over the bowels and bladder, the fæces passed involuntarily and insensibly; an insufferable odour pervaded the atmosphere around him; the left upper extremity was completely paralyzed, the right was partially so; and altogether I have seldom witnessed a more distressing case. The man died on the 12th of February.

There was considerable difficulty experienced in obtaining a *post-mortem* examination, and, therefore, the state of the brain was not ascertained.

On laying open the vertebral canal, the "*theca vertebralis*" seemed healthy at the exterior, its inner surface appeared congested, and of a pinkish hue. It was only when the "pia mater" was slit up, that we discovered the diseased condition of the "*medulla spinalis*," and the extent to which the disorganization had reached. The diseased portion extended throughout the entire dorsal region, and was characterized by a sort of inflammatory *ramollissement*, reducing the ordinary firm texture of the medulla to the consistence and similitude of whipped cream, inflammatory action having tinged it of a red colour. But the most striking pathological phenomenon presented in the diseased parts, was a tumour of about the size of a small kidney bean, and oblong in shape, embedded in the substance of the "*medulla oblongata.*" When carefully examined it felt hard and firm to the touch, and *it cut like a piece of cartilage.* It was embedded in the left lateral half of the medulla oblongata. It seems strange that so dense a tumour could have grown in a portion of the nervous system so directly essential to life—a part in which the slightest hemorrhage causes immediately fatal results, and in which the slightest injury is mortal. The slow and gradual development of the tumour would, perhaps, account for this. We must, I think, believe, that the existence and growth of this tumour were the cause of the ramollissement; but if so, was it not strange that the softening of the spinal marrow began from below and extended upwards, thus producing the long protracted series of morbid results which terminated in the death of the patient?—*Dub. Hospital Gazette, May* 15, 1861, *p.* 145.

13.—ON REFLEX PARAPLEGIA.

By the Editor of the British Medical Journal.

Perhaps one of the earliest and ablest writers on reflex paraplegia is Dr. Graves, in his lectures on paraplegia (1843).]

Dr. Brown-Séquard is, however, the latest writer on the subject. He gives us an excellent account of the affection, and also a novel explanation of its nature—an explanation, however, which we fear must, for the present, remain hypothetical. Let us see what it is that he tells us on this important subject.

The two most frequently observed and clearly defined forms of paraplegia are, reflex paraplegia, and paralysis depending upon myelitis or disease of the spinal cord. Heretofore there has been no proper line of distinction drawn by pathologists between these two varieties of paralysis; but this distinction is absolutely necessary for the important purposes of rightly

directing the practitioner's treatment; medicines which are suitable in the one kind being injurious in the other.

Paraplegia depending upon disease or injury of the spinal cord has been long well understood by us all; but what is meant by *reflex paraplegia?* Is there really such a disease? By reflex paraplegia is meant that kind of paralysis of the lower limbs which results from an excitation of the spinal cord, the excitation being conveyed to the spine through a sensory nerve. For example, some part of the body, say the uterus, is irritated, injured, or displaced; the effects of the displacement, &c., are appreciated by sensory nerves, and are thereby conveyed to the spinal nervous centre, and, by the action which they there produce, cause those changes to occur which result in paraplegia. According to Dr. Brown-Séquard, the paralysis is produced in the following way. The excitation aforesaid acts either upon the blood-vessels of the spinal cord itself, or on those of the motor nerves proceeding from it, or upon those of the muscle. This is what, pathologically speaking, is meant by reflex paraplegia. Through this excitation, spasmodic contraction of the blood-vessels is produced, and consequently a deficient supply of blood is sent to one or other of the aforesaid parts.

Dr. Brown-Séquard has, however, first of all, before proceeding to speak of its treatment, &c., to answer the objections of those who deny the existence of the affection; and there are a few men of high repute who do deny the thing. The answer of Dr. Brown-Séquard is, on the face of it, the best which can be given. He leaves transcendental physiology, and asks his readers to look at a plain series of facts which he unrolls before their eyes, and which are comprehensible to the understanding of the youngest and the oldest of us. To us, we must confess, these facts demonstrative are as "plenty as blackberries;" and we cannot imagine that any one except a *tête-monté* physiologist, or an absolute specialist can refuse to accept the consequences which flow from them. A female suffers from paraplegia. Her uterus is displaced. The abnormal position of it is rectified, and the paralysis disappears. Amaurosis exists in combination with irritation of the frontal nerve; the frontal nerve is divided, reflex action from the irritated spot is cut off, and sight rapidly returns. Neuralgia of external parts exists: and the patient's auditory powers are good, bad, and indifferent, according to the absence, presence, and acuity of the neuralgia. Dentition, worms, irritation of the womb, diphtheritis, &c., are at times associated with various kinds of partial paralysis; and *sublatâ causâ tollitur effectus* has been duly observed in such cases.

Now, the proof that these paralyses do not depend upon disease of the nervous centres is this very plain one; viz., that the removal of the peripheric irritation—the worms, the displaced womb, &c.—could not in such case have removed the paralysis. And if a very hardened sceptic in these matters should still object that, nevertheless, the cures in such cases might have been more properly called rather a coincidence than a consequence, we should advise him to consult Dr. Brown-Séquard's book, and study his witnesses. The collection of cases there to be found, derived from numerous different sources, will, we fancy, demonstrate to the satisfaction of every one —except those who, apparently for argument's sake, will on occasion deny that B follows A in the alphabet—that *reflex paralysis* is a medical fact.

In these cases it will be seen, that what Dr. Brown-Séquard calls the *outside irritation* preceded the paraplegia, and that cure of the latter quickly followed the removal of the irritation. Again, in many of these cases, the paralysis increases and decreases with the condition of acuity of the outside irritation; and no treatment has any effect in removing the paralysis so long as the irritation was left unremoved. In some of the cases the paraplegia appeared, disappeared, and reappeared several times over with the appearance, disappearance, and reappearance of the outside irritation. "Can there," says Dr. Brown-Séquard, "be more decisive proofs that it is the outside irritation starting from some sensitive nerves in various parts of the body, which produces the paraplegia?"

Besides, if we do not accept this view of the case, we are left without any

means of explanation of the paralysis in most of these cases. The idea of these kinds of paralysis being produced by, for instance, pressure of the uterus on the obturator nerves or on the sacral plexus; or by the action of poisonous matters accumulating in the blood in retention of urine, or in diphtheritis, or enteritis, is very chimerical.

But, it is objected, we cannot accept this reflex paraplegia; for it is impossible to understand how it can occur. And here the physiologist steps in, and answers without hesitation—*les pieces à la main*—that, on the contrary, it is very easy to show how a paralysis can take place by a reflex action starting from a sensitive nerve. And practitioners who pride themselves on being practical men, and *not* physiological physicians, must, we suppose, here take a lesson from the physiologist.

An outside irritation may produce paraplegia by *causing reflex contraction of blood-vessels* in two ways. It is proved beyond doubt, that blood-vessels have similar relations to the nervous system, to those which muscles of animal life have to this system. Blood-vessels contract with energy and sometimes pass into a state of prolonged spasm: 1. when their motor nerves are *directly* excited; and 2. when excited through the cerebro-spinal axis through irritation applied to some centripetal or excito-motor nerve. In *these contractions of blood-vessels* we find the cause of paralysis. The contractions may occur: 1. In the blood-vessels of the spinal cord; 2. In the vessels of the motor nerve passing from the cord; and 3. In the vessels of the muscles themselves.

Dr. Brown-Séquard has, he tells us, seen contraction of the blood-vessels of the pia mater take place in the spinal cord, when the lobes of the kidney, or the vessels and nerves of the suprarenal capsules, were ligatured; and the contraction is generally greatest on that side of the cord corresponding with the side irritated. Paralysis of the lower limb has been known to follow extirpation of the kidney on the same side of the body. Again, reflex paraplegia may be produced by *insufficiency of nutrition*, resulting from this contraction of the blood-vessels. This explanation of the paraplegia as being a consequence of contraction of blood-vessels, must, however, at present be received as theoretical.

Another argument in favor of this view of the case is derived from the fact of the normal condition of the spinal cord—the absence of all visible signs of disease—observed in persons who have died after presenting symptoms of reflex paraplegia; as in cases of paraplegia associated with retention of urine.

Thus, then, here stand the facts touching Reflex Paraplegia: 1. There is no organic disease of spinal cord; 2. The paralysis will rapidly disappear when the source of reflex irritation is cut off; and 3. The vascular supply of cord, of motor nerves, and of muscles is under the influence of reflex action.

And now, then, we see the grand anatomical distinction between reflex paralysis, and paralysis associated with myelitis. In the first case, there is diminished, in the second, increased vascularity of the cord. In myelitis, the colour, the consistency, and the vascularity of the cord, are visibly changed. In reflex paraplegia, these qualities of the cord are found, to all appearance, unaltered after death. And here, therefore, as Dr. Brown-Séquard tells us, we have at once before us two groups of paraplegic diseases —one in which too much blood is circulating in the spinal cord, and one in which the very opposite condition of the cord exists. And then, he adds, consider the value of a right discrimination between them. The remedies which suit one group are actually injurious to the other. There are some, and he says that he can prove the fact, that diminish the amount of blood in the spinal cord; and there are others that increase the quantity. And yet, these remedies are often blindly and indiscriminately employed in paraplegia. Mercury, ergot of rye, and belladonna, Dr. Brown-Séquard asserts, diminish the blood, and strychnine and brucia increase the blood in the spinal cord.

[In most cases, reflex paraplegia may be characterised as follows:]

There is an absence of the special symptoms which indicate organic disease of the spine; the paralysis is incomplete, and has slowly supervened upon an affection of an urinary, a genital, or some abdominal organ, upon inflammation of the lungs or pleura, or after irritation of a nerve in its trunk or cutaneous ramifications. In most cases of paraplegia, we must form our judgment as to its nature from a consideration of these conditions.

[The following are the distinctions which Dr. Brown-Séquard has collected together and given as characteristic of the two best marked varieties of reflex and centric paraplegia—urinary paraplegia and myelitic paraplegia.]

Urinary paraplegia is *preceded* by an affection of the urinary organs. The paralysis is incomplete, and varies in degree with the varying conditions of these organs. The lower limbs alone are usually affected, and the paralysis does not extend upwards. Reflex power is not wholly lost or is much exalted. Rarely do spasms seize the paralyzed muscles; nor is there pain in the spine, nor sense of constriction of the abdomen; neither formication, nor pricking, nor anæsthesia. Rarely is the bladder or rectum paralyzed; there is much gastric disturbance; and lastly, recovery often takes place rapidly, after notable amelioration of the urinary affection.

On the other hand, in *myelitic paraplegia*, the affection of the urinary organs does not *precede*, but is a *consequence* of, the spinal disease. The paralysis is complete, and is unaffected by changes in the condition of the urinary organs. Other parts besides the lower limbs are usually paralyzed, and paralysis gradually extends upwards. Reflex power is often lost, but sometimes much increased. There are always spasms and some amount of pain; also formication, pricking, &c.; feeling of abdominal constriction, and almost always anæsthesia or numbness. There is no gastric disturbance. The disease usually slowly progresses to a fatal termination, and rarely ever is there a complete recovery. Moreover, the urine in myelitis is alkaline; whilst in reflex paraplegia not depending upon urinary diseases it is usually natural.

In considering the *prognosis* of reflex paraplegia, we must, according to Dr. Brown-Séquard, reflect that there are two forms of the affection; that, in one of these forms, the paraplegia depends upon a defect in the nutrition of the spinal cord; and in the other upon an alteration of nutrition in the muscles of the lower limbs. The first of these forms is almost always curable, provided the external irritating cause be removable. But, in the second case, recovery rarely takes place, even though the provoking cause of the paraplegia be removed; and the reason is, that in this form the muscles have become rapidly atrophied and altered. Hence as a matter of prognosis, it is always important to know, in a case of reflex paraplegia, whether or not rapid atrophy of the muscles has occurred.

The *treatment*—or, to speak more correctly, the indications for treatment, of reflex paraplegia springs naturally from the pathology of the disease as here laid down. The first indication evidently is to remove the local irritation, the cause of the affection; and this necessarily involves the consideration of the treatment of nephritis, cystitis, enteritis, morbid states of the uterus, in fact, of all those morbid conditions with which the paraplegia may be associated. In carrying out this treatment, we must also endeavor to diminish the sensibility, or interrupt the nervous influence which passes from the diseased organ or nerve to the spinal cord, to paralyse the nervous sensation. For this purpose, says Dr. Brown-Séquard, no narcotising agent is more powerful than belladonna; but, in some cases, caution is required in its use. In disease of the urethra, or prostate, one grain of the extract of belladonna in twenty drops of laudanum should be injected into the urethra, and washed away after half-an-hour or an hour's residence there. This treatment should be repeated every two or three days; thirty drops of laudanum alone being injected on the intervening days. In bladder-disease, the aforesaid solution of belladonna is to be injected after evacuation of the urine; and then the sole laudanum injection on alternate days. Suppositories of these two agents may also be employed.

If the irritation radiate from the vagina or uterus, then a pill of half a grain of belladonna, with one grain of extract of opium, enclosed in a piece of cotton wool, may be passed well up into the vagina—a practice of M. Trosseau's, which Dr. Brown-Séquard much extols. Should the stomach, or the intestines, or kidney, &c., be the seat of local irritation, then opium should be taken by the mouth.

Dr. Graves has strongly recommended excitants or revulsives to be applied to the skin of the legs; and Dr. Brown-Séquard gives a theoretical explanation of their beneficial mode of action. They produce a *similia similibus* effect. We have seen that the disease, according to Dr. Brown-Séquard, results from a contraction of the smaller blood-vessels; but if this contraction be considerable, the muscular fibres of the vessels become exhausted, and then (by a well-established physiological law) they are relaxed, and so there follow dilatation of blood-vessels and admission of due flow of blood to the spine. Now, no irritating cause is more powerful in effecting contraction of blood-vessels by reflex action than cold; and, consequently, this sort of excitation, alternated with heat, should be applied to the spine. But, whatever be the kind of excitation employed in this case, it ought to be very powerful, in order to produce that exceeding degree of contractility of muscular fibres which is found, experimentally, to be followed by exhaustion and dilatation.

We fancy that to the theoretical explanation given by Dr. Brown-Séquard (for we must still call it such) of the cause of reflex paraplegia, our readers will not at present be prepared to give their unqualified adhesion. To us, we must say that a tonic permanent spasm of the blood-vessels, such as is supposed by him to be the permanent state of the blood-vessels of some of the nerves or of the spinal cord in reflex paraplegia, is quite incomprehensible; and we think that on this point, which is assuredly the great point of Dr. Séquard's labours in this direction, he would have done well to have more fully developed the basis of his conclusion. Surely it is not enough to satisfy us on this head, that Dr. Brown-Séquard *has seen* a contraction of the blood-vessels in the spinal cord when "a tightened ligature was placed around the hilus of a kidney." *Permanent* contractions of blood-vessels of this character are surely contrary to what experience teaches us in other cases; and, if they really exist, how comes it that the cord does not become organically affected through such a cutting off of its nutritive supply?

Keeping this idea of the cause of the paraplegia in view, Dr. Brown-Séquard recommends that the patient, during the night, and, as far as may be, during the day, should lie in that position which would favour (by gravity) the flow of blood to the spine. As regards general treatment, this should consist in tonics and generous food; the partially paralysed muscles being as much exercised as possible in the open air.

Strychnia, Dr. Brown-Séquard says, is of the greatest use in this affection. It is the only remedy which really deserves confidence. It increases the amount of blood in the spinal cord, and (acting in a special and direct manner on the tissue of the cord) it increases the reflex faculty; the amount of blood and of the reflex faculty being, as propounded, below par in these cases. It may be given with opium in doses of one-thirtieth to one-fortieth of a grain a-day, and, when used alone, in daily doses of one-twentieth of a grain. When employed with belladonna its dose must be larger, to compete with the antagonistic action of the belladonna. And hot and ice-cold douches may be applied to the spine alternately to produce that violent degree of spasm of the blood-vessels which is followed by their relaxation.

Besides this, the muscles of the paralysed limbs should be duly exercised, to prevent their becoming wasted and atrophied. It is therefore important to have them galvanised two or three times a week, and the limbs properly shampooed. If necessary, also, the heat of the limbs must be artificially kept up: remembering always that, unless the *primum mobile* of the paralysis—the local irritation—be removed, we can expect no permanent cure. —*British Medical Journal, April 27 and May 25, 1861, pp. 457, 553.*

14.—SOME REMARKS ON PARALYSIS.

By Dr. C. E. Brown-Sequard, F.R.S., Physician to the National Hospital for the Paralysed and Epileptic, &c.

[Paralysis is often caused by the irritation of a centripetal nerve, and yet this cause is not often noticed.]

The eye, which is the principal theatre of reflex action in the human body, is surrounded by muscles which are not unfrequently paralysed by a reflex action. In cases of neuralgia of the face, even when caused by a wound, a paralysis of the whole or of a branch of the third pair of nerves is often observed. This paralytic affection is generally easily cured when the neuralgia is cured.

The arms may be paralyzed by a reflex action from various sources. In one case, after a sprain of the left elbow-joint, the whole of that arm from the shoulder to the elbow became paralysed, and a few days afterwards the right arm was also attacked with paralysis, and to a greater degree than the left. There was no other symptom of disease of the nervous system; nor was there any appearance of a rheumatic affection. For several months a variable degree of pain remained in the left elbow-joint, and many times during that period it was ascertained that the degree of paralysis was in correspondence with the degree of pain; and when the pain ceased altogether, the paralysis was soon completely cured. It will easily be admitted that I observed this case with interest and care, as I myself was the subject of the observation.

In the above case there was paralysis without wasting; it is not rare to find wasting accompany the paralysis when its origin is in some irritation of centripetal nerves. As regards the lower limbs, I have related elsewhere several such cases; as regards the arms, I have seen three cases in which an irritation from a wound on the fore-arm produced a reflex wasting palsy, either in the same arm (in parts the nerves of which had not been wounded), or in the other arm. I showed some time ago one of these cases at the London Medical Society, and Mr. W. Adams then said that he had seen a similar case.

The upper as well as the lower limbs, and other parts of the body, may be paralysed in consequence of an irritation of the bowels by worms. Moll, of Vienna, relates a case of paralysis of the two upper extremities, which had lasted three months, when it was suddenly cured after the expulsion of a very long tænia. In a case reported by Dr. Calvert Holland there were anæsthesia and paralysis of the lower extremities, and partially, also, of the upper extremities. No good effect was obtained from calomel, bleeding, and purging; while cure took place in two days after the expulsion of lumbrici.

Hemiplegia, also, is often caused by an external irritation. It is not rare in children, in consequence of an irritation of the dental nerves or of the bowels. It is not rare, also, in women at the time of parturition, or in other cases of irritation of the womb. Mangon, quoted by Davaine, reports a very important case of a boy, nine years old, who was attacked with loss of consciousness, syncope, vomiting, and convulsions, and who, on recovering consciousness, was paralysed on the right side of the body. In a few days more than seventy lumbrici were expelled, and the patient gradually became better, and was cured of his paralysis in twelve days.

A paralysis of the face is often caused by an external irritation, such as a draught of cold air or a neuralgia. In a housemaid, who had frequent attacks of neuralgic pains in the infra-orbitalis nerve, I found a paralysis of all the muscles, and anæsthesia on the same side of the face during the paroxysms and for some time afterwards. Notta mentions several cases of facial paralysis evidently caused by a neuralgia.

In cases of paralysis it is often a difficult question to decide which of the two following causes is the one that has produced the paralysis; either an alteration of the blood, or a reflex action. In the many cases of paralysis

appearing during pregnancy or parturition, or some time after delivery, collected by Dr. F. Churchill, the most varied forms of paralysis were observed; but it is difficult to know whether they were due—1st, to an alteration of the blood, in consequence of anæmia, chlorosis, albuminuria, or uræmia; 2ndly, to a reflex action from the womb or vagina; or 3rdly, to a combination of these two causes. The same difficulty exists in cases of paralysis following diphtheria, typhus, rheumatism, gout, fever and ague, measles, scarlet fever, small-pox, dysentery, and diseases of the liver or the kidneys. I think that in most of these cases the two causes co-exist, the alteration of the blood producing the general debility, and the reflex action the greater local debility, which amounts to a paralysis. We generally find in those cases that the paralysis is in the neighbourhood of some part that has been the seat of pain—i. e., of an irritation of centripetal nerves, which is a fact in harmony with the view that it is at least partly caused by a reflex action. I may add that in cases of visceral inflammation, without any evident alteration of the blood, a paralysis may also be produced apparently by a reflex action only; so it is in cases of paralysis occurring during or after a gastro-intestinal inflammation, a bronchitis, a simple sore throat, a pleurisy, a pneumonia, &c., when there is no reason whatever to suspect that some organic lesion exists in the nervous centres.

It is not my intention to explain by what mechanism a reflex paralysis is produced. My only object is to show that the most varied forms of paralysis may be due to an external irritation of some centripetal nerve.

Loss of Speech.—This symptom may be due to the most distinct causes; 1st, some peculiar affection of the mind; 2nd, a defect in the memory of words; 3rd, a paralysis of the muscles of the larynx; 4th, a paralysis of the tongue. Leaving aside the first two causes, we find that a paralysis of either the larynx or the tongue may be due, like any other local paralysis, to some organic affection of the nervous centres or motor nerves, to some organic affection of the muscles, to an alteration of the blood, or to a reflex action. Loss of speech is a symptom induced more frequently than any other by a sympathetic influence from the cerebral lobes upon the basis of the brain.

Delirium and Insanity.—No one who has carefully studied these effects of certain disturbances of the nutrition of the brain can deny that the pretended organic causes of mental disorders are not their essential cause. It is impossible to admit that an alteration limited to a small part of the brain can be the cause of all the shades and degrees of delirium and of the numerous forms of insanity, and it is therefore necessary to conclude that when these various mental disorders arise from a limited organic affection of the brain, they are due to some peculiar influence exerted at a distance from the seat of the organic alteration upon other parts of the brain. As regards the production of insanity by a reflex action, facts are so abundant that it is scarcely requisite to mention any of them, as every one must have seen some. In the case of a gentleman brought to me by Mr. Foulkes, in December last, there was insanity with general paralysis, which I suspected to be caused by an inflammation of the prepuce and of the glans, with phymosis. According to my advice Mr. Foulkes performed circumcision, and the patient was soon cured of insanity. Shortly afterwards he improved rapidly as regards the paralysis, and he is now cured. This is a clear case of reflex insanity and reflex paralysis.

That the various forms of insanity and delirium may be caused by irritations of centripetal nerves, or by alterations of the blood, we have positive proofs every day in cases of worms or of any irritation of mucous membranes or of the skin, of acute inflammation of the thoracic or abdominal viscera, of chlorosis, of anæmia, of albuminuria, of poisoning by lead, arsenic, &c. As there cannot be the least doubt in this respect, I will not attempt to give a demonstration.

Coma.—This grave symptom is generally due to a pressure upon the brain by effused fluids (pus, blood, serosity). I need not say that hemorrhages, inflammations, and pressure upon sinuses or veins are frequent causes of

the effusion to which coma is generally due. My object is simply to state that effusions producing coma are very often caused by irritations upon centripetal nerve-fibres in the different parts of the body, acting upon the membranes of the brain, in such a way as to produce a more or less acute inflammatory state, and the consequent purulent or serous effusions. In children, indeed, such a thing is extremely frequent; it is not rare also in adults in cases of fever, especially with disease of the abdominal or thoracic viscera. Coma is then the effect of a reflex action. It seems probable that an irritation from certain parts of the cerebral lobes does sometimes act in the same way as the peripheric irritations or visceral nerves of the thorax and abdomen. Anæmia and several other affections with alteration of the blood, are sometimes also the causes of effusions producing coma.

Contraction and Rigidity.—These symptoms are so well known as being sometimes the effects of a reflex action (caused by worms, teething, &c.), that I have not to insist on that mode of their origin. I wish now only to say that if we admit that in cases of inflammation in a small part of the cerebral lobes, especially in their upper portion, a rigid or contracted state of paralysed muscles (a pretty constant symptom then) is due to the fact that the motor nerves of these muscles are irritated in the inflamed part, we should have to admit also this absurd, or at least impossible view, that the various motor nerves of the muscles of the limbs go up to each of the different parts of the cerebral lobes. We must admit that in those cases the rigidity or contracture is produced through some peculiar influence, exerted by the inflamed nerve-fibres in the cerebrum, upon some parts of the base of the brain. Is that peculiar influence a reflex influence? I cannot prove that it is; but I think the probability is great that there is then the same process that exists when contracture is due to a reflex action from the bowels, the gums, &c.

Convulsions and Convulsive Affections.—I need not give proofs that irritations starting from any part of the body often produce convulsions, or any form of the convulsive affections (epilepsy, hysteria, catalepsy, chorea, tetanus, and even hydrophobia.) There cannot be the least doubt on this point, and facts are too numerous and too striking for my insisting upon this mode of origin of convulsions. I wish only to speak of convulsions or convulsive affections depending upon an organic disease of the cerebrum or the cerebellum. In what way does such a disease produce these symptoms or symptomatic affections? To answer the question, let us suppose, for instance, a case of a small tumour in the upper parts of the cerebellum (without pressure upon the base of the brain), producing epileptic fits. In such a case, no motor nerve-fibre going to the various muscles of the head, the trunk, and the limbs, is directly irritated by the tumour. The mode of action of the tumour must therefore be through some peculiar influence exerted over those parts of the base of the brain where exist the motor nerve-fibres. It is, in fact, by an action on a distant part of the nervous centres that the irritation in the cerebellum then acts, just as is the case when epileptic fits are the consequence of an irritation in the bowels, the skin, &c. The same thing may be said of epilepsy caused by a disease of the cerebral lobes. So that convulsions or convulsive affections symptomatic of disease located in the cerebral lobes or the cerebellum owe their origin to a sympathetic influence of the part altered upon other parts of the brain.

Conclusions.—I hope I have succeeded in rendering probable, if not certain,—

1st. That the principal symptoms, and the various affections symptomatic of brain disease, originate much less frequently than it is admitted from the loss of function of a part of the brain, or from some direct effect of a disturbance of that function.

2nd. That an alteration of the blood may be the cause of these symptoms and symptomatic affections.

3rd. That the disease of the cerebral lobes, or of the cerebellum, gives rise

to symptoms and symptomatic affections chiefly through a peculiar influence exerted by the part altered upon other parts of the encephalon.

4th. That the principal symptoms and affections symptomatic of brain disease, may be caused by an irritation starting from any viscus or any nerve of the skin or mucous membranes.—*Lancet, July* 27, 1861, *p.* 79.

15.—TREATMENT OF DELIRIUM TREMENS BY LARGE DOSES OF TINCTURE OF DIGITALIS.

By Dr. Francis E. Carey, Guernsey.

As the treatment of delirium tremens by large doses of tinct. digitalis is still in its infancy, perhaps the narration of a few cases which have occurred in my practice since Mr. Jones's report, will not prove uninteresting. The hesitation which I at first entertained in prescribing these large doses, induced me to give our hitherto recognized sheet-anchor, opium, a fair chance; but it will be seen how egregiously its character was belied by the results, it being actually instrumental in aggravating the sensorial excitement, though no contra-indication existed to its administration.

P. B., aged 42, carpenter, was admitted into the Hospital, February 26, 1861, with well-defined indications of delirium tremens. I was requested to visit him the previous day at his own house, and found him laboring under all the hallucinations of the disease consequent on a protracted debauch of some weeks, which had depressed and enervated a naturally feeble system. For nearly three weeks he had been unable to procure any refreshing sleep. During this day, I prescribed four grains of solid opium and ʒj. of tinct. opii, which produced the inverse effect of aggravating the cerebral excitement. The evening of the day of his admission, I administered a half-ounce dose of tinct. digitalis, with an equal quantity of gin, which, after the lapse of half an hour, caused him to fall into a deep sleep, which continued with one brief intermission for twelve hours. He awoke quite rational, and two days subsequently was presented cured. The digitalis did not produce its alleged results, diarrhœa and vomiting, nor were the kidneys stimulated to increased action.

R. C., aged 30, after a prolonged course of dissipation, was seized with unmistakeable symptoms of delirium tremens. On his removal to the Hospital, which was necessitated by his extreme violence, eighty drops of laudanum were given. At eight o'clock p. m., as there was no mitigation of the delirium, an additional dose of forty drops was ordered. The night was spent in wandering and in attempts to escape from restraint. The following morning, after a copious evacuation, he was ordered a combination of laudanum (forty drops), and antimony, which was repeated after four hours. He had then taken the equivalent of at least ten grains of opium in twenty-four hours. In the evening, as no amelioration had taken place, the half-ounce dose of digitalis was administered, with an equal quantity of gin. His condition throughout the night was calmer, and for four hours he enjoyed undisturbed sleep. No further treatment was deemed necessary, notwithstanding that he continued somewhat disposed to be boisterous during the next twenty-four hours. A subsidence of this soon took place, and he was dismissed cured at the end of the fifth day from his admission.

A third case occurred in a gentleman, aged 28, whose nervous system had also been subjected to the effects of prolonged alcoholic drinking. Opium was equally generously administered, but, as in the preceding cases, its inefficiency was most manifest. Recourse was had to the digitalis with such good results, as to tranquillise our patient, who had been afflicted with the most soul-torturing delusion it has yet been my lot to witness.

The fourth case has occurred within the last few days in a man aged 25. He was the subject of epilepsy, induced by his habits. His appearance betokened serious disease of the heart, confirmed by auscultation, which detected an aortic systolic murmur, with extensive hypertrophy. This patient's

daily consumption of raw spirit was not much below two or three pints. Aberration of mind quite characteristic. Opium was again resorted to, but its shortcomings were as evident as in the other cases. Half an ounce of digitalis was given in the morning. Notwithstanding my remonstrances, the patient left his room and wandered about the house, much quieted, however, till he retired to bed at his usual hour. Next morning I found him calm and composed, having had a few hours' sleep. The immediate effect of the digitalis here was to equallise the circulation, and to restore warmth to the extremities, chilled by his morning's walk.

This latter case is interesting, as indicative of the non-injurious effects of wholesale doses of digitalis in cases where morbus cordis exists. This condition of the heart, though not to the same extent, was detected in the preceding case. In any of these cases, should the first dose of the digitalis not have sufficed, I would not have scrupled administering a second, as the influence on the pulse was not such as to offer any barrier to its repetition. In proffering these remarks, I cannot avoid expressing deep satisfaction for the application of a drug to the cure of a disease, which, hitherto, has very frequently resisted the most heroic treatment. From my small experience I cannot but entertain the hope that the mortality, which has been due, without exaggeration, as much to the large doses of opium as to the unrestrained development of cerebral complications, will be strikingly diminished.—*Med. Times and Gazette, August* 24, 1861, *p.* 206.

16.—DIGITALIS IN DELIRIUM TREMENS.

By Dr. D. M. Williams, Liverpool.

[Dr. Williams gives us the following interesting case to illustrate this new mode of treatment.]

At noon of Wednesday, August 28, I was requested to see a private patient, whom I have cured of delirium tremens on two previous occasions by a single subcutaneous injection of two-thirds of a grain of sulphate of morphia, dissolved in ℥xx. of water. Since the last attack he had been perfectly sober, until August 13, when he began to drink, but continued to eat as usual until the 20th, from which time he drank brandy, and continued doing so up to the day I saw him. He became delirious on the 26th, and slept none during that or the following night, nor had he eaten food since the 20th.

When I saw him he was perfectly furious, almost naked, and struggling to get out of the house; with difficulty I ascertained that the pulse was 108. 2 p. m. Administered tinct. digitalis, ℥ ss., and persuaded him into bed. In twenty minutes the pulse had fallen to 96, full and soft; tongue loaded. He was still very much excited, and I thought the lowering of the pulse was due to position and soothing conversation. To have another ℥ ss. tinct. digit. at 4 o'clock. 5 p.m. An hour has elapsed since he took the second dose of the tincture; he has been extremely wild and restless, excepting once, when he seemed to fall into a quiet doze for a few minutes, but awoke as mad as ever. Pulse 96, full, soft, and regular; skin cool, but as he is almost naked, and cannot be induced to lie in bed covered, that is easily accounted for. To have another ℥ ss. tinct. digit. at 7 o'clock. At 10.30 p.m. had become more manageable, and has been quieter since 6 o'clock; pulse 96, but wiry, with an uncertainty about an occasional beat; has slight nausea, will take no food, but wants beer continually; to have a little brandy and lemonade during the night, and as much beef-tea as he can be induced to take.

29th, 10.30 a.m. Has had a bad night, has not slept more than an hour and a-half since he took the first dose, but has been vomiting and purging very much all night, with violent palpitation of the heart between 6 and 10 o'clock this morning. He attempted to take beef-tea, also brandy and lemonade, but vomited everything; he is, however, quite quiet, talks rationally,

and complains much of the palpitation. Pulse 86, full, soft, and steady; skin comfortably warm and moist, tongue cleaner; to take ʒ ss. every three hours of the following mixture, and as much food as possible:—R. Bismuth subcarb. ʒ ij., sol. morph. 3 j. æther chlor. 3 jss., mucilage Arab. ʒ ss., aquæ ad. ʒ viij., M. 10 p.m. Has spent the day at Eastham, where he took six and a-half glasses of brandy, but no food or medicine. Has had no vomiting or purging since morning; kidneys have acted throughout as usual. He is now quite quiet, but provokingly obstinate, and craving more brandy; pulse 96. All stimulants forbidden; to take his mixture and as much milk, beef-tea, &c., as possible during the night.

30th. He is so much better of his delirium that his friends allowed him to go to Seaforth alone, as he complained of being ill, and thought a little change would enable him to return to business quicker. His wife says he has had a good night, and she thinks he slept a good deal, has had no stimulants, but drank a large quantity of milk, and from this time steadily recovered.—*Med. Times and Gazette, Sept*, 14, 1861, *p.* 284.

17.—DELIRIUM TREMENS TREATED BY LARGE DOSES OF TINCTURE OF DIGITALIS.

By Dr. Thomas B. Peacock, Physician to St. Thomas's Hospital, &c.

[The following account of the use of this remedy in delirium tremens adds to the interest of the preceding papers on the same subject.]

The cases of delirium tremens in which the large doses of tincture of digitalis were used at St. Thomas's Hospital, have been six in number (three only included in the former enumeration), five of these being cases of idiopathic delirium, in which the affection was the direct result of excessive drinking, while in the sixth the disease was traumatic, or supervened in a man of dissipated habits, shortly after he had sustained a fracture of the thigh.

In four of these cases only one dose of half-an-ounce was given; in a fifth, the same quantity was repeated after an interval of twenty-two hours; and in the sixth, the dose first given was only two drachms, and half-an-ounce was exhibited after an interval of about twenty-four hours. In two of five cases of which I possess notes, the stronger alcoholic stimulants were given in the earlier periods of the attacks, and in all but one or two, ammonia and bark were prescribed after the digitalis had produced its effects. The ages of the patients treated were 23, 30, 33, 44, and 45. The last patient only was a female, and all recovered.

In no instance were any alarming symptoms of depression induced; but the pulse usually soon fell remarkably in frequency, and acquired greater power and steadiness. The sickness subsided; the tremor became less, and the patient was much quieted. It did not, however, appear that the digitalis directly induced sleep, but it calmed the patient, and sleep seemed to ensue naturally, after a longer or shorter interval. The patient did not, however, sleep so soon, so soundly, or for so long a period, as, from Mr. Jones's report, had been anticipated; and in two cases morphia was given before rest was procured; while, in a third, small doses of compound ipecacuan powder were exhibited at short intervals during the attack. This difference may be due to the less liberal employment of the remedy at St. Thomas's. Mr. Jones states that he generally repeats the dose in four hours, and occasionally, in a smaller quantity, for a second time; whereas at St. Thomas's two full doses were only prescribed in one case, and in that instance the second was given after an entire day had elapsed.

The kidneys did not appear to have their action increased by the digitalis; on the contrary, so far as could be ascertained, the flow of urine continued, after the remedy was exhibited, to be only small in quantity.

I would remark that it would be altogether premature to venture a decisive

opinion upon the use of the large doses of digitalis in this disease; but the facts seem to warrant the conclusions—

1st. That the drug, when exhibited in full doses, does not by any means produce the amount of depression which our previous experience of its action, in small and frequently-repeated doses, would have led us to expect; and

2nd. That the remedy, in conjunction with other means, may probably be very usefully employed in the treatment of certain cases of the disease, and especially when it occurs in young and robust persons, whose strength has not been broken down by prolonged habits of intemperance; and particularly when it arises as the immediate result of excessive spirit drinking. Delirium tremens, like other acute diseases, occurs under varied and opposite conditions, and it would be alike opposed to sound theory and practice to extend the same treatment to all cases of the disease.

I have selected two cases to illustrate the views here expressed,—the first is one in which the disease occurred after excessive drinking; in the second, a dissipated man became delirious after having sustained a fracture of the thigh, from being deprived of his accustomed stimulus.

Case 1.—J. T., aged 30, formerly a soldier who had served in North America, the Crimea, and India, and had just returned from Calcutta, was admitted into St. Thomas's Hospital, under my care, on October 14, 1860.

He was received, labouring under symptoms of delirium tremens, at half-past two, p. m., and was reported to have taken a quart of spirits daily for several days. At half-past six, when he was in a very restless and excited state, suffering from retching and sickness, and his pulse 103 in the minute, he was directed to have half-an-ounce of tincture of digitalis, and the dose was given in a quarter of an hour. At a quarter to eight the pulse had fallen to 72, and was of good volume, and he was much quieter, free from sickness, and disposed to sleep. At eleven p. m., he was much in the same state; pulse 76 and regular; he had not slept, but was quiet and still seemed disposed to sleep.

At ten a. m. on the 15th he was seen by himself: he was reported not to have slept ten minutes during the night, and had been restless and trying to get out of bed, but not violent. The tongue was dry and a little red, the pulse 80, somewhat sharp, and decidedly resistent. The sounds of the heart were natural, except the second, which was somewhat indistinct. The pupils were large, and not apparently affected by light. The urine deficient in quantity, and highly coloured. His manner was hasty, but he spoke quite rationally, and he took his food well. During the evening and night of the same day he slept for a considerable period, and the following morning he was much better. He was quite intelligent, but still had a somewhat agitated manner. The pulse was 80 to 84 in the minute, and feeble, and the second sound of the heart was very indistinctly audible. He was directed to have five grains of the sesquicarbonate of ammonia in an ounce and a-half of decoction of bark every three hours.

On the 17th he continued to improve, and on the 18th his appearance and manner were much more tranquil, the tongue somewhat dry and slightly furred: the bowels regular; pulse 80. He had made water freely, but said that the quantity was less than usual. He was directed to have the quinine and iron mixture three times daily and the mixed diet. To be allowed to leave his bed. When up there was a considerable increase in the frequency of the pulse—greater than was perhaps explained by his general state—in bed it beat steadily 80 in the minute, when sitting up it was 100, and was more irritable and feeble, and when standing it became 120, very sharp and feeble.

He was presented and went out cured on the 20th.

During the period of his residence in the hospital he was not allowed any stimulants.

Case 2.—S. S., aged 44, a waterman, admitted into St. Thomas's Hospital, on October 15, 1860, under the care of Mr. Mc Murdo, after having sustained a fracture of the thigh from a fall. On the 18th he began to have symptoms

of delirium tremens, and had become very violent when, at 4.45 p. m., a dose of 40m of tincture of opii was given to him. At 10.30 p.m., the laudanum was found not to have had any soothing effect, and his pulse was 120, and [℥ss. of tincture of digitalis was given. At 11.15 p. m. the pulse was still 120.

19th. At 9 a. m. he was reported to have been violent during the night, but less after taking the dose of digitalis till 4 a. m., when he again became more excited. He had no sleep during the night, but was then quieter. Pulse 98, quiet and feeble. Four p. m. He had still had no sleep. The pulse was 98, full and regular. To repeat the dose of tincture digitalis. Ten p. m. Has still not slept. Pulse 82, full, and regular. Pupils natural and contractile. He has passed a small quantity of water. He appears disposed to sleep.

20th, 4.30 a. m. Has still not slept. Pulse 80, regular. He has passed some water, which smells of digitalis. To have a glass of gin. 11.25 a. m. Still no sleep. Pulse 80, irregular, and intermittent. 4.45 p. m. I saw him with Mr. Whitfield. He had still had no sleep. The pulse was 80 and irregular, and he was much depressed. To have 6 grains of the sesquicarbonate of ammonia in an ounce of decoction of bark every four hours. Two pints of porter in 24 hours. Nine p. m. Pulse 70, irregular and intermittent. The bowels have been acted upon. To have four ounces of gin and half-a-drachm of the solution of hydrochlorate of morphia and a drachm of aromatic spirit of ammonia directly. Eleven p. m. Pulse 88, regular.

21st, 10 a. m. Did not sleep till five o'clock this morning. The pulse is now 84, full, soft, regular. The tongue is dry, and he complains of thirst. He has passed water naturally. Nine p. m. Has slept at intervals and taken his meals naturally, is otherwise going on well.

22nd, 10 a. m. Has slept during the night. Pulse 80, soft, slightly intermittent. The sesquicarbonate of ammonia every six hours. The full diet. From this time he progressed favourably.

Note.—At the time when large doses of digitalis were first tried in the the Hospital, struck by the very little effect which resulted from the treatment, I suggested to Mr. Whitfield the employment of digitaline; as MM. Homolle and Quevenne, and the reporters on their communication to the Academie de Médecine,—Rayer, Soubeiran, and Bouillaud,—had shown that while capable of producing all the effects of digitalis, the digitaline was much more certain in its action. This suggestion was adopted, and a case was successfully treated by a dose of the active principle regarded as equivalent to the large doses of digitalis, but I have failed to obtain the notes of this case.—*Med. Times and Gazette, Aug.* 3, 1861, *p.* 105.

DISEASES OF THE ORGANS OF CIRCULATION.

18.—ON BLOOD-LETTING IN APOPLEXY.

By Dr. Thomas K. Chambers, Physician to St. Mary's Hospital, &c.

[Perhaps no question is more serious than that regarding the propriety of taking blood in cases of coma, apoplexy, and paralysis. Delay may be irretrievable; yet a false step may be equally fatal.]

In the first place, let us clearly understand what is the *modus operandi* of the means you design to employ, and then what morbid conditions it can and what it cannot relieve.

The two most marked immediate effects of blood-letting are, diminution of the force of the heart, and contraction of the area of the blood-vessels. That these are the most important and most direct of the resulting phenomena, is shown by their being in direct proportion to the dose; the more rapid and copious the detraction of blood, the more certainly they follow. Both natural and artificial hemorrhage thus produce, first syncope, or failure

of heart-power; and then a slackening and arrest of the blood-stream by contraction of the vessels.

What now are the morbid conditions in apoplexy which such results as these are likely to benefit? I am not going to enumerate all the causes of apoplexy, because such a recapitulation would be very unsuited for the purpose of the present lecture, and, if they came into your head at the time you were in presence of your patient, would serve rather to confuse than to clear your ideas. A most practical thing to do will be to divide them into three heads with reference to the treatment to be pursued; and I think you will find them all capable of being classified under *destruction* of the nerve-fibres, *compression* of the same by a *solid*, and *compression by a fluid* substance.

Now, if the nerve-fibres be destroyed, as is the case, for example, in what appears after death as a softening of the cerebral substance, it seems unadvisable to take away the pabulum of nutrition; for you would be diminishing the power of the remaining brain-tissue to take the place, as far as it can, of that which is irretrievably lost, Nor are the immediate effects of blood-letting likely to be advantageous; for in these cases the force of the heart is not morbidly augmented, and the blood-stream is in general rather deficient in quantity already. We shall probably be only adding to the disease by substracting blood. When, then, we have reason to think from the previous history that there is old disease leading to softening of the cerebral substance, we should abstain from bleeding altogether. If, for instance, there have been before the present attack a state of mental excitement, or severe headache, or feverishness; or if the gait have been unsteady, the speech thick and stammering; if there have been earache, or a purulent discharge from the auditory meatus; if previous to the apoplexy, there have been convulsions; if the patient be scrofulous, or of a scrofulous family,—we must take all these circumstances as warnings against the lancet—altogether, probably, but certainly against its immediate use.

Supposing, however, that we have no evidence of such a diseased state of the cerebral substance, and can allow ourselves to conjecture that the nerve-fibres are more compressed than permanently disorganised, then we have further to consider whether the compressing substance is *solid* or *fluid*. By a solid likely to cause apoplectic paralysis, I refer to a clot of blood which has been thrown out long enough to become entirely coagulated. This is the only solid sufficiently common as a cause for us to consider. Is it likely you have to deal with anything of this sort? Is there in the patient's brain before you at the moment a clot of blood such as you see put up in bottles in the museum, and often also in the dead-house? Do not be in too great a hurry to conclude so at once. Four days ago you saw on the dissecting-table the corpse of a man who more than thirty-six hours previously had fallen down stairs, and was supposed to have fractured his skull: the cranium was uninjured, the fall was apoplectic, and there was a quantity of fluid blood pressing upon the brain. You see by such instances as these that blood extravasated from the vessels inside the body does not coagulate immediately, as it does when drawn from a vein; it remains long, and may remain much longer than it did in that case, in a liquid state. It takes, not minutes, not hours, but I may really say days, to become solid. When the apoplexy or paralysis has lasted for days, then indeed, but not till then, you may conclude that a clot has formed; and then, and not till then, may you pronounce that the force of the heart's action will not increase the oozing of blood into the focus of pressure. After that—after you have a solid, and not a fluid pressure, to deal with—I am willing to concede, and anxious to impress upon you, that abstraction of blood is useless and hurtful; but till then I believe its immediate influence to be a beneficial one, and in the right direction. Therefore do not conclude that a clot is formed, or that bleeding is for that reason useless, till at least twenty-four hours are passed after the fit.

Supposing that a fluid has to be dealt with, you are perhaps anxious to know of what nature it is; and, in point of fact, I find that I am constantly

asked by students, when they see a case of apoplexy in the wards, whether it is serous or sanguineous apoplexy. I say to you now, what I always say then, that there is no possibility of certainly distinguishing them; and that, fortunately, it is of no importance to either physician or patient to do so, as far as the immediate treatment of the fit is concerned. One, indeed, forms a clot after some time, which we have been just now discussing; and the other remains liquid. But that does not affect the question of the comatose condition now before us. I will speak, then, of "fluid," not distinguishing blood or serum, or bloody serum or serous blood, but clubbing them all, as must be done by practical men, together. When, then, there is fluid compressing the nerve-fibres, I feel sure that there is an influence for good in blood-letting, accompanied, of course, by a danger, as all active treatment is, but still a decided influence for good. It is capable of lessening the force of the heart, which is driving the blood towards the place where it is oozing out either as serum or complete blood, and of diminishing the calibre of the vessels that allow it to pass. The anxious question is, how to secure those advantages without the necessarily accompanying dangers, without adding too great an additional shock to the already shocked nerves, without weakening fatally the already weakened general system.

You will observe, by the example of the patient I have used as my text, that the first effect of an apoplectic seizure is the violent blow to the nervous system above mentioned. The poor woman was damp and cold, and pale as a corpse; the pulse and heart beat quick, and irregularly, and weakly, just like those of an ox stunned by the slaughterer's pole-axe. Had she been bled then, she would certainly have died outright; and moreover, even had she survived, there was no object to be gained by it; for the heart was weak enough, surely. But after a time—seven hours in this instance, sometimes sooner, sometimes later—the heart and pulse recover, and the blood must be driven against the ruptured or oozing vessels with the same force which originally caused them to rupture or ooze. Now is the time to step in with the lancet. You prevent by it what so often happens to apoplectic patients if left alone entirely; you prevent the relapse into coma which frequently follows an apparent partial recovery during reaction; you prevent it in a very intelligible way, by lowering the force of the blood-stream, which the injured vessels have already shown themselves unable to bear. This was the time, and this was the object, of the blood-letting in our patient's case. So complete was the apoplexy, by such a slender thread was she hanging on to life, that I believe the slightest additional extravasation of fluid in the cranium must have been fatal; and to this judicious use of the lancet by our house-surgeon, Mr. Chisholm, I attribute the patient's life.

You will observe the bleeding was not a large one—only eight ounces when reaction first occurred, and eight ounces again in the evening, when the pulse was again getting hard. I mention this to warn you against a mistake into which you might be led by an old and rather questionable maxim "*Extremis morbis, extrema remedia.*" You might be disposed to say, the more the apoplexy, the more the bleeding. Such a notion would be most dangerous. A small bleeding accomplishes the object in view; and a large one can do no more, while it seriously abridges the vital powers. Above all things, don't open the temporal artery; it is nearly equivalent to cutting your patient's throat.

I much prefer, in these cases, venesection to cupping the temples or the nape of the neck, as is sometimes done. What you want to do is to affect the general system, and particularly the centre of circulation—an object which is attained with most rapidity and certainty by opening a vein. You are aiming to prevent blood from being driven into the skull, not to extract that which is already in; at least, if you are striving after the latter result, your anatomy and physiology ought to have taught you better.

The effect of bleeding during the collapse, before reaction has taken place, is, as a rule, sudden death, especially when the heart or its valves are diseased. The effects of *over-bleeding* at a later period are, an excited condition of the circulation, and consequently a more violent impetus against the brain; and

at the same time a more watery state of the blood, and consequently a greater tendency to serous effusion. As it is impossible to tell whether red blood or serum is exuded in the cranium, you run the greatest possible risk of augmenting the very evil which your injudicious zeal was intended to obviate.

Perhaps you may say, "I am going to bleed my patient again, for the sake of encouraging absorption; and surely absorption of the clot should be aimed at." Now, I am not so sure of that: if it were absorbed sooner than the nerve-fibres were ready to resume their functions, something else must be effused to take its place in the cranium. At all events, I am quite convinced that such absorption is best left to the reviving powers of nature; and you are diminishing these by unnecessarily depriving the body of blood.

In apoplexy and central paralysis, remember that you are dealing with diseases where nature still retains an inherent power of repair; in a moderately healthy constitution, the tendency is to get well. Do not, therefore, attribute all cases of recovery to the means used, unless you can give a probable physiological explanation of their beneficial action; but if you can thus rationally justify your treatment, do not hesitate to feel satisfied with it, whether successful or unsuccessful.

There is no necessity for being idle while you are awaiting the time to bleed. Stimulant injections and purgatives, hot water and mustard poultices to the feet and legs, a careful arrangement of the clothing and bedding so as to prevent congestion to the head, with keeping up the animal warmth and keeping the friends quiet, will amply occupy your time. It is also conducive to the end you have in view, to shave the head and apply cold lotions to the skull.

After reaction has returned, you will find advantage in the application of ice to the head: it is physiologically correct, and is often a great relief to the patient. You may remember that this poor woman used the first power of speaking that returned to her in begging me to continue the ice; whenever it was applied, it induced sleep, and was most agreeable. What a strange state a person must be in for ice to the head to be agreeable? But so it was.

When, then, you are called in to a case of apoplexy, let these considerations pass through your minds:—

1. Does it depend on destructive softening of the cerebral substance? If so, I must not let anything persuade me to bleed.

2. Has the effusion taken place so long ago as to be, if blood, coagulated? If so, again, I had better abstain.

3. If it be fluid, bleeding is very much to be desired; it may prevent increase of the effusion, and relapse.

4. But to make bleeding most useful and least hurtful, the proper time must be selected; namely, the time when the heart regains its strength.

5. The best guide is the circulation. The sharpness and hardness of the pulse and heart, as felt by you, are a faint picture of the sharpness and hardness of the pulse, as felt by the patient's brain. With your fingers on the wrist, let your mind travel into the interior of the skull.—*British Medical Journal, June* 8, 1861, *p.* 599.

DISEASES OF THE ORGANS OF RESPIRATION.

19.—ON THE UNCERTAINTY OF THE SPUTUM AS A DIAGNOSTIC FEATURE IN PNEUMONIA.

By Dr. Thomas Williams, F.R.S., Physician to the Swansea Infirmary.

The following cases are highly instructive. They convey, in a practical sense, a lesson of great value. It constantly occurs that a case of true pneumonia passes through all its stages to recovery without once exciting

the suspicion of the practitioner as to its real nature. It is allowed to pass under the vague name of "influenza," "feverishness," "cold," or "slight fever," &c. There is no expectoration; there is no cough; there is no pain in the side; no obvious symptom is present by which the attention is invited to the chest. If a stethoscopic examination is made, it is done hurriedly and carelessly, and the case is dismissed as something which it is not. These oversights are committed because students are taught by nearly all standard authorities to lay faith in the most variable rather than the least variable symptoms, as signs of the disease.

Another class of cases mislead by the hæmoptysic character presented by the sputum. Pure blood, of a bright scarlet color, in portions of variable amount, is mixed up with the sputum, the latter being colourless, as in bronchitis. Able and cautious practitioners have condemned such cases as phthisical, and therefore fatal, in which the most complete and lasting recovery has afterwards taken place. Fallacy in this direction is painful in the extreme. The verdict of death is pronounced over those whose life is in no danger. Character and confidence are destroyed, and the medical attendant becomes the greatest sufferer.

In the description of the following cases the strictest brevity will be observed. The fact that a pneumonic consolidation of the lung existed has been carefully determined in each instance by auscultatory examination. To mention the familiar signs of such consolidation were to occupy space unnecessarily; the object being to show how extremely uncertain, variable, and untrustworthy the sputum—the expectoration—is as a symptom of pneumonia.

Case 1.—T. M., aged thirty-eight; a fine, healthy, tall, muscular man. Consolidation of middle and inferior lobes of right lung. Expectoration considerable in quantity for six weeks, blended twice or thrice a day with teaspoonfuls and tablespoonfuls of bright scarlet blood; rest of sputum not rusty nor viscid, but muco-purulent. Was very ill for two months; returned to his employment in three months. Three years have elapsed since the date of this attack; the man is now quite well; the lungs are perfectly free from any evidence of tubercle. The wife and friends have never forgiven me for having said at the time of the attack that he was consumptive and would not recover.

Case 2.—Mr. S., aged forty, a teetotaller, a broad-chested, firmly-built man, a baker and grocer. Nearly the whole of the right lung hepatized. For three weeks was very ill, without any cough or expectoration whatever. Suddenly a cough occurred. "Spitting" of frothy mucus followed. In two days the expectoration amounted to a pint; in four days, to a quart in quantity. It now became most offensive and disgusting to the smell. For a month it continued at this rate, and of this quality. It gradually ceased. In three months the man was quite recovered. In this case the first two stages of engorgement and red hepatization were marked by no expectoration whatever. The third stage, that of grey hepatization, was strikingly indicated by the characteristic sputa.

Case 3.—J. W., aged eighteen. Fine crepitation; dull percussion-note; bronchophony under left scapula; rusty expectoration copious. Was quite well in nine days.

Case 4.—Mr. J., aged forty-six. Pneumonic signs very clear under and around the left scapula; sharp heat of surface; hurried breathing. Was ill for fourteen days. Not the slightest particle of anything coughed up from first to last. No dry cough.

Case 5.—The Rev. Mr. ———'s son, aged seventeen. Very distinct pneumonic signs over a small space of the right lung between the two scapulæ. Was ill twelve days. Threw up once or twice only by cough a slight quantity of clear white mucus. No other expectoration whatever.

Case 6.—W. F., aged thirty. Pneumonia of middle and back portion of the inferior lobes of the right lung. Expectorated large quantities of characteristic rusty sputa. Quite well in sixteen days.

Case 7.—C. M., aged twenty-five. Pneumonia of middle and lower parts of the left lung. Signs unquestionably distinct. Was ill for about a fortnight. No expectoration whatever during any stage.

Case 8.—D. J., aged twenty. Fine crepitation; tubular breathing; bronchophony around the angle of the right scapula. Recovered in seventeen days. Very slight clear mucous expectoration during a day or two.

Case 9.—T. P., aged thirty-five. Pneumonia of the right lung. Signs audible round to the front under the right breast. Was ill for three weeks. Nothing that could be called expectoration at any time.

Case 10.—T. W., aged twelve. Consolidation of the back and side of the right lung; slight pain in side; slight dry cough; no expectoration whatever. Was quite well in thirteen days.

Case 11.—Mrs. D. Pneumonia of the left side; signs very clear under the breast and round under the axilla. Had a little cough, by which mucus only was brought up. Saw a slight streak of blood once only. Recovered in seventeen days.

Case 12.—A. B., a strong working man. Pneumonia of right lung extensive. Rusty sputa thrown up in copious amount for seven days. Recovery complete and rapid. Returned to work in three weeks.

Case 13.—Master D., a boy in school, aged fifteen. Signs of pneumonic consolidation very clear round right scapula; no pain; no expectoration whatever. Quite well in fifteen days.

Case 14.—Capt. F., aged fifty-five, a very stout man. Attended by the late Dr. Rowland and myself. Fine crepitation of bases of both lungs. Percussion-note dull. Was ill a fortnight, when he died in a typhoid condition. Never had the slightest expectoration.

Case 15.—A. F., aged fourteen, a boy in school. Slight pneumonia of right lung; had no cough; spat up nothing whatever. Recovered in ten days.

Case 16.—D. T., aged twenty-five. Pneumonia of right lung, signs distinctly audible between the two scapulæ. Slight clean mucous sputa. Returned to work in three weeks.

Case 17.—The Rev. Mr. ——, aged twenty-eight, a clergyman. Pneumonic signs traceable clearly from base of right lung forward. Expectorated a small quantity of uncoloured mucus. Was quite well in sixteen days.

Case 18.—Mr. L., a grocer, aged thirty-seven. Right lung extensively pneumonic. Was very ill for nine days, without any expectoration whatever. Suddenly, a severe cough occurred, and threw up large quantities of very offensive semi-purulent sputum; this continued for a fortnight. Was not well under three months. Never had any rusty expectoration.

Case 19.—J. W., aged eighteen. Lung on right side, at base and side, solidified. Was ill sixteen days. No expectoration whatever; once only a little "coloured stuff" came up to his mouth by coughing.

Case 20.—T. P., a copperman, aged twenty-seven. Distinct pneumonia of right lung; middle lobe chiefly affected. Was confined to the house for nineteen days. One day a small portion of tinged, not rusty, sputum was coughed up; no other expectoration, but a little white mucus.

During the past winter and spring, ten other cases of this character were noted by me. The notes of each in my note-book are short. They are recorded especially for the purpose of tracing the history of expectoration in pneumonia. On every occasion the diagnosis was carefully and correctly made. In two only out of these ten cases did the distinctive rusty sputa occur.

Thus, in thirty cases, twenty-six of which occurred in the same season, and therefore under the same climatal conditions, five only presented the supposed characteristic symptom, the "rusty expectoration." In one it was hæmoptysical; in two only did grey hepatization take place—that is, if the peculiarly offensive, copious sputum is to be regarded as the distinctive sign of that stage. In twenty cases cough and expectoration were, in truth, totally absent at every stage. In two the sputa were slightly tinged with blood, as is the case in slight hæmoptysis. In one it was copiously intermixed

with bright blood. The colourless mucous sputum, sometimes a little frothy, is the product of the bronchial membrane. Ciliary epithelium, with the cilia in motion, may be constantly detected in it. True epithelial cells are not discoverable in that sputum which comes from the pulmonary cells. The cell-elements of the hyaline epithelium which line these cells are lost in the pneumonic exudation. If, as supposed by Dr. Flint, the contents of the pulmonary cells in pneumonia were nothing but blood, such as it exists in the blood-vessels, and the exudation were, in truth, an act of depletion (virtually blood-letting), rather than an act or product of inflammation, and if the increased weight which occurs in the affected lung were the expression simply of the quantity of blood withdrawn from the system, pneumonia, as a pathological problem, would indeed have received a highly simplified form. To my understanding, such a solution of the problem, simple only in a mechanical sense, is by no means satisfactory. Place true pulmonary apoplexy and unmixed pneumonia in parallel columns, and compare the distinguishing symptoms; is not the difference most striking? There is scarcely a single point of resemblance. The exudative process constituting pneumonia is not exempt from the law which regulates such processes in other tissues and organs. The diphtheric deposit is a substance which possesses very peculiar and distinctive properties. Although it comes directly from the blood, it is very different from any of the constituent parts of this fluid. The arachnoid, the pericardium, pleura, peritoneum, and tunica vaginalis, when the subject of inflammation, throw out severally fluid products of unquestionably distinctive properties. The pulmonary and alimentary tracts of mucous membranes exhibit corresponding differences. The brain, liver, spleen, and kidney parenchymæ afford each a peculiar history under the circumstances of inflammation. The urine in Bright's disease is not self-coagulable. The fibrine is either detained in the blood or modified in quality. Transudation from the blood, whether it occurs in a direct or indirect manner, is, in truth, tantamount to blood-letting. The serum, lacking fibrine and red corpuscles, which is poured out in serous inflammations, may indeed be described by the free-thinker as blood drawn. The purulent fluid in suppuration would form a link in the same chain of argument.

Such reasoning may satisfy the mere machinist; it is very unexplanatory to the intelligent vital pathologist. The act of inflammation, whether marked and accompanied by a small or a large amount of exudation, is a highly complexed *vital* process. The composition of the exudated product differs according to the ultimate anatomical structure of the part affected. Mr. Lister has shown that the solids and fluids mutually react upon one another. What occurs in the fluids must affect the solids; a change in the latter will necessarily involve the former. Dr. Brown-Séquard has greatly aided our inquiries by proving that an irritation of one part of the nervous system is capable of entailing disease upon a very remotely situated structure or organ by an influence transmitted through the nerves. Local disease from this time forth will be investigated under improved knowledge. In no long time the pathologist will be able to explain *why* a local seizure falls upon one organ or part in preference to another. Already it is obvious that humoralism and solidism, as exclusive doctrines, must be banished from the schools. In studying local pathological changes, the whole act of nutrition should be considered. Beginning with the fluids, the inquiry should pass through the solids, to return again to the fluids. The doctrines of Dr. Hughes Bennett and Dr. Beale are incomplete in this respect, that they suppose visible form, or structure, to be indispensable to vitality and nutritive activity. The liquor sanguinis is a vital fluid; it is a fluid, and is vital. If it is not an amorphous, formless fluid, but a mass of germinal molecules, then inevitably it must be admitted that pure water is not a fluid, but a mass of molecules held together by mutual attraction. The idea of fluidity as a condition of matter must therefore be dismissed from the mind. Solidity is the only possible form!

It is true the chemical philosopher would say all matter is *atomic* in constitution, whether gaseous, fluid, or solid. Consenting, the physiologist

would remark that the forces by which the imaginary "atomi" of a fluid are related are lower in degree, bordering nearer on inorganic chemistry, than those which unite into a consentient whole the component particles of a formed, constructed solid. In such abstract reasoning, of course, there is no other difference than that of degree between a solid and a fluid and a gas. But whether a fluid be defined as a formless, homogeneous substance, or a mass of invisible imaginary molecules, it must be conceded that the complex and compound fluids of the animal body are *vital*—that is, that they obey those chemical conditions (probably in a less degree than the formed solids), which constitute vitality. If this be admitted, which appears unavoidable, it must follow that the "round" or cycle of the vital processes do not initiate in the molecules which the microscope can demonstrate to the eye, but in *the fluid* in which the *molecules* arise. The act of germination begins, in fact, at that border line which separates inorganic from organic chemistry. It is, therefore, undeniable that a pathological act, such as inflammation, involves the fluids as well as the solid tissue. The fluids alter in composition either primarily or secondarily, and thereupon follows a change in the mutual relations between the solids and fluids. Under this changed relation certain parts of the blood are extravasated to the retention of other parts. Viewed in this light, the differences in pathological products of different structures under inflammation admit of a satisfactory explanation. The product which fills the pulmonary cells in pneumonia is, undoubtedly, peculiar. That excepted which is contained in vessels of the Malpighian corpuscles, the blood does not lie so near the exterior in any other part of the body as in the pulmonary air-cells. The escape of the blood-corpuscles from the vessels into the cells with the inflammatory exudation, is only an accidental result, not a necessary part of the process. If a large vessel ruptures, a visible quantity of unmixed blood is poured out; if several small ones—capillaries only—the rusty tinge is uniformly mixed with the substance of the sputum. If no vessels at all are mechanically ruptured, no colour appears. The sputa are colourless, exhibiting always, under all circumstances, a singular viscidity, such as is not observed in any other pathological product. It is highly gifted with vital properties; its microscopic growth and changes occur rapidly; it is capable of almost immediate resorption. In a few days frequently a considerable consolidation will disappear. If the affected portion of the lung were simply in a state of ecchymosis, the entire history would be wholly different. As stated by Dr. Walshe, the blood is highly hyperinotic; the serum milky and fatty; the sputum overcharged with chloride of sodium; the urine with oxalates and triple phosphates; the respiration increased in frequency; the skin pungently heated. The whole system, in every sense, sympathizes with the local morbid action. If the disease consisted merely of an act of blood-letting into the lung tissue, as supposed by Dr. Flint, such phenomena would not occur because they would be unnecessary. The argument held by Dr. Flint against venesection in this disease cannot, therefore, for a moment be admitted. The exudation into the lung tissue in pneumonia is an act of pathological excretion, determined by two conditions—1, an altered composition of the blood; 2, favourable anatomical conditions afforded by the lung structure for secreting the elements of a peculiar proximate principle from the blood. The condition of the general system, coexisting in time with this pathological act of exudation, may be either sthenic or asthenic, such as never exists under the circumstances of loss of blood.—*Lancet, Sept.* 7, 1861, *p.* 224.

20.—CHLORATE OF POTASH AND SUGAR IN PHTHISIS.

By Dr. James Turle.

[In an American journal lately there was a short notice of the beneficial employment of large doses of chlorate of potash in phthisis, by Dr. Fountain. Dr. Turle corroborates this. He says:]

I have been in the habit, during the last two years, of giving a very strong solution of this salt, almost *ad libitum*, in tubercular affections. The proportions I use are one ounce to three pints of boiling water.

In the early part of this year I mentioned the subject to Dr. Garrod, who expressed an intention of making a trial of this treatment in suitable cases in the wards of University College Hospital. If he has done so, his results will be of far more value than the more limited scale on which I have tried it.

Since the end of 1859 my practice has been to combine the administration of from one to three or four ounces of the chlorate per week with the use of three or four pounds of sugar during the same period. When the patient has a dislike for sweets, I substitute the sugar of milk, on account of the little taste of this variety.

Of about twenty cases of confirmed phthisis which have been under my care since the beginning of 1860, the only one which terminated fatally was that of a lady who, from first to last, expressed a most unaccountable repugnance to the taste of the drug, though this is, in comparison with other medicines, very slight, from the small amount water will dissolve. I have, however, found the same objection preclude its use in the case of two other female patients. In but one of the favourable cases had the disease reached the third stage; but in that (the daughter of a tradesman in Portland Town) there was a very large vomica in the right lung, and at the time I commenced the chlorate she was so exhausted by hæmoptysis, hectic, and diarrhœa, that none of those who saw her, including myself, supposed she could live above a day or two. The tone of the stomach was rapidly restored, although there had been nothing like appetite for weeks previously, and her recovery of comparatively good health was uninterrupted. Her general health continued excellent six or seven months subsequently, although she was then described by a surgeon at Brighton as having but one lung left. This young person had been formerly an out-patient at University College Hospital under Dr. Hare. The same gentleman was consulted in another of my cases: that of a man in whom there was very extensive solidification of the upper part of both lungs, with hæmoptysis, and all the other symptoms of rapidly-advancing disease, with softening. His father and all his brothers and sisters had died young, of consumption, and Dr. Hare of course corroborated the very grave prognosis I had given. Cod-liver oil was ordered, but could not be borne by the stomach. Within four weeks of being put on the treatment above described, not only had the emaciation, which had been very great, ceased, but he insisted on leaving off part of the allowance of sugar, on the plea that he was getting too fat. He soon resumed his employment, that of a master blacksmith, and has not since been laid up. I mention these two or three cases to show some of my reasons for thinking favourably of the simultaneous exhibition of these two agents. I have a crude theory on the *modus operandi*, the exposition of which I shall defer to some other occasion when less pressed for time. For the present I shall be satisfied if these hurried jottings should influence some who have the opportunity to make a trial on a larger scale.—*Med. Times and Gazette, June* 1, 1861, *p.* 588.

21.—*New Remedy for Whooping-Cough.* By M. FOSTER, Esq., Huntingdon.—[The remedy in question is the common clover hay *(Trifolium in fœno).*]

While other symptoms have to be met with emetics, aperients, salines, and tonics, its effects in alleviating the spasmodic character of the paroxysms have been very remarkable, so that, even if after taking it the cough continues, it is no longer so distressing or exhaustive. I was made acquainted with it in an indirect manner; I believe it came originally from an old fisherman at Hastings.

One has naturally great suspicions about a remedy so purely empirical, but my success with it has been so great, as to cause me to recommend it to my fellow practitioners in these troublesome cases. Since using it I have

begun to disbelieve in "the natural course" of pertussis. I have generally found that it answers best when it acts slightly on the bowels.

The hay should be sweet and leafy. The formulæ are as follows:—

The Infusion:—

℞. Trifolii in fœno, ℥ij.; aquæ bullient., Oj.; macerate for four hours and then strain. For a child of five years, take a table-spoonful three times a-day.

The Syrup:—

℞. Trifolii in fœno, ℥ijss.; sacch. cand., ℥ij.; aq. bullient, Oj.; macerate the hay in the water for an hour with gentle heat, then boil down to a proper consistence. For a child of five years, take two tea-spoonfuls four times a-day.

Mr. Squire has prepared for me a fluid extract, which may be obtained from him.—*Med. Times and Gazette, May* 25, 1861, *p.* 561.

22.—ON ARSENIC SMOKING IN ASTHMA.

By Dr. Frederic G. Julius, Richmond.

[The patient was a French lady, who has been subject to spasmodic asthma for twenty-five years, for which she has been bled, smoked belladonna and stramonium, and taken every form of medicine, and changed her residence to various places in Europe,—all without the slightest benefit.]

Four years ago, when at Marseilles, Dr. Cauvin read an account to her of the benefit derived by asthmatics in China from smoking arsenic. Her sufferings were so great that, although Dr. Cauvin fully pointed out to her the risk and danger she incurred, she insisted upon trying it.

She commenced by smoking a quarter of a grain of arsenic three or four times daily in a cigarette, and this she continued to do for about fourteen days, with the greatest benefit to her breathing and general health. She has subsequently much increased the dose, and when she feels an attack of asthma coming on, she does not weigh the arsenic, but takes up what she considers a sufficient dose with a small paper knife. I asked her to-day to give me in a piece of paper the dose she intended smoking, which she did, and on weighing it carefully I found it a little over three grains. I analyzed it, and found it to be pure arsenious acid. I must also mention the important fact that she does not inhale the fumes and blow them out again as in ordinary smoking, but when her mouth is full she swallows the smoke.

The only ill effect she has ever experienced is swelling of the eye-lids, and, when she first commenced, slight pricking pains in the stomach, but never to any troublesome extent. She considers herself cured. From being in a state of constant breathlessness and suffering, unable to lie down or make the slightest exertion, she is now able to go about like other persons, and is rarely threatened with an attack oftener than once in three or four months, and that is at once checked by smoking arsenic with a very small quantity of belladonna or stramonium in the dose I have mentioned. She now uses, instead of a cigarette, a small red pipe about five inches long.

She tells me that Dr. Cauvin has used arsenic in the same way in many cases of confirmed consumption, and has rarely failed in giving great relief and retarding the disease.—*Lancet, Aug.* 10, 1861, *p.* 138.

23.—DRILL FOR AUSCULTATION.

By Dr. Thomas K. Chambers, Fellow and Censor of the College of Physicians, Lecturer on Medicine at St. Mary's School, and Physician to the Hospital.

I place before you a table where you will find seventeen indications which (as a rule) the combinations of the easily recognised signs of pulmonary auscultation afford. It will be very useful to copy them out several times, that you may have impressed on your minds the exact amount of informa-

tion which the process is capable of giving. Expect this much and no more, or you will be disappointed. Remember that auscultation tells you nothing of the *nature* of the substance whose morbid presence it has enabled you to detect. The "*fluid in the pleura with solid lung*," which "*bulged ribs*," combined with "*dulness*" and "*tubular breathing*," indicate, may be pus, serum, or blood—the history of the case alone can lead you to know which. The "*cavities*" in solid lung may be tubercular vomicæ, pneumonic abscesses, or dilated bronchi—the ear, unassisted by other records, does not enable you to diagnose. Fine or coarse crackling very often cannot be absolutely determined to be inside or outside the lung,—to be the crepitation of the tubes or the rubbing of the pleural surfaces. Do not attempt to decide by auscultation, but by other symptoms.

There are however, a few by-aids to the diagnosis by auscultation which are worthy of separate mention, in order that you may know how to use them worthily, and to caution you against their abuse.

	1. Flat ribs.	2. Bulged ribs.	3. Resonance.	4. Dulness.	5. Tubular breathing or voice.
A. Resonance	Lung recovered from pleurisy, or from condensation.	Air in pleura, or emphysema on contrast with flat side.			
B. Dulness	Lung solid and shrunk, from chronic disease.	Fluid in pleura, or aneurism, or malignant tumor.			
C. Tubular breathing or voice.	Lung solid and shrunk, from chronic disease.	*If dull*, fluid in pleura, with solid lung.	Tubes stiffened, without solid lung.	Lung solid.	
D. Fine crackling	Recent action added to old disease.	Emphysema and roughened pleura, acute, or chronic.	Lobular congestion, early stage of pneumonia.	Lobular congestion, early stage of pneumonia, or pleurisy.	Lung becoming solidified, or becoming pervious.
E. Coarse crackling	Lung solid and shrunk, from chronic disease, with muco-pus in bronchi and cavities.	*If resonant*, emphysema and bronchitis; *if dull*, fluid in pleura, with adhesions and bronchitis.	Bronchitis with slight, if any, consolidation.	Solid lung, with muco-pus in bronchi or cavities.	Much muco-pus in large bronchi or cavities.

1. *Whispering Pectoriloquy.*—When the patient whispers a few words of several syllables, and the distinction between the syllables is clearly heard, it is often taken as a sign of a cavity. It is an absolute sign of *diseased lung*, but not an absolute sign of a cavity, unless it is found in a limited spot with a clearly defined line, outside which line the voice is quite different.

While on this subject I will give you a hint about auscultating the voice. Always make the patients speak in words of several syllables: make them count "twenty-one," "twenty-two," &c.; not "one," "two." You get twice as much information thus.

2. *Cracked-pot Sound.*—A jingling noise like a cracked-pot, or more like the striking of the clasped palms on the knee, as when we amuse children by imitating the jingling of money, is often produced by percussing a superficial large cavity while the patient holds the mouth open. But it is not a sign absolute. I used to show you last year in the wards a child with a cavity at one apex, where the cracked-pot sound never occurred, though it was remarkably distinct almost always in a perfectly healthy part of the opposite lung. It is caused exactly in the same way as the imitative money jingle in the close

hand—namely, by the air being *jerked* out of a partially open space. When the parietes of the chest are thin, it may thus be produced by the jerk of the air in the trachea or large bronchi. Do not, therefore, set store by it as a sign.

3. *Metallic Tinkling.*—This is a valuable sign of a very large cavity with small bubbles bursting in it. It is like the sound of a small shot or fragment of gravel falling in a thick metal vase. It shows the existence of either a vast vomica, or of air in the pleura, with a communication open to the bronchi.

I will now go through the eighteen simple combinations into which these tabulated signs may enter, and point out what information you get immediately from them, and in what direction you are to seek for more on which to ground your diagnosis.

Combination A 1.—The *flattened ribs* show that some morbid state has existed; there has been either inflammation of the parietal pleura, which has thickened it with contracted scars, or a condensation of the lung for a sufficiently long period to leave cicatrices, and to diminish the volume of the organ. But the recovered *resonance* shows that the lung is again at work—that the fight is over, and that corn again grows on the battle-field.

A 2.—Be quite certain that the *bulging of the ribs* is not merely apparent, and that the other side, being flattened from disease, does not make them seem too rounded and prominent by contrast. If they are really *bulged*, and yet resonant, there must be a larger body than natural beneath the point of percussion. And this air must be either in the pleura (pneumo-thorax) or in the lung (emphysema). If the former, you find a peculiar resonance like that produced by tapping the stomach under the ribs, you find the breath-sounds quite absent, and probably also "metallic tinkling." If the latter, you have breath-sounds, perhaps bronchial breathing, and not unfrequently a peculiar crackling like the crumpling of fine tissue paper. When you hear this "crumpling," it is very distinctive. It arises, I would suggest, from the rubbing of the rugged, bubbly-looking pleura of the lung against the parietal.

B. 1, C 1, and C 4.—Solidification of the lung makes it dull, and makes the voice and breathing bronchial or tubular, whether arising from pneumonic acute condensation, or from such a chronic cause as tubercle. But if the cause be chronic, the ribs will be flattened in proportion to its chronicity and the length of time it has endured.

B 2.—*Dulness* on the *bulged part* of the ribs shows that some matter more solid than air is bulging them. In the lower part of the chest it is most probably fluid, and then you must try, if you can, a confirmation of your diagnosis by making it move about as the patient changes his position from one side to another. In the upper part it may be an aneurism of a trunk vessel, and then you get pain on pressure of the corroded ribs in the place where they are gnawn away by the lump inside. To the diagnosis of a pectoral malignant tumour you can arrive only *par la voie d'exclusion;* but in all the cases I have seen, the veins, by their remarkable prominence in the immediate neighborhood, have led me to the suspicion.

C 2.—The only case where bulging of the ribs is likely to be joined with tubular breathing is where a lung adherent to the ribs is condensed by the pressure of fluid, and then absolute dulness is of course produced.

C 3.—The larger air-tubes sometimes become stiff and hard from chronic degeneration without any hardening of the long-tissue, especially in old people, and then you get *resonance* combined with *tubular sounds.* The knowledge of this chronic degeneration is important, as it leads you to expect future degeneration in more important viscera,—such as of the lung-tissue, in the form of emphysema; of the kidneys, as Bright's disease; of the heart, as dilatation, &c.

D 1 and E 1.—The flat ribs show that there is chronic disease of some standing, and this will modify the activity of your treatment of the acute action indicated in its first stage by the fine crackling, and in a later stage by the coarse crackling. In such a case as this, the fine crackling is an

evidence *against* a vomica; while the coarse crackling (especially if bounded by a sharply-defined line) is the best single evidence you can have of one being already formed.

D 2.—The condition of fine crackling or crumpling as a sign of emphysema has been just mentioned under B 2.

D 3, D 4, and D 5.—When only separate lobules are congested (as in the first stage of pneumonia), the quantity of air in the lung is not sufficiently diminished for dulness on percussion to be distinguishable by the ear; but when the whole substance of the lung is infiltrated with blood or serum or fibrin, instead of air (as in a more advanced stage), comparative dulness is to be perceived. However, this is not such absolute dulness as in a further stage, when it is so solidified that no air enters. Then fine crackling ceases, and silence or coarse crackling or tubular sounds take its place. Then, as the lung again becomes pervious in the course of cure, fine crackling returns; the acoustic condition of *becoming* solid and of *becoming* pervious being the same, though the fluid which causes that condition is different.

E 1, 2, 3, 4, 5.—A *coarse crackling* is distinctive of nothing but the immediate cause of the sound—viz., a thickish fluid in the area where it is found, whether bronchi or vomicæ. It may be associated with all sorts of conditions of lung. You hear sometimes physicians pronounce that there is "bronchitis" in a certain case, and expect those who have consulted them to be satisfied with such a diagnosis, and nothing more. It is no diagnosis at all, and certainly not worth a guinea; for it helps in no degree in the treatment, and gives no information available for prognosis. Every patient with emphysema, chronic or acute catarrh, hard tubercle, vomicæ, pleurisy, and what not, has from time to time a secretion in the bronchi (or bronchitis), and to declare with grave sapience that "there is bronchitis," is simply to put into bad Latin what the patient has been telling *you*—namely, that he has got a cough. No; when you do take your patients to a physician, get something more out of him than that.—*Lancet, Oct.* 19, 1861, *p.* 369.

DISEASES OF THE ORGANS OF DIGESTION.

24.—CURIOUS CASES OF FURRED TONGUE ON ONE SIDE, FROM VARIOUS CAUSES.

By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital, &c.

Furred Tongue on one side caused by a Decayed Tooth on the same side.—As I purpose making the case of gangrene of the fingers from pressure upon the ulnar nerve the culminating point of a little group of cases bearing upon the subject of physiological rest, or rather the evil resulting from the want of physiological rest to the nerves, I must request your attention to the case of a tongue furred on one side, and comparatively clean on the other. Now, this furred condition of the tongue, let me say in anticipation of what I have to speak of more in detail, is frequently associated with a diseased condition of the second division of the fifth nerve; in fact, it is a functional and structural deterioration depending upon nervous influence, in that respect very much resembling what we have already seen in its extreme condition in the case of gangrene of the fingers.

Furred Tongue on one side, depending on Disease within the Cranium.—The first time I had an opportunity of clearing up such a case as this, occurred in 1843. On December 17th, 1843, with Mr. Blenkarne, a surgeon in the city, I examined the body of one of his patients, who had died with disease of the brain and spine. She had suffered from intense pain on the left side of the head. She had also during her life, a furred tongue on the left side, and scarcely at all on the right side. It therefore became an important point to clear up what was the probable cause of that condition of the tongue. We made a post-mortem examination, and found a diseased spine, as was anticipated. Then, upon very careful examination of the head,

for the especial purpose, on turning up the dura mater from the anterior part of the petrous portion of the left temporal bone, we exposed the Gasserian ganglion, or the ganglion of the fifth nerve, and there we found what might be called a scrofulous deposit upon the convex edge of this ganglion, involving the second division of the fifth nerve, more than the third, but still involving all more or less. Here, then, appeared to us to be the explanation of the probable cause of the furred tongue on its left side. This observation was made in 1843; the disease was near the ganglion of the fifth nerve, and on the same side as the furred tongue. If I should not mention that fact again, it will be clearly understood that the diseased condition of the tooth, or of the fifth nerve, was always found on the same side as the furred tongue, and that the fur was confined to the anterior two-thirds of the upper surface of the tongue, over the distribution of the lingual gustatory nerve—a portion of the third division of the fifth.

In 1844, when delivering some lectures upon the nerves at Guy's Hospital I mentioned this case, and one other that I had seen before, the cause of which I had not distinctly proved. After the lecture, one of the students said, "Look at my tongue; it is furred on one side, and I cannot get rid of it." He had a decayed and painful tooth—the second molar in the upper jaw. That tooth was, by my advice, removed, and after a short time the fur on the tongue entirely ceased. Now, here, in this case, appeared to be a demonstration, as far as we could judge from these concurrent circumstances, of the probable cause and effect. Here was a painful and diseased second molar tooth in the upper jaw, on the same side as that on which the tongue was furred, and apparently the cause of that condition; for as soon as the tooth was removed the fur on the tongue ceased, and the two sides became quickly alike. This case seems to confirm the opinion as to the cause of the unilateral furred tongue; but there is this peculiarity and difficulty in the explanation, which I am anxious to admit,—that instead of the cause being associated with the third division of the fifth, as you might expect from the lingual gustatory nerve supplying that part of the tongue which is furred, in all the cases which I have seen where the cause was connected with the teeth the seat has appeared to be the first or second molar tooth of the upper jaw (the second division of the fifth nerve) on the same side as that on which the tongue is furred. I must leave others to explain how that occurs. I am responsible for the accuracy of the facts which I have advanced, but I cannot explain the relation of cause and effect in these instances.

Case of Fractured Base of the Skull, with Furred Tongue on one side only.—Some time afterwards a case of fracture of the skull came into Guy's Hospital, where the fracture ran across the foramen rotundum containing the second division of the fifth. In that case the man had furred tongue on the same side, indicating the probable relation of cause and effect.

When Mr. Bransby Cooper was alive, a gentleman was brought to Guy's Hospital, who was believed to have fractured the base of the skull, and being anxious about the case, he requested Sir Benjamin Brodie to see him. That patient had a furred tongue on one side, and he had also indications, from the loss of sensation about the face, that the line of fracture interfered with the second division of the fifth. He had a furred tongue on the same side as that which we supposed to be the seat of injury during the whole time he was in the hospital.

Furred Tongue on one side; Second Molar Tooth of Upper Jaw Diseased.—This last spring, a lady whom I have known for some years, consulted me regarding some matters not of importance, and I, perhaps not for any very precise purpose, said, "Let me look at your tongue." She put out her tongue, and it was furred on one side. I said, "You have a bad tooth," and she thought it was exceedingly clever on my part. "Yes," she replied, "and I am going to Mr. Bell to have it taken out." She went and had it extracted; I saw her a fortnight afterwards, and all the fur had subsided. This was a second molar tooth in the upper jaw of the same side as the furred tongue.

Very recently my own boot-maker had some little accident, and he came

to my house to consult me. I found he had a furred tongue on the right side, and I remarked to him, "You have a decayed tooth in your upper jaw." "No," he said, "I have not; but I have had a bad tooth stopped, and it is very painful when I touch it in this way, (pressing his finger upon his cheek over the tooth;) almost the whole of the time since I had my tooth stopped, my tongue has been like this."

Now these cases taken together, and made into a series or small group, seem strongly to suggest the influence of irritation or of pressure upon what we may call common spinal nerves grouping the fifth nerve as a common spinal nerve, although it has its relation only with the face and the head.

Grey Hair on the Temple depending on a Decayed Molar Tooth in the Lower Jaw on the same side.—This spring a person was brought to me by a surgeon suffering very great pain on the left side of his face. He was much exposed to the weather, and he suffered a great deal in consequence. He had taken many things to cure the neuralgia, as it was termed. I observed that he wore a wig, and I asked him the reason. He said, "Curiously enough, the hair on my left temple has all turned grey. I did not like to have black hair on one side and grey on the other, so I had my head shaved and wear a wig." Upon examining his mouth I found he had a decayed and painful molar tooth on the left side of the lower jaw—the third division of the fifth nerve. The patch of grey hair appeared to me to be the effect of the nervous association of the temporal branches of the third division of the fifth nerve and the decayed tooth in the lower jaw. When this second molar tooth in the lower jaw was extracted the neuralgic pain very nearly ceased. I have not seen the patient since, and cannot say whether the hair has recovered its colour. All I can say is, as it was stated to me, that during the time he was suffering extreme pain on the left side, the hair over the temporal region became nearly white—that difference in colour suggesting to myself some structural deterioration, and to the patient the propriety of having his head shaved and wearing a wig.

Diseased Molar Tooth of the Lower Jaw producing Excoriation of the Auditory Canal on the same side, and an enlarged Lymphatic Gland, cured by removal of the Tooth.—In last year's lectures I mentioned that a professional friend of mine had suffered from a condition bearing on the subject of the influence of nerves upon the structures supplied by them. That friend, unfortunately, is no more. It was Dr. Addison to whom this happened. The case is one of some interest, and I will repeat it in a few words. Some years ago Dr. Addison had a very offensive discharge from the auditory canal of one of his ears, which annoyed him very much; and below the external ear was a small gland enlarged in the upper part of the neck. He had tried various remedies for this discharge, and had gone, I believe, to some surgeons who attended specially to the ear; but, as far as I could learn, no good resulted from any of the applications. Upon examining the ear from which the offensive discharge proceeded, I found a slight ulceration upon the floor of the auditory canal. On arguing the question out between us, we came to the conclusion that the ulceration probably depended upon a diseased molar tooth in the lower jaw on the same side. We had that tooth extracted, and in a very short time the ulcer healed, the discharge and morbid secretion disappeared from the auditory canal, and as soon as that ulceration was cured the enlarged gland subsided. Here the true cause of error was remotely situated from one of the most prominent symptoms,—namely, the enlarged gland in the upper part of the neck: in that respect the case is of great interest. The case stands also in nice and close association with this group of results of nervous disturbance which prove that irritation induced in a nerve, or pressure upon a nerve, is sufficient to lead to more or less change in function and structure, and that that morbid influence may after a time induce a deterioration resulting in, or sufficient to produce, a furred tongue, ulceration of skin, or gangrene of the fingers. In Dr. Addison's case there was an ulceration in the auditory canal, and the gland enlarged as the consequence of the exposed lymphatics absorbing and carrying the morbid fluid of the ulcer down to the lymphatic glands. That I believe to be the true

interpretation of glandular enlargements from the irritation, as it is termed, of a remote cause; the "irritation" is supposed to travel along the lymphatics to the anatomically associated glands. I believe this hypothesis of irritation is an erroneous idea, and that the true explanation is, the arrival at the gland of a morbid fluid conveyed to it by the lymphatics. I doubt the validity of the reasoning which endeavours to press upon us the conviction that mere irritation can be conveyed from a sore to a lymphatic gland; it must be that the absorbents carry a morbid fluid centripetally to the gland. Fortunately for us, these glands are highly conservative in their tendency, and they stop the progress of the morbid fluid in its onward course towards the circulation of the blood, where its damaging influence might be more diffused, and lead very frequently to the occurrence of the more serious general condition which we are in the habit of denominating pyæmia.—*Lancet, August* 31, 1861, *p.* 201.

25.—ON DIPHTHERIA.

By Dr. WILLIAM BUDD, Clifton, Senior Physician to the Bristol Royal Infirmary.

[It must be well known to our readers what a great variety of remedies have been suggested in this disease, each having its warm supporters.]

About two years ago, on the strong recommendation of my brother, Dr. G. Budd, I began to employ Beaufoy's solution of chlorinated soda, undiluted. Before that time, I had tried most of the preparations in vogue; but this one has seemed to me so superior to them all, that I have since abandoned every other in its favour. In my hands, it has appeared to have more than all the advantages of the other remedies in common use, without any of their drawbacks.

In all the cases in which I have hitherto tried it—amounting now to fifteen—the local disease has begun to decline from the moment of its first application. An immediate subsidence of swelling in the lymphatics in the neighbourhood of this disease, has, in particular, always followed its use—a fact from which the inference seems pretty clear, that this agent may do much to prevent that secondary poisoning of the system by the absorption of putrescent matters from the throat, which, in many cases, has the chief hand in giving a malignant turn to the malady.

On the other hand, its employment is attended by little or no pain: it causes no inflammation, and it leads to none of those breaches of surface which are inseparable from the repeated use of powerful escharotics, and which, when once produced, often make convalescence so painful and protracted.

Lastly—and this alone is a consideration of the highest importance—by exercising a local disinfection in the very focus of the diphtheritic virus, there is reason to suppose that this preparation greatly lessens the contagious power of the malady.

A full-bodied camel's-hair brush is the best instrument for applying it; and to insure its full efficacy, both as a curative and a sanitary agent, the throat should be thoroughly mopped with it as many as three or four times a day.

The general treatment adopted was, in all cases, in entire accordance with that now almost universally followed, in so far as this consists in a supporting plan, and a pretty liberal use of alcoholic liquors.

Diphtheria is the effect of a poison, whose chief characteristic it is to produce rapid septic changes; and to enable the body to resist these changes, an abundance of good nutriment, and a sufficiency of wine or spirit, are indispensable.

These are matters which, in this country at least, are so well understood, that I need not enter further into them. The only speciality to which I have to refer in connection with the medical treatment of the cases which form the subject of these remarks, was the administration of the tincture of sesquichloride of iron in considerable doses.—*British Medical Journal, June* 1, 1861, *p.* 575.

26.—ON THE TREATMENT OF ACUTE DYSENTERY BY IPECACUANHA.

By Dr. R. W. Cunningham, Assistant Surgeon to H. M. 4th Bengal Europeans.

[It is only lately that ipecacuanha has been used in large doses in tropical dysentery, though it has long been known and used as one of the best remedies in that disease. It was formerly given in comparatively small doses; now in doses of ʒ i.— ʒ iss.]

On admission, a sinapism is applied to the epigastrium, and ♏ xxx. of tincture of opium are given, for the purpose of rendering the stomach more tolerant of the remedy. An hour afterwards, one drachm to one drachm and a half, of ipecacuan is given in powder. Mr. Docker recommends it to be given half an hour after the tr. opii.; but, by waiting for a whole hour, the medicine is retained longer, and produces a more powerful effect. As may be supposed, considerable nausea is the result; but vomiting does not generally come on until at least an hour afterwards, sometimes two hours. During this nausea, copious perspiration is produced over the whole body; the pulse becomes more full and soft, and less frequent; the countenance loses the expression indicative of suffering, so marked in acute dysentery; tenesmus and abdominal pains immediately cease, and the patient has no more stools for 12, and sometimes for 24, hours. As soon as the nausea passes off, he expresses himself immeasurably relieved. The perspiration is kept up for a considerable time, and, of course he feels languid and depressed for a little; but this has not, in any case that I have seen, proceeded so far as to call for any interference. When the patient next goes to stool, he does so without pain or tenesmus; the evacuations are fluid, and hold in suspension small masses of fæces, but are entirely free from blood or mucus. Sometimes the first dose is not sufficient, the dysenteric stools returning after a short interval; in which case a repetition of the dose will be required. Chicken broth and arrow-root are the only diet allowed. In most of the cases that I have met with nothing further has been required after the action of the ipecacuanha, than to keep the patient upon the above-mentioned diet for a few days, until the bowels recover their tone; or, if any tendency to debility manifested itself, a few doses of infusion of chiretta with nitro-muriatic acid have been sufficient to complete the cure.

The *rationale* of the action of ipecacuanha, thus administered, appears to me to be, that it produces the same relaxing effect upon the mucous membrane of the bowel that it does upon the system in general: a copious flow of the secretions of that membrane is produced, and an effect similar to that which we produce when we scarify an erysipelatous surface is the result,—the membrane is relieved from the tension caused by the congestion and effusion of lymph, thereby preventing destruction of tissue and ulceration. The excreted fluids lubricate any scybalæ that may be retained in the saccules of the colon; and the general nausea overcomes the spasmodic action of the muscular coat, thereby enabling the scybalæ to be set free. But the action of the ipecacuan does not end here; enough is retained to produce a cathartic effect; or it may be that the whole mucous surface of the bowels has been induced to pour forth an increased secretion during the nausea; at all events the stools become copious, fluid, and fæculent. This catharsis continues for several days, thereby relieving any abdominal congestion that may exist, by causing a drain from the whole portal system; and it possesses this advantage over that produced by most purgatives, that it is mild, free from irritation, and neither constipates nor debilitates the bowel after it has ceased.

Of the cases treated by me, or which I have had an opportunity of seeing, not one has had a relapse after leaving hospital; and, on looking to the old Registers, I find that cases similar to those I have seen were, under other systems of treatment, much longer in hospital, and also very liable to relapse. Army surgeons have this advantage, that all acute cases of illness must come under their observation in the very commencement; so that, in most cases of acute dysentery, the patient is under treatment while the disease is yet in its

first stage, and no actual destruction of tissue has taken place. The means is therefore invaluable by which we can arrest this frightful disease in its infancy, before it has advanced to the stage of ulceration, rendering necessary a long and tedious course of treatment, and too often giving rise to serious complications in other organs than the one originally affected in simple acute dysentery. By overcoming the disease at the onset, we at once relieve the general abdominal plethora with which it is associated; and thus, though the small intestine, mesenteric glands, liver, and spleen, be at the same time in a state of active congestion, the action of ipecacuanha as above described, is not only efficient in removing the actual inflammation of the organ primarily affected, but is even more so in overcoming the congestion of other organs, so intimately related as these are with the principal seat of the disease.

Though my experience of this disease has been very limited, I think we may fairly assume, on these grounds, that the occurrence of hepatic and other complications will be far less frequent, and chronic dysentery much more rare, than under other systems of treatment which require a longer continuance of the remedies, and thus allow active congestion of important organs to ripen into actual inflammation.

I append the following case as illustrative of the previous remarks. It was acute and uncomplicated. The patient was six days in hospital; but, after two days, may be said to have been convalescent, as the only treatment consisted in a regulated diet, no medicine whatever having been given.

Pte. J. L., aged 28, a stout, healthy-looking man, was admitted on the 20th October, suffering from acute dysentery. Has been to stool twice at hospital within a few minutes, and says, that during the whole night he has been to stool every half hour. He has much tenesmus, intense heat and pain in the rectum, deep-seated pain and burning sensation in the left iliac region; skin hot and dry; pulse small and frequent, about 100; tongue slightly coated. The stools contain no fæculent matter, but consist entirely of mucus, with bright fluid blood.

℞. Tr. opii ℥ xxx. stat. sumend.

℞. Emplast. sinapi epigastrio applicand, et post horam.

℞. Pulv. rad. ipecacuanhæ, ʒ iss.

Diet.—Chicken broth, with ʒ i. of arrowroot added to one pint.

Vesp. The medicine was retained for a very short time. There is a little improvement in the stools, but they are still frequent, and attended by much tenesmus.

Repeat.—Medicamenta supra scripta.

21st. Has had no stool since he took the ipecacuan last night. Retained the dose for two hours. All abdominal pain and tenesmus have entirely passed off. Deep pressure in the left iliac region does not now give pain. Skin cool and moist; pulse more full and soft, 90.

Vesp. Has had no stool; says he is quite well.

22nd. Has had a small stool this morning, fluid and fæculent, without any dysenteric appearances, and without pain or tenesmus.

Vesp. Has had a large fæculent stool, fluid.

23rd. One stool this morning; becoming more solid. Continues quite well, and complains of not having enough to eat.

24th. Continues well; has had no stool since yesterday. To have mutton chop ʒ viii. in addition to the former diet.

Discharged on the 26th October, quite well; and now continues so on the 1st November.

This case affords a good example of the success of the ipecacuanha major plan of treatment. The failure of the first dose I attribute to its not having been retained sufficiently long, the nausea and other effects were consequently very transient, and insufficient to produce the desired result.—*Edinburgh Med. Journal, July* 1861, *p.* 26.

27.—ON THE TREATMENT OF DIARRHŒA BY NITRIC ACID AND OPIUM.

By Dr. Patrick J. Hynes, Nottingham.

[Dr. Hynes recommends an old favourite remedy, which is sometimes forgotten, and we therefore think it well to let the reader see it again. He says:]

About ten years since, diarrhœa and dysentery prevailed rather extensively in this locality, and emboldened by the successful treatment of these diseases from the remedy I was in the habit of employing, I ventured to bring it, through the medium of the public journals, into notice, and since that time I have been repeatedly assured by many gentlemen who adopted it, that my opinion of its efficacy has been by no means exaggerated. I believe I am indebted to the work of Dr. Abercrombie 'On Diseases of the Abdominal Viscera,' for the formula I am in the habit of employing; but as I write from memory, and have not an opportunity of confirming my opinion on this point by a direct reference to that excellent work, I do not wish to deprive any other author of whatever merit may be found to attach to its introduction as a remedy for those diseases.

I may observe that during the last four or five weeks diarrhœa has been very prevalent in this locality, and I do not overstate when I assert that I have used it in more than a hundred cases already, and always with almost instantaneous benefit. The formula that I generally employ is as follows: Compound infusion of gentian, eight ounces; tincture of opium, a drachm to a drachm and a half; nitric acid, twenty minims; one ounce to be taken after every liquid stool or painful alvine evacuation. A mustard plaster applied to the epigastrium, and drinking sparingly of ice-cold mint-tea, relieve the sickness and thirst that frequently accompany the severer form of these diseases. I have found the remedy equally beneficial in some rather intractable forms of dysentery. I would strongly urge upon gentlemen, who meet with dysenteric cases, to give it a trial, as I feel satisfied they will find it a very powerful curative agent in their hands.

In an old number of the 'Medical and Physical Journal,' from which several years since I made an extract, I find the following from a Mr. Hope, of Chatham. He says:—

"The first occasion of its use was remarkable. A young man, of sobriety and temperance, had suffered long with dysentery, and had been attended by a friend of mine for some time, who recommended those remedies that high authority and experience pointed out for relief; but finding no advantage, he sent for me, expecting more might be done. Nothing, however, recommended was successful, and, as I could but go over the same ground, no prospect of relief appeared; indeed his death was daily expected. At this time a woman who lived with him was attacked with dysentery, with extreme thirst. An acid occurred to me; but fearing it might produce unpleasant effects, opium was added:—Nitric acid, two drams; opium, two grains; water, two ounces; a spoonful to be taken in any vehicle three or four times daily. The effect produced was so great that my dying patient, unknown to me, begged to partake with her, and when I saw him next morning, which to my great surprise was with a cheerful countenance, he told me if ever I had a patient ill with his complaint, I should never fail to send the drops I had sent for the woman, for they had relieved his complaint at the first dose, and he was sure he should mend now, for they had saved his life this time. This was the only medicine he took, and in a few days he was able to walk about his room. In a third case I tried the acid without the opium, but it did not succeed. I then united them, and it effected immediate relief. I was still unwilling to persuade myself into the belief of its being a specific remedy, until a case of so extraordinary a nature occurred as compelled me to decide unequivocally in its favour. A young lad, sixteen years of age, fell over a dredging-boat into the water in the month of July last. Indisposition succeeded, but to what degree I could not deter-

mine, as another practitioner was at first called in to his assistance. Finding no relief, they applied to me, requesting my attendance. This was near a month after the accident. The remedies I applied failed in their efficacy. His friends, despairing of relief, requested me not to trouble myself to attend him any more (they lived six miles from me,) saying he must be left to his fate, being assured a day or two more would finish the scene. At this time I recommended the anodyne, and with great difficulty it was that they could be persuaded to give it a trial. Twenty-four hours had not elapsed before he began to find relief, so that in four hours he left off the remedy. The disease returned; reapplication of the drops again removed it, and in a very short space of time, without any other medicine intervening, he became as hearty as ever he had been. The manifest advantage of the medicine was recognized by the parents; nor did they spare unjust reflections that I did not employ it before."

I am unwilling to theorise upon the *methodus medendi* of what I believe to be the principal agent in the above formula; yet I cannot help thinking that the nitric acid possesses some disinfecting agency, no less than an astringent efficacy, over autumnal diseases. "The fumes of nitric acid are believed to be efficacious," says the late Dr. Montgomery, "in destroying the effluvia of typhus and other febrile diseases." Diluted with water so as to form an acidulous drink, Dr. Duncan of Edinburgh used to employ it in the low fevers that prevailed in the suburbs of that town. But independent of its chemical action over animal effluvia, it appears to me to act as a direct astringent in all diseases of the mucous membrane. Thus in purulent ophthalmia, what remedy is so efficacious as nitrate of silver in solution? In fact, in all mucous discharges it is almost the sole remedy upon which the surgeon depends; and of course the nitric acid is the chief agent in this valuable therapeutic. I am in the habit of employing a formula of a nearly similar kind as a topical application, but with double the amount of acid, in cynanche and in diphtheria; and I can speak very strongly upon its beneficial effects. In broken-down constitutions impaired by mercury, by syphilis or other irregularities, the above remedy will be found frequently valuable; and given in combination with taraxacum, it will prove very serviceable in sluggish conditions of the liver. I am in the habit of employing it with very considerable advantage in the diarrhœa of infants, and, combined with the muriated tincture of iron, in tabes mesenterica. I may further state that I have given the other acids, singly and in combination, the full benefit of a fair trial in all the above forms of disease; and, without any prepossession or prejudice, my experience enables me to give a decided preference to the claims of the nitric acid in combination with opium, as a very superior therapeutic remedy. I have not had an opportunity of making trial of it in the very severe forms of epidemic cholera, although I have had formerly ample opportunities of treating that disease. I have, however, many years ago, tried extensively, *inter alia*, the plan of treatment recommended by Dr. Toulmin of Brighton, thus anticipating his views (see the 'Lancet' of August 17th), which certainly look well in print, particularly as they are eloquently, indeed elegantly, introduced; but when weighed in the balances they will be found wanting.—*Lancet, August* 31, 1861, *p.* 204.

DISEASES OF THE URINARY ORGANS.

28.—ON THE DISEASES OF THE KIDNEY, ACCOMPANIED BY ALBUMINURIA.

By Dr. C J. B. Williams.

[The following remarks were drawn forth by a paper read before the Royal Medical and Chirurgical Society by Dr. Dickinson. Dr. Williams commenced by suggesting the question as to whether the granular and smooth

mottled forms of kidney disease were not different stages of the same affection.]

Many years ago, when more particularly investigating this subject, he had arrived at the opinion that the smooth and mottled kidney was the early form of the disease, which at first was attended with smoky urine from the addition of blood, and that, subsequently, as the kidney advanced to a granular condition, there was no blood and less albumen in the urine, which was of a low specific gravity, for want of its natural constituents. He believed that he had traced some of those cases to an original congestion or inflammation of the kidney, as, for instance, after scarlet fever. Such cases would be better for treatment, and many were quite cured; but others in subsequent years would become liable to kidney disease, ending ultimately in granular disease. He quite admitted the anatomical differences, of disease of the cortical and of the tubular parts of the kidney, yet he considered that such differences were different stages of one disease rather than separate and definite affections. Cold, or the scarlet fever poison, might attack the tubules, and fill them with diseased epithelium (acute desquamative nephritis), endangering life by their obstruction; but suppose this danger escaped, the same cause might proceed insidiously to attack the cortical structure and produce the granular kidney. He would mention some facts connected with the association of degeneration of the kidneys with organic disease of the heart, cerebral apoplexy, and consumption. The belief in this connexion had been forced on him so strongly that he could have no doubt of it. There may be some doubt whether the affection of the heart or of the kidney took place first. He was quite sure that in some cases of heart disease the kidney became affected, and he believed that often disease of the heart killed by inducing disease of the kidneys. He had shown that disease of the heart *per se* is not necessarily a fatal disease, that the patient may live, provided he lives in a lower grade of vitality, a reptile's life, and so he may exist for many years; but when any great excretory organ became affected, then would come the beginning of the end; and no organ was more likely to be the immediate cause of death than the kidney. Simple hypertrophy would lead to the same results in extreme cases, in which the force of the circulation caused an exudation of serum into the urine. On the other hand, the condition of the blood in renal disease had a peculiar effect on the action of the heart. He had seen cases in which, from a particular sound of the heart, he had been led to suspect disease of the kidney, sometimes a reduplication of the second sound, but more frequently of the first, what is often called the "cantering sound." He had seldom heard this without also finding some renal affection. Purgatives sometimes relieved this condition of the kidneys, and then the cantering sound and signs of hypertrophy would cease. The association of phthisis with renal disease was not so common, as phthisis destroys life before the kidney can become extensively affected. But in instances where by care and treatment phthisis was protracted for very long periods, for years, he had seen death from granular disease of the kidney, as proved by post-mortem examination. He admitted that in a large majority of cases this disease was very intractable, but it was not uncommon to see old, gouty people, whose urine every now and then contained a little albumen and was of low specific gravity, and yet who lived for a great length of time. It was in his experience rare to find a kidney not somewhat granular after forty, and as such persons generally die of some other disease, it might reasonably be supposed that there were means of at least retarding the progress of the disease during life, just as phthisis might sometimes be kept comparatively quiet for many—even thirty or forty—years.—*Med. Times and Gazette, June* 8, 1861, *p.* 615.

29.—ON URÆMIC INTOXICATION.

By Dr. W. A. Hammond, Professor of Anatomy and Physiology in the University of Maryland.

Professor Hammond has a long article on this subject in the 'American Journal of Medical Sciences' for January 1861, in which he contraverts the opinion of Frerichs, that uræmia is due to the conversion of urea into carbonate of ammonia, and ascribes the poisoning to direct action of the elements of the urine retained in the blood, upon the brain and nervous system; and among these elements, urea may be deemed the most poisonous. Frerichs performed two series of experiments upon which his theory is based: the first of which consisted in injecting into the veins of dogs, the kidneys of which had been previously removed, a solution containing from thirty-one to forty-six grains of urea. In a period varying from one and a quarter to eight hours, the animals became restless, vomiting and convulsions followed, ammonia was expelled with the expired air, and coma and death supervened; after which ammonia was found in the blood, stomach, bile, and other secretions. In the second series, a solution of carbonate of ammonia was injected into the circulation, the kidneys remaining intact; convulsions occurred immediately, followed by coma, the expired air meanwhile loaded with ammonia. As this ceased to be expired the animals slowly recovered. The author considers this no evidence that carbonate of ammonia was the cause of death, but on the contrary that ammonia may generally be detected in the respiration of healthy dogs, and is generally present in the blood of most animals, the truth of which he has satisfied himself of by repeated observation and experiment.

It is admitted that urea is a normal constituent of the blood in variable proportions; and it is only when the kidneys fail to eliminate in accordance to the requirements of the system, that the normal balance between the urea and blood is disturbed. In support of his views, the author cites three series of experiments, the first of which was the injection of urea into the blood of sound, healthy animals. Large dogs were fed for three days on meat, during which time ammonia was constantly found in the breath, and urea in the blood. Upon injecting a solution of urea, symptoms of uræmia were soon apparent, of a severity in proportion to the amount injected. Upon a second examination of the blood, the urea had increased in quantity, while the ammonia remained about the same. "From the foregoing experiments it is perceived that there is a limit to the power of the system to eliminate urea, and that when this substance is introduced into the blood in large quantity, it causes death by uræmia."

The second series of experiments had reference to the effects following ligature of the renal vessels, or removal of the kidneys. For this purpose large dogs were selected, urea found in the blood, and ammonia in the breath before the operation. After removing the kidneys, or ligaturing the arteries, the amount of urea was increased threefold in a short time, and there was no evidence of any of it being converted into carbonate of ammonia. The animals died in periods varying from forty-nine to two hundred and seventy-eight hours. Congestion and inflammation of important organs were produced, as after injection of urea into the blood, or during the course of Bright's disease. It was also seen that "so long as vomiting and purging continued, there was no accumulation of urea in the blood, and consequently no uræmic intoxication."

In the third series, the kidneys were removed, and urea or urine was injected into the blood, which induced death with all the symptoms of uræmia, in from eight to fifteen hours; and when urine was injected into the circulation death occurred in a shorter time than when simple urea (though to a greater amount) was used in solution, whence it is inferred that urea is not the only poisonous element in urine. From his experiments and observations, Dr. Hammond arrives at the following conclusions:—

1. The injection of urea, in limited quantity, into the blood of animals, produces a certain amount of disturbance in the nervous system, similar in

its symptoms to the first stages of uræmia, but that this condition even disappears, if the kidneys are capable of so depurating the blood as to eliminate the toxic substance.

2. Urea, when introduced into the circulation in larger quantity than can in a limited period be excreted by the kidneys, induces death by uræmia.

3. By ligature of the renal arteries, or removal of the kidneys, the elements of the urine being retained in the blood, render this fluid unsuitable to the requirements of the organism, and, consequently, induce a condition of system not essentially distinguishable from the uræmic intoxication of Bright's disease, or that caused by the direct introduction of urea into the blood. As, however, was pointed out by Bernard and Barreswil, so long as the urea, or the products of its metamorphosis, are discharged by the stomach or intestines, uræmia does not take place, but, that when these channels become closed, convulsions and coma are produced, and death soon follows.

4. The introduction of urea or urine into the circulation of animals, the kidneys of which have been ablated, shortens the life of such animals, as Frerichs and others have already shown.

5. There is reason to believe that the urine, as a whole, is more poisonous than a simple solution of urea, for, in those cases in which urine was injected into the blood, the amount of urea thus introduced was much smaller than that previously thrown in, in a pure state, and yet symptoms of as great intensity followed.

6. Urea, or the elements of the urine, as a whole, induce such a condition of the nervous system, as strongly to predispose to congestion and inflammation of the viscera, especially the lungs, pericardium, and spleen.

7. Urea, when directly injected into the blood, or suffered to accumulate in this fluid by extirpation of the kidneys, deranges, in some manner, the process of sanguification, so as to disturb the normal relation of proportion existing between the white and the red corpuscles, and either to hasten the decomposition of these latter, or to interfere with the due removal from the blood of such as are broken down and effete.

8. There is no reason to suppose that, under the circumstances specified, urea undergoes conversion into carbonate of ammonia, but that, on the contrary, there is sufficient evidence to warrant the conclusion that no such process ensues. The fact that in the foregoing experiments a larger amount of urea is generally found in the blood taken from the body after death, than in that abstracted during life, is, of itself, conclusive against any such hypothesis.—*British Med. Journal, May* 25, 1861, *p.* 550.

30.—THE URINE IN ALBUMINURIA.

By Dr. Lionel S. Beale, F.R.S.

"If the quantity of albumen be small, amounting to milkiness or opalescence when heat is applied, or nitric acid added to the urine, and especially if the urine be pale and of specific gravity 1012 or lower, we should suspect that the lesion giving rise to the escape of the albumen was chronic, and would ultimately destroy life.

"If the proportion of urea to the other constituents of the solid matter were large, we should form a more favourable opinion than if the per-centage in the solid matter were very much less than in health. In the latter case, a great part of the renal structure would probably be involved; but in the former there would be reason to think the disease had only affected a certain number of the secreting tubules. There are, however, some exceptions to these general statements. Patients have passed small quantities of albumen in the urine for many months, and it has afterwards disappeared. In other cases, the progress of the disease is exceedingly slow. I have known a man pass urine of the character above mentioned for upwards of twelve years; and I believe this might go on for twenty years, or even longer, the patient perhaps dying at last of some other malady."

In testing for albumen in urine, Dr. Beale reminds us that, in certain specimens, nitric acid occasions a cloudiness from the precipitation of uric acid or an acid urate, quite independently of albumen. The former may be distinguished from the latter either by boiling, which dissipates the urate, or by the microscope, which reveals crystals of uric acid in it. It is, however, quite easy to tell the difference between them without the aid of either means. If the test-tube containing the urine be held in an inclined position, and the acid allowed to trickle slowly down its sides, the heavier liquid will sink through the urine, and form a distinct layer at the bottom of the tube. If albumen be present, a cloudy zone will be produced *immediately above and in contact with* the nitric acid; but if no albumen exists in it, but an excessive proportion of urates, a cloudy zone will appear *at the surface of the liquid*, while the lower part—that in contact with the nitric acid—will remain perfectly clear.—*British Medical Journal, June* 8, 1861, *p.* 608.

31.—ON A NEW CLINICAL METHOD OF ESTIMATING THE QUANTITY OF SUGAR IN DIABETIC URINE.

By Dr. William Roberts, B.A., Physician to the Manchester Royal Infirmary.

[The following paper by Dr. Roberts is highly interesting, and will enable the busy practitioner to ascertain the quantity of sugar in diabetic urine, in a very short time. It is quite true that when a man has a long round of patients, he has not time to perform many of the minute operations which would enlighten him as to the nature of the cases before him; but when a thing can be done easily and rapidly, it will be done much more frequently.]

The only instrument required is the urinometer, an instrument with the use of which every practitioner is familiar, and especially so in this very complaint, diabetes. The principle of the method is most simple. It is, to take the specific gravity of the urine before and after fermentation, and, from the loss of density occasioned by the conversion of sugar into carbonic acid and alcohol, to calculate the amount of sugar destroyed.

I would suggest that this be called the CLINICAL METHOD of estimating sugar, in order to distinguish it from another and older fermentation process, the principle of which is to determine the weight of carbonic acid evolved in fermentation, and therefrom to calculate the quantity of sugar broken up.

The analysis of a diabetic urine by the clinical method is thus performed:—About four ounces of the urine are poured into a twelve-ounce phial, and a lump of German yeast, of the size of a small walnut or so, is added. The phial is then loosely corked, or covered with a slip of glass, and placed on the mantel-piece, or other warm place to ferment. Three or four ounces more of the same urine are likewise put up tightly corked in a *companion phial*, without any yeast, and placed beside the *fermenting phial*.

Fermentation soon begins, and proceeds with such rapidity, that in twenty-four hours not only is the process completed, but the froth and scum are dissipated from the surface, and the urine, though still cloudy, is in a fit state for the second step in the process,—namely, taking the densities. It is advisable, however, to remove the phials from the mantel-piece a couple of hours beforehand, so that their contents may cool down to the general temperature of the room.

The specific gravity of the two products is then separately observed in the usual way,—by pouring the urine into a cylindrical glass vessel, and dropping in the urinometer. The fermented urine will be found to have suffered a very great diminution of density, ranging from thirty to forty degrees—more or less, according to the amount of sugar destroyed. This diminution of density holds such proportion to the sugar originally present in the urine, that, for each grain of sugar per fluid ounce, there is a fall of one degree of density; or, in other words, *for every degree of density lost, you may count one grain per ounce of sugar in the urine.*

The following example will serve to illustrate the proceeding:—

Density of a diabetic urine before fermentation,	1040 degrees.
Density after fermentation, ...	1002 ..
Density lost by fermentation, ...	38 ..

These 38 degrees of 'density lost' indicate that the urine contained exactly 38 grains of sugar per fluid ounce, or 740 grains per imperial pint. In ten minutes—five minutes one morning and five minutes the next—an accurate quantitative analysis can thus be made. Indeed, the preliminary part of the operation may be left to the patient himself or his attendant. There is no skill required to set aside a few ounces of urine in a twelve-ounce bottle, with a lump of German yeast, and to place beside it a 'companion phial' of the same urine without yeast. The medical attendant need only perform the second part of the operation—the taking of the densities; so that, with five minutes a-day, he may keep an exact register of the diurnal separation of sugar by a diabetic patient.

The reason why so large a phial is required for so small a quantity of fermenting urine is, that sometimes a great deal of froth and scum are produced, and the contents overflow unless plenty of room be provided. The cylindrical glass used for taking the densities does not answer for a fermenting vessel, because the greater depth of the liquid in it impedes and protracts the fermentation. Its wide mouth is likewise less easily managed than the narrow mouth of a bottle. A vent for the escape of the carbonic acid has to be provided, but evaporation of water and alcohol must be kept at the lowest possible point.

The solid German yeast is superior to brewer's barm for the present purpose, inasmuch as it interferes less with the original density. German yeast yields scarcely any soluble matter to water or urine, and consequently has no sensible effect on the density. The quantity used should be in great excess, in order to quicken the fermentation; but a little more or a little less does not perceptibly sway the results. In a trial carefully made, with a view of testing this point, I found that 96 grains of yeast added to a 1000 grains of saccharine urine depressed the density, through fermentation, from 1036·40 to 1001·12, showing a loss of 35·28 degrees. The same urine, with 192 grains of yeast, or twice the quantity, lost 35·20 degrees. So that although twice as much yeast was used in the second as compared with the first experiment, the result was so nearly alike in the two cases that they may be regarded as identical.

A somewhat more important matter than the quantity of yeast is the degree of clearness attained by the fermented urine at the time when its density is taken. A fermented urine remains turbid for many days after the cessation of fermentation, and a sediment gradually accumulates at the bottom of the vessel as the floating sporules of the yeast plant subside. As the liquid clears, there is a progressive, though slight, diminution of its density. In twelve hours the density falls, from this cause, about a quarter of a degree, and in twenty-four hours it falls from two-thirds to half a degree. Error from this source, at the worst, is of no great moment; and it may be reduced to insignificancy, or altogether avoided, by taking the density always at about the same time after the cessation of fermentation. With an adequate supply of yeast, and a sufficiently warm situation, fermentation is usually completed in about twelve hours, and the remaining twelve hours of the day suffice for the dissipation of the froth and scum. It is therefore advisable, for the sake of accuracy as well as convenience, to take the density of the fermented urine about twenty-four hours from the commencement of the experiment.

The use of the 'companion phial' is not of course, an essential feature of the analysis; but it affords two not inconsiderable advantages. It does away, in the first place, with the necessity of making a note of the specific gravity of the urine the day before, and permits the two densities to be taken at the same moment and submitted to immediate comparison. But a far more important advantage is, that it provides a sure and easy means of obviating inequalities of temperature, which would otherwise seriously compromise the

exactness of the results. A variation of temperature of 10° F. affects the specific gravity about one degree; so that, if the temperature of the fermented urine were 20° higher or 20° lower than that of the unfermented urine at the time when the densities were taken, an error of two grains per ounce would creep into the analysis. And if the densities were taken on the successive days, a variation to this extent might easily arise from oscillation of the external temperature, or of the heat of the room. But if the two urines —the fermented and unfermented—be kept at the same temperature, and they would be so standing side by side, the difference between their specific gravities would remain constant, whether that temperature were 70° or 40°, and the accuracy of the analysis would be maintained.

The ordinary urinometer answers very well for the determination of the gravities. It is advisable, however, to choose an instrument with a long scale for easier reading. If more accurate results are desired, the scale may be divided into two parts on separate instruments. I have had constructed for my own use two urinometers, the scale of each ranging through 30°—one from 995 to 1026, and the other from 1025 to 1055. The degrees are so far apart that, in a clear urine, the density can be read to a quarter of a degree.

The clinical method is susceptible of very great accuracy. Indeed, if the densities are taken by the specific gravity bottle, at the temperature of 60°, it scarcely yields to the volumetrical analysis, as may be seen by a glance at the following tables. The first table places in comparison twenty observations made by the two methods on various diabetic urines, with densities ranging from 1031·52 to 1053·48.

No.	Sugar per 100 parts by the Clinical Method.	Sugar per 100 parts by direct Volumetrical Analysis.	Difference.
1	7·51	7·69	0·18
2	7·47	7·69	0·22
3	6·68	6·66	0·02
4	6·72	6·66	0·06
5	5·16	5·18	0·02
6	5·19	5·18	0·01
7	5·65	5·77	0·12
8	5·65	5·77	0·12
9	4·47	4·35	0·12
10	4·49	4·35	0·14
11	7·85	8·06	0·21
12	5·91	6·10	0·19
13	11·27	11·36	0·09
14	11.21	11·36	0·15
15	5·69	5·68	0·01
16	8·11	8·06	0·05
17	8·09	8·06	0·03
18	8·00	8·06	0·06
19	8 29	8·20	0·09
20	7·61	7·74	0·13

These results are so close, that the discrepancies may be considered as within the limits of unavoidable error.

In order yet further to test the constancy of the results the method was subjected to a still severer trial. A natural diabetic urine was chosen, and its per-centage of sugar accurately ascertained. Various dilutions of known strength were then prepared from it, both with water and with healthy urine. Assuming the estimate of sugar in the original urine as correct, the per-centage of sugar in the dilutions could be ascertained with almost absolute correctness by a simple calculation. These dilutions were then fermented, and their loss of density compared with the known proportion of sugar contained in them.

The following table exhibits the results :—

	Sugar per 100 parts by the Clinical Method.	Sugar per 100 parts by calculation from the first estimate.	Difference.
1. A natural diabetic urine,	5·91		...
2. The same, mixed with 1-10 of its bulk of water,	5·31	5·32	0·01
3. The same, mixed with 2-10 of its bulk of water,	4·71	4·73	0·02
4. The same, mixed with 3-10 of its bulk of water.	4·16	4·14	0·02
5. The same, mixed with 1-10 of its bulk of healthy urine,	5·34	5·32	0·02
6. The same, mixed with 2-10 of its bulk of healthy urine,	4·77	4·73	0·04
7. The same, mixed with 3-10 of its bulk of healthy urine,	4·15	4·14	0·01
8. The same, mixed with 9-10 of its bulk of healthy urine,	0·70	0·59	0·11

Numbers so nearly alike as those in these two columns may be regarded as practically identical. In the last experiment only, where the quantity of sugar was under one per cent., was there a sensible discrepancy.

Solutions of cane sugar in water and in healthy urine were subjected to similar trials, and yielded equally satisfactory results.

In the foregoing part of the paper, the result of the analysis has been brought out as so many grains in the fluid ounce ; and from the simple relation existing between the 'density lost' and the number of grains of sugar per ounce, this is evidently by far the most convenient formula for clinical purposes. If it be desired to bring out the result as so much per 100 parts, the following formula expresses the amount of sugar in those terms. Let D be the density of the unfermented urine, and D′ the density of the fermented product; then — D′ expresses the density lost in fermentation. By multiplying the 'density lost' into the co-efficient 0·23, the product represents the sugar per 100 parts. That is, sugar per cent., or $S = D - D' \times 0{\cdot}23$.

The successive steps of the analysis may be thus recapitulated :—

1. Four ounces of urine are placed in a twelve-ounce phial, with a lump of German yeast of the size of a small walnut.
2. This is loosely corked, or covered with a slip of glass, and placed in a warm place to ferment.
3. A companion phial filled with the same urine—say a three-ounce phial —is tightly corked, and placed beside the fermenting phial.
4. In about twenty-two hours, when fermentation has ceased, the two phials are removed, and placed in some cooler part of the room.
5. Two hours after—that is, about twenty-four hours from the commencement of the experiment—the contents of the phials are separately poured into cylindrical glasses, and the density of each observed.
6. The difference between the two densities is thus ascertained, and every degree of 'density lost' indicates one grain per fluid ounce of sugar in the urine.—*Edinb. Med. Journal, Oct.*, 1861, *p.* 326.

22.—*New Test for Saccharine Urine.*—This test, which possesses extreme facility of application, and is stated by the author to be of great delicacy,

depends upon the simple conversion of the sugar into caramel. It is thus described:—"Upon a clean slip of tinned iron, place one or two drops of the suspected material, and hold it over a spirit lamp; the fluid will speedily evaporate, leaving, if the process be arrested at that point, scarcely a trace upon the metallic surface. Continue the application of heat; in a few moments after the dessication is complete, a spot of an inch or so in diameter, over which the drop had spread with the first ebullition, will gradually assume a rich reddish-brown colour, with a brilliant lustre, as if coated with a film of varnish or Japan laoquer. A strong heat produces a darker colour, but the lustre continues till the heat becomes sufficiently intense to decompose the substances."—*Southern Med. and Surgical Journal; and London Med. Review, May* 1861, *p.* 569.

33.—ON ANASARCA.

By Dr. CHARLES WITT, Spring Gardens, London.

The following method of relieving that well-known result of internal disease—anasarca, may prove of interest to the profession. It is, to say the least, well worth a careful inquiry, whether something cannot be done, however temporarily, to effect the removal of the water, and thus to save our patients much of the alarm and annoyance of this addition to their sufferings.

Let me premise, that there appears to be nothing in anasarca partaking of the character of a curative effort of Nature, it is usually the consequence of a mechanical obstruction to the circulation, and experience has not yet shown whether there is any valid objection to such consequence. Medicines have been plentifully administered with this object, but, it is well known, with no satisfactory results; and I venture to hope, therefore, that in the simple process here submitted means of relief will be found. By an easy and painless operation (founded on the structure of the tissue where the mischief lies) the whole of the serum can be quickly discharged. The way in which this may be accomplished will be best shown by detailing a case and its treatment:—

A lady in Northamptonshire had long suffered from anasarca, the result of heart-disease. From the chest downwards the body was so much distended with fluid that for many months she had been unable to lie down, but was obliged to sit day and night on a chair. On account of her size and weight, every requisite movement was effected with difficulty; her breathing was short and laboured: to be brief, her death was almost from hour to hour expected. At a consultation on the case, Dr. Robertson (consulting physician to the Northampton Infirmary), Mr. Dix (of Long Buckby), and myself being present, the following mode of relief was adopted in the utter hopelessness of any other:—Each leg was placed in a separate foot-bath, when a lancet was deeply inserted into the mass of cellular tissue, which projected most on the outer side of each ankle. A stream of water instantly began to flow from both apertures, and continued to do so with such volume and rapidity that in less than twelve hours the whole of it was removed, and the body and legs had assumed their natural size and form. Bandages of flannel were repeatedly applied as the diminishing size required, and were kept tight about the body as a precaution against faintness. The relief to the patient was immediate and gratifying, and for the first time for several months she laid down on her bed and slept comfortably. There was, of course, a slight oozing from the apertures; but these, being kept closed by adhesive plaster and bandages, readily healed—almost as soon, perhaps, as they would have done in a patient otherwise healthy. The operation was subsequently performed by Mr. Dix six or seven times during the remainder of the lifetime of this patient, which lasted about five months after the first administering of relief, and the water was never again allowed to accumulate so as to cause inconvenience.

I trust that a satisfactory proof is here given of what may be safely done, even in the advanced stage of anasarca; and I submit that the same treat-

ment will prove efficacious in its earlier stages by the removal of smaller accumulations of water, while the operation may be repeated as often as is required. Some faintness occurred after the first operation in the above case; indeed, the loss of a large quantity of fluid which has so long pressed on the blood-vessels must inevitably cause this tendency. Hence arises the necessity of keeping a bandage round the body; and it is necessary also that the patient should keep strictly quiet for some time. As to erysipelas following this operation (the probability of which has been suggested by one or two of my medical friends), I am not of opinion that this is much to be feared. Let me add, that if patients afflicted with anasarca are to be left without artificial aid in the discharge of the serum collected—what follows? Nature attempts it, in a somewhat similar, but ineffectual manner; the distension goes on increasing, till at last the legs burst open by ulceration, and then only a partial escape of the fluid takes place; no real relief ensues, not to speak of the pain of so tedious a process. In the majority of cases, I believe that certain relief may be given, as well as life prolonged.—*Lancet, Oct.* 19, 1861, *p.* 370.

SURGERY.

AFFECTIONS OF THE BONES AND JOINTS, ETC.

34.—ON AMPUTATIONS.

By THOMAS P. TEALE, Esq., Surgeon to the Leeds General Infirmary.

[In an interesting paper on this subject, chiefly referring to amputations at the Leeds Infirmary, Mr. Teale makes some remarks which are valuable. He finds that in traumatic cases the secondary amputations were much more fatal than the primary; and, that his rectangular operation had the advantage over other methods. This favourable result he attributes to the more complete covering of the end of the bone, and to the avoidance of irritation and distress in the subsequent treatment. He says,]

In Hospitals, when erysipelas and pyæmia are prevailing, stumps frequently "open out," suppurate, or become phagedenic. But when rectangular stumps are thus affected, and life is preserved, the resulting stump is, with very rare exceptions, a good one. In several cases, apparently most unpromising, although the stump has lain long without showing a disposition towards repair, yet when recovery was at length accomplished, there was a soft moveable mass of tissues over the end of the bone, almost as satisfactory as in the cases which had done well from the beginning.

After ordinary amputations it is admitted by surgeons of great experience, that exfoliation and necrosis are *frequent*. After rectangular amputations they are *rare*. After 103 of these operations, in two cases only, and to a very small extent, have exfoliation or necrosis come under the knowledge of my colleagues or myself.

In some of the cases of rectangular amputation published in the journals, sloughing of the long flap is occasionally noticed. Our Hospital records show that it is about equally frequent but less extensive in this as compared with other modes of operating. The following short extracts from the hospital note-book show all the notices which it contains of sloughing of the stump. It thus appears that in 103 rectangular operations, sloughing occurred to a greater or less extent, in 8, as follows:—

1. Thigh, pathological, one inch of slough.
2. Leg, traumatic, one corner.
3. Thigh, pathological, one inch and a-half slough, excellent stump.
4. Leg, pathological, a little sloughing.
5. Leg, pathological, a little sloughing.
6. Arm, traumatic, some sloughing, good stump.
7. Leg, pathological, sloughing of considerable portion.
8. Leg, traumatic, considerable sloughing.

In 86 amputations, not rectangular, sloughing is recorded in 6, as follows:—

1. Thigh, pathological, double flap, some sloughing and exfoliation.
2. Thigh, traumatic, circular, considerable sloughing and sequestrum.
3. Leg, pathological, double flap, some sloughing.
4. Leg, traumatic, irregular, considerable sloughing.
5. Arm, traumatic, irregular, sloughing.
6. Forearm, traumatic, circular, extensive sloughing, and bone exposed.

From these records it appears that, so far as the cases at the Leeds Infirmary are concerned, sloughing is a more serious affair in the general amputation than in the rectangular. It should also be stated, in reference to the

rectangular operation, that, in consequence of its being a new proceeding, minute deviations from a satisfactory state were noticed; whereas in the other and earlier operations, slight degrees of sloughing have probably occurred, and have not been thought worthy of notice.

As far as sloughing in rectangular amputation of the leg is concerned, a circumstance may be mentioned which has, I believe, in some instances, been the cause of it. I allude to the wounding of the anterior tibial artery at the base of the flap. This accident may be avoided by observing the following rule. The long anterior flap having been marked out by carrying the two lateral incisions through the integuments only, and the transverse incision through all the structures down to the bone, the aponeurosis covering the muscles in front of the leg should next be divided throughout the whole length of the flap close to the edge of the tibia on one side, and of the fibula on the other. This being done, there is little need for the further use of the knife, as the finger is almost sufficient to detach the mass of muscles upwards from the interosseous ligament, more especially in the situation of the artery, a few touches of the knife being only required laterally to separate the muscles from the bones.

The most marked advantage of the rectangular amputation is seen in the character of the stump produced by it. The perfection of a stump may be tested by its ability to bear pressure on its extremity, and thereby to favour free locomotion. I can with confidence state that, with scarcely an exception, in the cases under the observation of the surgeons of the Leeds General Infirmary, these stumps allow of a firm bearing of their extremity on the bottom of the socket of the artificial limb, and, consequently, give to the patient great stability and confidence in walking; while the circular and double-flap stumps rarely can bear pressure on their extremity, and the subjects of such stumps have to remain contented with the uncertain support which is derived from the apparatus resting on the head of the tibia, or on the tuberosity of the ischium.

On the grounds which have now been stated, namely, the somewhat lessened mortality, the greatly diminished suffering in the after treatment, and the vastly superior condition of the stumps, so far as comfort and usefulness are concerned, my colleagues and myself during an experience of six years have adopted the rectangular as our general mode of amputating, deviating from it from necessity, and not from choice, in such cases only in which the extent of the injuries or the disease renders the rectangular amputation impracticable or undesirable.

The following table of the two classes of amputations, as practised during each of the eight years embraced in this record, will show the extent to which the rectangular has, in our estimation, superseded the other methods of amputating:

	Amputations not Rectangular.	Rectangular Amputations.
1853	23	—
1854	22	—
1855	13	6
1856	8	11
1857	9	21
1858	2	19
1859	2	16
1860	7	30

—*Med. Times and Gazette, July* 6, 1861, *p.* 6.

35.—ON THE INFLUENCE OF REST IN THE TREATMENT OF DISEASES OF THE JOINTS.

By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital, &c.

When a joint becomes inflamed, it is painful and difficult of movement; it becomes involuntarily fixed by Nature's own process, thus securing compara-

tive rest to the interior of the joint. Indeed, we may lay it down as an axiom, that Nature instinctively renders an inflamed joint comparatively fixed and flexed. Now, what is the key to the explanation of the flexed position and rigid state of an inflamed joint? Some might say, "Because the patient finds it the easiest position for himself, and the easiest position must, of course, be the best." But this is no answer to the question, How is this position induced? It is not voluntary. The patient cannot either prevent it or change it when it exists; nay more, it is not true that the flexed position is the easiest; for every day's experience declares the contrary, and I shall hope to have the opportunity of showing you distinctly that, when the hip-joint is diseased, it is flexed, and it is often in a very painful state; but that when the femur is put straight so that the flexed condition is destroyed, the patient from that time is free from pain. Here is an instance in direct contradiction of the idea that the flexed condition is the easiest for the patient. When the joint is made straight, the patient experiences, almost immediately, a diminution of pain. It is true, however, that in the case of the knee-joint it is sometimes otherwise; the reason for which I will endeavour to point out, through the medium of these diagrams, which represent the knee-joint in an extended, semi-flexed, and completely flexed position. It is plain that when the femur, tibia, and patella are in the position in which you here see them, the leg being in a state of extension, there must be a much larger extent of articular surface coaptation than in any other position of the joint; and this surface coaptation becomes more and more reduced, or less in extent, as the tibia is carried backwards away from the femur; and, probably, it is this diminution in the extent of the articular surface contact, as well as the diminished pressure, which explains the feeling of comfort that patients sometimes experience when the inflamed knee-joint is allowed to become flexed.

Still the question comes to us, Why is an inflamed joint fixed and flexed? It appears to me to result from the circumstances to which I shall allude presently more in detail—viz., that the irritated or inflamed condition of the interior of the joint (say the knee-joint), involving the whole of the *articular nerves*, excites a corresponding condition of irritation in the same nervous trunks which supply both sets of muscles, extensors, and flexors; but that the flexors, by virtue of their superior strength, compel the limb to obey *them*, and so force the joint into its *flexed condition*. The joint becomes rigid and flexed then, because the same nerves which supply the interior of the joint supply also the muscles which move the joint.

This anatomical arrangement, physiologically considered and rightly interpreted, may be made the means of explaining some of the most prominent symptoms of joint diseases, and of constituting a foundation for the required treatment of diseased joints by rest, which I propose to advance and to recommend.

In order to place this important anatomical, physiological, and pathological subject comprehensibly and definitely before you, I will state it thus: "*The same trunks of nerves whose branches supply the groups of muscles moving a joint furnish also a distribution of nerves to the skin over the insertions of the same muscles; and—what at this moment more especially merits our attention—the interior of the joint receives its nerves from the same source;*"—thus implying an accurate and consentaneous physiological harmony in those various co-operating structures.

The large drawing which is placed before you was taken from a recent dissection, kindly accomplished for me by Mr. Durham, of Guy's Hospital, and depicted by an artist (Mr. Tupper) not now attached to Guy's School. You may see here the circumflex nerve branching into the teres minor and the deltoid muscles, distributing itself to the skin over the deltoid, and then proceeding to the skin over the fascia of the upper arm, to which the deltoid muscle is partly affixed. Yonder will be noticed the articular branch of the circumflex nerve, the branch which proceeds to the shoulder joint. Other nerves go to that joint, such as a branch from the subscapular trunk, which is derived from almost precisely the same part of the axillary plexus as the

circumflex nerve. So the suprascapular nerve, after supplying the supraspinatus and infraspinatus muscles, sends little filaments to the interior of the same joint. Thus the same circumflex nerve supplies these muscles, the joint which these muscles move, the skin over the muscles, as well as the skin over the fascia, which is a part of the insertion of the deltoid muscle. But here I touch upon a point, by which I am reminded to state that every fascia of the body has a muscle attached to it, and that every fascia throughout the body must be considered partly as the insertion of a muscle. I shall be able hereafter to trace nerves derived from the same trunk supplying the joint and muscle, sometimes distributed over a large extent of fascia. That extent of nerve distribution, however, I shall show to have a very exact relation to the proportionate attachment of the muscle to the fascia, of which I shall be able to adduce some well-marked examples.

The object of such a distribution of nerves to the muscular and articular structures of a joint in accurate associations, is to insure mechanical and physiological consent between the external muscular or moving force, and the vital endurance of the parts moved—namely, of the interior joints (whether it be of friction or of pressure); thus securing in health the true balance of force and friction until deterioration occurs. If this point of balance or adjustment be overreached during exertion, pain, Nature's warning prompter, is induced within the joint, and suggests the necessity of diminishing or arresting exertion. This cessation or this reduction of exercise, or friction and pressure upon the articular structures, *must be effected* by the muscular apparatus of the joint, either through the will, or immediately by its own instinctive efforts, called into play by means of the nervous association. The muscles, indeed, appear to be told, through the medium of the nerves of the interior of the joint, that its articular structures are overtaxed; and, the antagonistic muscular forces of the joint being thus involuntarily excited, it (the joint) is at once rendered rigid and stiff, for the purpose of keeping it at rest. Or it may be put in this way (only as confirmatory, however),—that the interior of the joint itself, under the influence of physiological exhaustion, contributes something towards the same end, by failing to secrete the proper quantity of synovial fluid. The joint thus robbed of its lubricating medium demands of the *exhausted muscles*, for its movement a still greater effort than in its normal state; and their inability to respond to the extra demand necessitates the quietude or rest of the joint. Still in this mode of viewing the subject, the necessity for limiting exertion in order that the interior of the joint may obtain the congenial rest requisite for the renewal of its physiological power and structural integrity, is intimated to the moving agents by the same nerve interpreters, and they (the moving agents), in obedience to the announcement, render the joint stiff and difficult of movement.

Without this muscular and articular nervous association in joints, there could be no intimation, by the internal parts, of their exhausted function; and there cannot be any doubt that it is when this period of functional exhaustion in the internal parts has been reached, and articular friction is nevertheless continued (notwithstanding the structural and functional prostration), that the mischief to the articular structures commences, and disease of the joints, as we term it, starts into existence.

Again, through the medium of this *muscular and cutaneous* nervous association, great security is given to the joint itself, by those muscles being made aware of the point of contact of any extraneous force. Their involuntary contraction instinctively makes the surrounding structures tense and rigid, and thus is brought about an improved defence against further temporary encroachments.

This same associated muscular and cutaneous nerve distribution is obviously of the highest importance in reference to the sense of touch, through a recognition induced by due pressure. In the absence of this peripheral, or cutaneous, and muscular harmony, through the medium of the common distribution of the same nerve, the refinements of the sense of touch could scarcely be acquired; for rude or heavy pressure is incompatible with minute

and delicate appreciation by touch. So that if I touch anything with my forefinger, the impression made upon the sensitive nerve conveys to the associated motor nerve from the same trunk the precise extent of pressure which is necessary for the recognition of the required force. In prehension, or grasping with the hand, the indication as to the completion of the necessary force to be employed must be the impression made upon the sensitive branches of the same nerve which supplies the muscles employed in the act of prehension or grasping. Thus the median nerves, with others, are employed in grasping and in the adaptation of the fingers to the palm of the hand. The limit of that grasping is the perfect recognition of the contact of the different parts of the hand to be pressed upon. Whether the body to be grasped be great or small, there must be an instinctive limit to the grasping effort. This can only be indicated to the muscular apparatus employed by the sensitive nerves. So that I say, in prehension, the indicator as to the completion of the necessary force to be employed must be the impression made upon the sensitive branches of the same nerve which supplies the muscles employed in the act of prehension or grasping.

So in standing, walking, or jumping. The contact of the skin of the foot with the ground intimates, in health, only the degree of muscular force necessary to sustain the erect posture in standing; to guage the use of only the required energy in walking; and to moderate the concussion of jumping. Without this normal, consentaneous muscular and sensitive function, precision of action would be lost, and unmeasured exercise of muscular force would be employed during the performance of any of these functions. The nice adjustment of muscular power must be an important thing, for we know that unexpected muscular force breaks the patella.

Now let us go to the bedside. Is it not indisputable that if we allow an inflamed knee-joint to lie upon a soft pillow, unconstrained by mechanical means, the muscles of the joint, stimulated to undue exertion, never allow the articular surfaces to be kept quietly in apposition without pressure upon each other?—thus explaining the fixed condition of the joint. The flexion, too, receives its explanation from the exalted muscular vigour. Thus the flexors —as I before remarked, the more powerful muscles—act unceasingly day and night, apparently without any rest, and especially declare their mischievous assiduity by the wakeful slumbers and disturbed sleep of the patient. This persistent action of the flexors slowly alters the relation of the articular surfaces of the bones to each other—more slowly indeed, but not less surely, than the hour-hand of that clock, which does not appear to move, but yet if I compare its present position with that which it occupied when I commenced this lecture, I discover that it has gone on marking the course of time.

Let us take the case of the knee-joint again. The surgeon sees the inflamed and swollen joint, lying on its outer side, and desires that it may be kept quiet upon the bed, or on a soft pillow. Whether he sees it again in a few hours or days, or in a week, the first thing that strikes him with regret, and perhaps surprise, is the changed position of the bones. He sees that the knee-joint is considerably more flexed than when he saw it last. On inquiry, he ascertains that neither the patient nor the nurse has disturbed it, and then he recognises that the stealthy and mischievous flexors have secretly effected this change in position. If you give the patient chloroform, and destroy muscular action for a time, the joint is then perfectly moveable by ordinary manual force; so that it is not the mechanical distension of the *interior* of the joint by fluid, or the incumbrance of the joint by the fluid or solid results of inflammation upon its *exterior*, which produces early rigidity or flexion; it is forced coaptation and flexion under the influence of muscular energy involuntarily exerted; for as soon as muscular contraction is destroyed by chloroform, the fixed and flexed conditions of the joint are for the time entirely lost. And here I may state, that over and over again I have seen the mischievous and destructive effect of this increasing state of flexion of the joint, and not seldom, I may say, resulting from procrastination, or inexcusable want of courage on the part of the surgeon to rectify it, or from his yielding too easily to the importunities of the patient or his friends, in order to avoid giving pain

to the patient. From the time when the limb begins to be flexed starts the mischief, which by-and-by cannot be controlled.

It seems to me that a great mistake is made on the part of professional men, when they see an inflamed knee or other joint beginning to be contracted or flexed, not to correct it at once. I believe that their duty lies in preventing the increase of the flexion, and so averting the condition which, in some cases at all events, leads to the necessity for amputation.

This sole explanation of the cause of the fixed and flexed joints applies particularly to recently inflamed joints, before the deposition of new and comparatively solid material has taken place around and within the articulation, such as usually occurs in cases of long continued or chronic inflammation of joints, when, no doubt, the contraction and fixed condition depend partly on the incumbrance of the joints by the new material, and hence the deformity of the joint at that period cannot even under the influence of chloroform be entirely removed.

And here let me just refer again to this constantly flexed state of an inflamed joint. Take an inflamed hip-joint: I venture to say that no gentleman here ever saw an inflamed hip-joint with the leg extended. It is uniformly bent, and, also, as a rule, slightly adducted, the cause of which I shall have occasion to refer to at another time. In the case of the knee-joint, when inflamed it is always flexed; but, curiously enough, the malingerer, willing to deceive and to impose, almost always endeavours to indicate his long continued and extreme suffering by fully extending the leg. But this extended position displays the imposition, for an inflamed joint is never straight, but always flexed, the degree of that flexed condition depending upon the intensity or the long duration of the mischief. Did any person ever see an inflamed ankle-joint, permitted to pursue its own course, remain nearly at a right angle with the foot? The heel is always raised by the gastrocnemii, and the toes pointed downwards. So with an inflamed elbow-joint; it is always flexed, never extended. But when we come to the wrist-joint, we find that although the muscular power associated with it is very great, yet in consequence of the extending and flexing forces being pretty equally balanced, we seldom have much flexion of the hand when the wrist-joint is inflamed. This is in strict accordance with the law which I have endeavored to place before you—the quality of muscular force not causing much disturbance of the bones, but simply maintaining the joint in a quiet, but fixed condition. So again with the fingers. Who ever saw a finger with an inflamed joint of any duration in which the finger was perfectly straight? Such a thing was never seen; the inflamed joint is always more or less bent under the influence of the flexors. I think we may in the hand recognise the proportionate condition of flexion associated with diseased joints, in reference to a distribution of nerves, on which I shall have occasion to dwell at our next meeting. The forefinger, for example, when inflamed, is generally not so much flexed as the others, because it has an added muscle, the extensor indicis, which counteracts the tendency to become flexed. With regard to the thumb, I should say, as a rule, it is not proportionately so much flexed towards the palm when inflamed as the middle or ring finger. The explanation lies in this: that there is a greater balance of power in the muscles of the thumb, the flexors and extensors, as compared with the corresponding muscles of the fingers. This will explain the extreme flexed condition of some of the fingers, the slight degree of flexion of the forefinger, and the nearly straight condition, as frequently observed, in an inflamed joint of the thumb.—*Lancet, July* 20, 1861, *p.* 57.

36.—ON LOOSE CARTILAGES IN THE KNEE-JOINT.

By W. Joseph Square, Esq., Surgeon to the Devon and East Cornwall Hospital, &c.

[Mr. Syme and others have been accustomed for some time to remove these bodies from the joints by subcutaneous operations. Mr. Square and his col-

league, Mr. Whipple, give us the following interesting cases in confirmation of this practice.]

Case 1.—James Ellis, aged 33 years, a farm servant, of healthy constitution, was admitted into the South Devon and East Cornwall Hospital on the 3rd of December, 1856. He has been lame 18 years, and very decidedly so during the last twelvemonth. He has felt two moveable bodies in his right knee-joint for fourteen years. With the movement of the joint they glide from one part to another with extreme mobility, and thereby cause great distress and sudden lameness. The joint is often distended with synovia.

Operation, Dec. 8th. I first fixed the cartilages firmly at the lower and outer side of the joint, and entrusted them to an assistant. Two subcutaneous separations of the cellular tissue were then made with a long tenotomy knife (two inches in length,) at two separate points of puncture.

From the lower puncture the point of the knife was directed upon the largest cartilage, and the synovial membrane freely incised upon it. The cartilages were then easily slid along the subcutaneous tract prepared for their reception. They were fixed at the end of this tract with a pad of lint, adhesive plaster, and bandage. A straight splint was applied across the back of the limb. The limb was placed at an angle of forty-five degrees, and cold water dressing applied.

Dec. 22nd. He has experienced no pain, nor has there been any inflammatory action, or synovial effusion. The cartilages were therefore excised.

Dec. 30th. Discharged, cured, with slight stiffness of the joint.

Case 2.—Robert Windsor, aged 27 years, farm servant, healthy constitution. A loose cartilage the size of a walnut is readily felt in the left knee-joint. It gives rise to the ordinary symptoms.

April 28th, 1858. The cartilage being firmly fixed by Mr. Fox at the upper and outer angle of the joint, I punctured the skin with a two-inch tenotomy knife, two inches above the cartilage; by a semi-circular sweep separated the cellular tissue from the subjacent fascia, then cut upon the cartilage at right angles to it, freely dividing the joint capsule. The body now eluded Mr. Fox's grasp, and slipped back into the joint. With difficulty I managed to bring it again to the opening in the synovial membrane, and pressed and lifted it into the before described subcutaneous tract. The patient was treated exactly as detailed in Case 1.

May 4th. No pain or inconvenience has arisen; cartilage excised.

May 6th. Discharged, cured; walks well.

Case 3.—Edward Mitchell, aged 30 years, farmer, healthy constitution.

May 13th, 1858. Three small foreign bodies are distinguishable in the right knee-joint, the smallest is not as large as a garden pea. By slight pressure they glide with amazing swiftness into various parts of the joint, and can only be fixed with moderate certainty at its inner and lower part, just in front of the internal lateral ligament. There is no thickening of the synovial membrane, nor effusion into the joint. Two years ago he first discovered a loose cartilage in the joint, but he had then been already lame for some months. Two months since he discovered a second, and a fortnight ago a third foreign body. His conviction is that the first discovered cartilage was larger than either now in the joint, whence the inference that the two last discovered were produced by the disintegration of the first.

Operation, May 15th. He sat in a chair as the most ready mode of fixing the cartilages, which was effected with difficulty at the lower and outer part of the joint. The puncture and subcutaneous separation being made as in Case 2, I endeavoured to push the cartilages out of the joint, instead of which they slipped back into its cavity. The patient now after much trouble and manipulation, presented one of the cartilages at the incision of the synovial membrane, and I slid it into its bed. Ten minutes of continuous manœuvre were consumed in getting a second cartilage into the subcutaneous tract, but the third could not be found. The cartilages were retained *in situ*, and the patient treated as before described.

May 21st. The operation has caused no pain or inconvenience. Cartilages excised.

May 28th. Returned home. Motion of joint weak but natural.

Case 4.—Francis Warren, aged 20 years, farm labourer, robust constitution. Two months ago he was suddenly attacked with severe pain in the right knee-joint, with considerable tumefaction. Under surgical care he gradually recovered, but has never entirely lost pain in the part. Three months ago he discovered the existence of the loose cartilage. It is now about the size of an almond in its shell. It is extremely mobile and difficult to fix.

Operation, Jan. 25th. I performed the operation of subcutaneous section, with the intent of embedding the cartilage in the cellular tissues of the thigh, but from the excessive mobility of the cartilage, I failed to accomplish my object. The patient was treated precisely as if the operation had succeeded. No inflammatory action resulted.

Feb. 8th. Operation repeated, with this difference, that the subcutaneous operation was effected over the inner surface of the tibia. I now succeeded well, and lodged the cartilage in the space prepared for it. He was treated in all respects as before described.

Feb. 15th. No inflammatory excitement or uneasiness has occurred. Cartilage excised. Discharged cured.

Case 5.—William Davey, aged 24 years, a healthy farm labourer. His right knee has been weak for some years. He discovered the presence of a loose cartilage nine weeks since. It gives great pain in walking. It is flat, and about the size of an almond.

May 11th, 1860. By subcutaneous incision the cartilage was removed from the joint, and embedded in the cellular tissue, on the inside of the head of the tibia. By pad, adhesive plaster, and straight-back splint, it was retained *in situ*.

May 11th. Cartilage excised.

May 25th. Discharged cured. No inflammatory action has followed the operation. Walks well.

Case 6. William Davey was again admitted into the South Devon and East Cornwall Hospital on Feb. 5th, 1861. For some weeks he has felt another loose cartilage in the same knee-joint.

Feb. 8th. I endeavoured to perform the usual subcutaneous operation, but failed to dislodge the cartilage. He was treated in the manner already described, and no bad results occurred.

Feb. 22nd. Operation repeated, and again without success. He was treated as before, and with the like immunity from inflammatory action.

March 5th. I discharged him from the hospital, being unwilling to wound the synovial membrane again so soon after two operations.

April 24th, 1861. William Davey was again admitted into the hospital. On the 26th I again performed the operation of subcutaneous incision, but unsuccessfully. His after-treatment was as before.

May 2nd. Allowed to get out of bed. He has had no pain or inflammatory action in the joint since the operation.

May 20th. Discharged for re-admission.

Case 7.—John Ellacott, aged 40 years, a healthy farm servant, was admitted into the South Devon and East Cornwall Hospital, on the 8th of September, 1858, under Mr. Whipple. A loose cartilage, the size of a horse bean, is readily felt in the right knee-joint. He has the ordinary symptoms of the malady. The joint contains no fluid, and its capsule is not thickened. He discovered the cartilage about a year since. It has not increased in size. He never had an accident.

Sept. 10th. The cartilage slipped into the cellular tissue of the thigh, by the ordinary subcutaneous plan.

Sept. 14th. Cartilage excised. It was much smaller than any of the others. No inflammatory symptoms followed the operation. Cured.

Case 8.—Medland, a miner, of good constitution, was operated upon by Mr. Whipple, in 1859. The patient had recognized the presence of a large cartilage for about three years. His symptoms were severe, occasionally he suffered excessive pain, and could not move from this cause, and from faint-

ness, until he had by manipulation altered the position of the cartilage. The operation was performed after the usual manner, and the cartilage was excised on the eighth day. No inflammation followed. He walked four miles the day after the excision of the cartilage, six the next day, and four days after resumed his labour.

Case 9.—Joseph Badcock, aged 39, game-keeper, healthy constitution, admitted into the hospital under the care of Mr. Whipple, April 18th, 1861. He has felt the cartilage (which is now very large) for fourteen years. He has the ordinary symptoms, and is almost *hors de combat* as regards active exertion. There is considerable synovial effusion, and thickening of the joint capsule.

April 19th. Mr. Whipple operated successfully. He displaced the cartilage from the joint and lodged it in the cellular tissue. Patient treated in the ordinary way.

May 5th. Cartilage excised. It was found to be of very irregular outline; its surface nodulated.

May 10th. Patient discharged. He has had no pain, but there is some synovial effusion, and lameness.

June 13th. Is much improved, walks better. Synovial effusion still exists, but is diminished.

Thus, in the treatment of these nine cases of loose cartilage, the knee-joint has been opened by subcutaneous incision thirteen times. It is worthy of remark that neither pain, inflammatory action, nor any serious symptom has in any one instance arisen. The steps of the operation have been alike, the after-treatment the same. A small pad has been placed along the tract of the wound, and upon its orifice, to press the surfaces of the tract together, to prevent hemorrhage, and to retain the cartilage *in situ*. A straight splint has been applied along the back of the limb, to avoid movement of the articular surfaces. The patient has been confined to bed, with the limb at an angle of 45°, and in the majority of the cases, but not in all, water dressing has been employed.

It would be absurd to say that, by the old method of getting rid of these bodies the knee-joint could not be opened with impunity thirteen times successively; but, it is not too much to state that this is far, very far from the ordinary experience of surgeons. In common justice to humanity, I must urge the adoption of the subcutaneous operation devised by Professor Syme, and performed with such marked success by Mr. Whipple and myself, in the cases now reported. I do not relate them on account of their novelty, but I do so, to assist in bringing a safe operation into repute, and to rescue it from that oblivion under which it appears to be in danger of sinking, when we find that such an accomplished surgeon as Mr. Fergusson still prefers the ancient plan. Surgeons often regard an innovation upon the beaten paths of practice with suspicion or disfavour, and are content with the fair amount of success attached to any established operation. Thus valuable advancements in surgery become obsolete, or having fallen into disuse, are exhumed by later observers, and sometimes claimed as new discoveries. Look at the history of resection of the knee-joint, now an established operation—when first introduced by Park and the Moreaus, it was speedily lost sight of because its first efforts were not strikingly successful. A very few years since, this operation was revived by Mr. Fergusson, whose skill and love for conservative surgery have rarely been equalled. May not, therefore, this truly scientific application of subcutaneous surgery become obsolete, if its early success is not duly impressed upon the profession. Moreover, when a new and important operation based upon right principles has been introduced, it is surely incumbent upon its originator, as well as its first promoters, to chronicle all cases treated by it, in order that a proper estimate of its value may be entertained, and its claim to general adoption tested.

It is curious, perhaps instructive, to note, that all these cases of loose cartilage in the knee-joint, occurred in healthy countrymen, and in every instance but one, in those engaged in farm operations. All were young men, or at any rate, young when the disease was detected.

Beclard. Andral, Rokitansky, Syme, Adams, and other pathologists, have written and propounded their views upon the formation of these bodies.

Without any pretension to long and patient investigation, I would state my conviction, that a very large proportion of these loose bodies are the result of local injury. That by accidental violence, small or larger portions of the normal cartilaginous structure are detached. How many patients refer to some particular injury as the antecedent and cause of the disease.

It is pretty certain that in the 3rd case, that of Mitchell, a large cartilage was resolved by the ordinary friction of the joint into three smaller ones. Do these bodies grow? James Ellis (Case 1.) stated positively to me that the one in his knee had done so. Indeed, as they possess independent vitality, there is no reason why they should not increase in size as well as be sustained in life. Their independent vitality does appear to me a very curious fact. Is it maintained by the chemico vital phenomena of endosmose and exosmose? I have never subjected them to microscopic examination, but some of them are enveloped in a fine membrane. Their appearance is peculiar, waxy, irregular and as if worm eaten. They usually consist of bone and cartilage in indefinite proportions.—*London Med. Review, Oct.*, 1861, *p.* 163.

37.—Excision of the Knee-Joint:—Recovery with a False Joint, and a Useful Limb.—[A patient was brought into the operating theatre at King's College Hospital a short time since, on whom Mr. Fergusson had performed excision five years before.]

The excision was performed on March 1, 1856. The patella was not removed, as it was only superficially affected; it was scraped and then replaced. The disease was found to be very extensive, especially of the tibia; and after removing two slices from that bone, Mr. Fergusson removed also a quantity of dead bone by the scoop. The recovery was for a time rather sluggish, but ultimately she got quite well, and was able to attend to her household duties, suffering no inconvenience whatever; she could easily stand on the leg, and could run up stairs or jump off a chair as if she had had no disease and no operation. Up to within five months her health had been quite good. The great feature of interest in this case was, that there was considerable mobility of the joint, which remained to the present time, and which Mr. Fergusson demonstrated to those present. It was the first case Mr. Fergusson had seen in which this had happened. It put an end to "the factious opposition of persons who knew nothing on the subject, and yet declared that no good result could be obtained unless anchylosis occurred, and that this was so rare, that the operation must therefore be considered unadvisable." Since seeing the good result of this case, he (Mr. Fergusson) had been indifferent as to whether the result was osseous or fibrous union.—*Med. Times and Gazette, June* 8, 1861, *p.* 601.

38.—TREATMENT OF HOUSEMAID'S KNEE BY THE THREAD SUTURE.

By Frederick C. Skey, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

Mr. Skey's practice is to pass through the tumour a thickish thread, which is allowed to remain in. This sets up inflammatory action, known by a little redness around the entrance of the thread, and the swelling either subsides altogether, or what is more common, an abscess forms, which is opened, and the cavity becomes obliterated. In these three patients this treatment was followed out, and suppuration took place in all, with the result of cure. In one—the girl of sixteen—erysipelas was contracted in the knee, and in the foot of the same leg, from a patient in the neighbouring bed. An abscess formed in the foot, which was opened, and the erysipelas is disappearing under the use of quinine internally.

Mr. Skey applies the seton to all forms of housemaid's knee. He thinks it is perhaps better suited, however, to the hard and indurated bursæ.

There are some examples of this disease, wherein the walls of the cyst have become so thick and solid that no plan of treatment short of actual removal will prove of any avail. We have seen Mr. Fergusson, at King's College Hospital, dissect them out, under such circumstances, with good results. And we can call to mind an instance that came under our notice some months back, at University College Hospital, under Mr. Erichsen's care, of a girl who had a bursal tumour of this character wholly removed.

In October last, Mr. Quain had a girl, aged 19 years, under his care in the same hospital, in whose left knee was a fluctuating bursal tumour, of the size of a small orange. This was treated by a thread seton, with the result of causing evacuation of its contents, mild suppurative inflammation, and obliteration. In that instance the tumour had been present ten months, and arose from kneeling while scrubbing.—*Lancet, April* 27, 1861, *p.* 409.

39.—EXCISION OF A PORTION OF THE OS CALCIS FOR CARIES.

By Dr. JOHN K. BARTON, Surgeon to the Adelaide Hospital, and University Lecturer on Practical Anatomy, Dublin.

Maria Rothwell, a little girl from the county Dublin, eleven years of age, of remarkably fair, clear complexion, light hair, and blue eyes, was admitted into the Adelaide Hospital upon the 5th of last February. Six months previously she had hurt her left foot, and soon after perceived a small swelling near the heel, which after some time burst, and continued to discharge purulent matter ever since. The pain and inability to use the foot was increasing. Upon examination, the left heel, measured from one malleolus to the other, was two inches larger than the right; it was a peculiar oval or rather globular shape. Upon the inner side, about half way between the malleolus and extremity of the heel, were two sinuses, through which, when the probe was introduced, the bone could be distinctly felt, quite bare, and breaking down when pressure was made upon it by the probe. The integuments upon the inner side of the heel were of a livid, purplish colour; just at first there was some constitutional disturbance, but this soon disappeared when the heel was poulticed, and perfect rest was insisted on. The treatment adopted was both local and constitutional. The local consisted at first in the application of three leeches to the inner side of the heel, followed by a poultice and perfect rest of the part; this was succeeded by a diminution in the pain, and lessening of the swelling in the part; so that in about a fortnight the poultices were discontinued, and the heel was firmly strapped with soap plaster. The constitutional treatment consisted in a tablespoonful of cod-liver oil, with ten drops of the muriated tincture of iron, twice daily, and a milk and meat diet. This treatment had so far succeeded, that by the middle of March, her general health was greatly improved; the sinuses still remained open, but the discharge was very small, and there was very little pain in the foot. She was accordingly sent back to the country, with directions how to pursue the treatment, and not to use her left foot at all; and to present herself again in two or three weeks. Before the end of this time, however, she returned to the hospital much worse, having, as she confessed, used the foot as before, although cautioned not to do so. The whole of the posterior half of the foot was now much swollen. Through the sinuses, as before, the bone could be felt, but to a still greater extent. There was considerable constitutional disturbance. Perfect rest and poulticing reduced the inflammation round the diseased bone; but it soon became manifest that the caries was extending. An abscess formed beside the tendo-achilles, from which, when opened, a sinus was found to extend to the back of the calcis, so that the internal and posterior surfaces of the bone were engaged at least. Finding that rest, careful strapping, iodine locally, with iron and cod-liver oil internally, were not followed by the improvement now which they produced at first, and as

there was every likelihood, that as the disease was not decreasing, it would soon engage the other bones of the tarsus, I determined to endeavor to save the foot by removing a part, or the whole of the os calcis. The latter alternative to be adopted, if, when the diseased part was laid bare, it became plain that the entire, or nearly the entire of the bone was engaged. From the position of the sinuses, and from the evidence given by the probe, however, I was led to hope that the diseased part of the bone was so situated that it could be sawn off from the rest.

Upon the 17th of April, the child was placed on the operating table, and a few drops of chloroform in a towel, in the usual manner, held before her face; she immediately, however, resisted this very much, and when, partly by persuasion, partly by a certain degree of force, she was induced to take one or two inspirations of the vapor, she immediately became pale, and her pulse fell alarmingly; it was, in consequence, immediately discontinued, and I proceeded without it. First, a semilunar incision about four inches in length, connecting the sinuses upon the inner surface of the heel was made, laying bare the bone, which when the flaps were dissected up a little, could be felt very rough and crumbling, upon this its inner surface. The incision being prolonged backwards to the tendo-achilles, the bone was felt to be diseased above its insertion; it was, therefore, divided, and the whole cap of the heel dissected from the bone as quickly as possible, until the outside of the bone was reached, where there was no appearance of disease. Now the inner surface was again examined, and I found it necessary to dissect an anterior, or inner flap, from this surface up to the malleolus, the knife being kept very close to the bone, the posterior tibial artery being felt all the time beating in the flap, the back of the knife being towards it. When this much was accomplished, my colleagues who were present, agreed with me that a section of the calcis might be made, which would remove the diseased portion. A Butcher's saw was, consequently, placed upon the outside of the bone, and carried quickly in a direction upwards and inwards: by this the posterior half of the bone was removed. As soon as this was accomplished, I carefully examined the state of the remaining portion of the upper and inner part; it felt soft, so, with a strong gouge, I freely removed every suspicious part. The flaps were then simply laid together, supported by a few broad strips of plaster, wet lint laid over this, and the foot placed upon the outside. The child being removed to bed, got an opiate, and passed a quiet day.

An examination of the portion of the bone removed, showed the outer side quite hard and healthy, the inner soft and carious; while at the posterior part, just above the insertion of the tendo-achilles, there was a necrosed piece about the size of a large pea, surrounded by carious bone; this necrosed piece was hard, quite unlike the soft disintegrating bone round it, but was firmly united to the bone below it. The presence of this piece of dead bone seemed to augur well for the result of the operation, as this source of irritation had been completely removed. The little patient went on very favourably; the constitutional disturbance was trifling, but the wound only united partially by the first intention, and discharged an unhealthy serous pus for some time. It was kept firmly supported with strong adhesive plaster, and occasionally the granulating surface freely touched with sulphate of copper and nitrate of silver. In about a fortnight the line of incision was nearly healed, but a surface, about the size of half-a-crown, remained open at the back of the heel, while just above the malleolus a little abscess formed which was opened, but no diseased bone could be discerned here. The cap of the thick integument of the heel, which, immediately after the operation, seemed so much in the way and so loose, was now contracted firmly to the parts beneath, not appearing in the least too large, and making an excellent heel. The cicatrization of the parts which were granulating took place very slowly. This was attributable to the state of the constitution, which was markedly strumous, but up to the present time, nearly three months since the operation, there has been no return of disease, either in the remainder of the calcis or in any other of the bones of the foot. The little girl only left the hos-

pital on the 5th of this month, her foot presenting a very good shape and firm heel, upon which, however, she has not yet ventured to rest much of her weight, running about with the aid of a crutch or a stick. She can bear pressure upon any part of the heel or foot without the slightest pain. She has as yet but slight motion of the ankle-joint.

In this case we may infer, as far as the time elapsed since the operation permits us to judge of its effects, that the removal of a portion of the os calcis has saved the remainder of the foot. The disease was manifestly extending, and if so, must have necessitated finally the removal of the whole foot. Should the disease return at any future time, the patient will not be in a more unfavourable state for an amputation than she was at first. The result of the case so far, seems to me to encourage us in the attempt to give the patient, when youth and a tolerable constitution are on our side, a chance of preserving a limb, by the operation of excision of the carious bone.—*Dublin Hospital Gazette, July* 15, 1861, *p.* 215.

40.—*Pirogoff's Operation.*—Case under the care of PROFESSOR FERGUSSON, at King's College Hospital.—The patient, a boy, had suffered from diseased foot for some length of time. He was placed under the influence of chloroform. Mr. Fergusson made a section across the sole of the foot, and sawed through the calcaneum from below upwards, previously to disarticulating the joint. Mr. Fergusson said he considered this a decided improvement in simplifying the operation. He then made a transverse section over the dorsum of the foot, and disarticulated the tarsus. Finding the internal malleolus of the tibia a little projecting, he sliced a small portion more off the calcaneum to receive it; he preferred doing so to removing the malleolus. He found the external malleolus required humouring in a similar manner. Mr. Fergusson said, sloughing was advanced as an objection to this operation: no doubt it does occur, but Mr. Fergusson did not consider it a valid objection. He said, Mr. Syme was the first to introduce removal of foot after this mode; previously amputation being always resorted to, and sometimes very high-up. No doubt, Mr. Fergusson said, Pirogoff derived his idea from Syme's operation. Both Mr. Pirrie's and Mr. Watson's, of Glasgow, were modifications of this operation. We believe Mr. Hancock has performed a very clever mode of removal of foot.—*Med. Circular, July* 24, 1861, *p.* 64.

41.—ON DISLOCATIONS OF THE SHOULDER-JOINT.

By Dr. N. R. SMITH, Professor of Surgery in the University of Maryland.

[Dr. Smith has for many years practised the following method of reducing dislocated shoulder, and has found it to be most efficient].

Extension, counter-extension, and manipulation, constitute the mechanism of the operation of reduction. Counter-extension is the mere fixing, or rendering firm and immovable, the scapula. The head of the humerus alone requires to be relatively moved. The immobility of the scapula effected by counter-extensions, is obviously of essential importance. All the movements of the head of the bone, effected in the effort to reduce the same, would merely drag or thrust the scapula loosely about, without changing the relative positions of the head and glenoid.

To effect the immobility of the scapula is the real difficulty in the reduction of this dislocation. In many of the methods practised, the bands employed for this purpose oppose the return of the head of the bone to its place. By some a slit is made in a broad band of stout muslin, the arm carried through it, and one margin of the slit brought into the axilla and the other against the acromion. The tails of the band are carried, one across the breast, the other across the back, horizontally, and are secured to the wall. The lower margin of the slit supports effectually the lower portion of the scapula, but the support of the upper, which is by far the most important, is not

well effected. The margin of the band cannot be expected to remain opposed to the thin edge of the acromion. It will either glide over the top of it, or fall beneath it into the depression caused by the dislocation. In the former case it will not support the scapula above, but will allow it to be drawn outward and downward. In the latter case it will, by occupying the cavity to which the head of the bone is to be returned, hinder the reduction.

In effecting counter-extensions, it is undoubtedly, in most instances, expedient to apply our resisting bands as directly as possible to the bone from which the other is dislocated. But the difficulty in this case is to effect it without defeating the object in the manner indicated above.

On noticing the mechanical relations of the scapulæ, with their apparatus of muscles, to each other, it occurred to me to make counter-extension from the opposite wrist, an expedient directly at variance with the commonly received principle, because as remotely as possible from the bone to be supported. There are exceptions to most rules, and I shall endeavor to establish this as one.

Let us observe the continuity of ligament, bone, and tendon by which the two scapulæ are bound together and made mechanically dependent upon each other. Anteriorly the two acromion processes are bound together by an unyielding chain of bone and ligament. The two clavicles, the sternum, and the interclavicular ligament chiefly constitutes this bond of union. The fibrous-resistent ligaments in this chain are not capable of being stretched. If traction be made from opposite wrists, the two acromion processes, thus tied together, are not capable of being drawn asunder to the extent of half an inch.

Posteriorly the continuity of resisting parts is almost as perfect. The broad expansions of the scapulæ approach each other not remotely, and they are bound to each other by the interposition of the ligamentum nuchæ, and indirectly by muscles and tendons attached to the spine. Traction from the two extremities will not therefore separate the scapulæ to any considerable extent.

The superior angles of the scapulæ indirectly support each other through the medium of the cervical portion of the spine. The levator muscles, arising from the superior angles of the scapulæ, pass upward to be inserted into the tranverse processes.

Now the object of counter-extension is to prevent the yielding of the scapulæ to the tractive force exerted upon the dislocated member. Nothing does this so effectually as the fixing of the opposite scapulæ by counter-extending from the wrist. Not only is the scapulæ thus sustained, but the spine erected and prevented from yielding to the tractive force, and becoming curved to the injured side, as invariably happens when counter-extension is chiefly made from the axilla, as is usual. Let the experiment be made by allowing a person to incurvate the spine laterally, and then making traction horizontally from the two wrists. The spine will be immediately erected and the two scapulæ will at once assume symmetrical positions. And so in dislocation, the symmetry of the two sides tends at once to be restored. The head and spine are elevated, the two scapulæ firmly sustained, and effect is given, botht o the tractive force, and to the necessary manipulation.

In some of the first cases in which I employed this method, I directed simply traction from the two wrists, and I am not now confident that this is not the best method. I placed the patient in a chair and directed two strong persons to make steady horizontal traction from the two wrists. As soon as the spasmodic resistance of the muscle was overcome, the head of the bone was disengaged, and the muscles which help us in such cases, suddenly lifted the head into its place. It will be observed that this method causes no appreciable pain, but rather relieves the suffering of the patient caused by the pressure of the head of the humerus.

It is now some twenty years since I commenced the use of this method; I am not, therefore, premature in expressing my confidence in it, and recommending it to the profession.

In cases in which, from unusual muscular development, or the age of the dislocation, much resistance is expected, I have modified the application of the counter-extension thus:—

I place the patient in a chair, sitting a little on one side of it, so as to allow room on the side of the injury for the operator's foot. I then pass a piece of stout muslin, folded, around the chest and under the axilla of the injured side. The tails of it I carry horizontally to the opposite side, one in front, the other behind, and extending the arm horizontally, bandage them firmly to the wrist of the sound side, leaving the ends projecting, to be well secured to the wall, or other unyielding substance.

I then pass an ordinary roller over the top of the injured shoulder, and back and forth, twice under the muslin band, to prevent its slipping down. Then I continue the same roller under the bottom of the chair and over the shoulder, three or four times. This helps to give steadiness to the scapula, and especially to prevent the involuntary rising of the patient from the chair, or the tilting of the scapula upward, when it is necessary to make the manipulation of which I am to speak.

I now attach the extending band to the wrist of the injured side. I am aware that this is counter to the practice of many very eminent surgeons, especially Sir A. Cooper. My reasons are these: First, the wrist furnishes, as we may say, a very convenient handle to the arm. The band employed, if properly attached, does not slip, nor lacerate the integuments. In cases in which no great resistance is expected, it can be conveniently grasped by the hands only of one or two persons. In the next place, this method gives much more mechanical advantage in the way of leverage, in executing the final manipulations by which the head of the bone is thrown into place. It also removes the bands and hands of the assistants further from the patient's chest, and gives the surgeon more free access to the shoulder and more command of the arm.

The principal argument in favour of attaching the extending bands above the elbow is the relaxing, by this method, of the biceps muscle, which, by the complete extension of the forearm, is rendered tense. This disadvantage in my opinion is more than counterbalanced by the advantages mentioned above. The biceps is not capable of insuperable resistance; besides, a certain tension of its tendon, where bound to the head and neck of the bone, is favourable to the reduction, and it is partly the action of this muscle that finally, with a sudden shock, restores the bone to its place. Besides, when the bands are applied above the elbow, the forearm being at right angles, from the form of the arm, if any considerable traction is made, they will invariably slip over the elbow, make very painful pressure on the sensitive parts in the bend of the elbow, and not only cause great suffering, but extend the forearm and thus defeat the object in view, so that the advantage sought is lost, and many disadvantages encountered.

I first apply a wet roller to the wrist, and then attach a muslin band by the clove hitch. Next I direct the extension to be made by two persons, at first outward and a little downward, gradually raising the arm to the horizontal direction, and finally a little above it. The extension must be made gently and steadily, gradually increasing the force, so as not to provoke the muscles to spasmodic resistance. As no pain is created by the force thus employed, it may be continued for a considerable time. The muscles, which at first resist, become fatigued and finally relaxed, and, in a large majority of instances of recent luxation, the head will slip into place without resort to any species of manipulation. I would even continue this traction, where much resistance is encountered, for a quarter of an hour before modifying the force; but, in case the object is not then effected, let the surgeon place his foot on the margin of the chair, and his knee in the axilla. Then let the assistants raise their line of traction above the horizontal as much as possible, and continue it for a moment. The surgeon should then direct that the arm be, by a sudden movement, carried downward, while, by extending his foot, he elevates the knee in the axilla. He aids the assistants in this by grasping the arm near the elbow and using it as a lever. If the first effort is not successful, repeat it.

Sometimes I place on the knee a ball made by rolling up a bandage, but

it is not important. The surgeon can thrust his knee into the axilla so as to avoid much pressure on the marginal muscles of the axilla.

I generally direct those who make the traction to sway the limb horizontally backward and forward, and, grasping it with my hands, at times rotate it a little on its axis, thereby contributing to the disengagement of the head of the bone.

Where the consecutive displacement under the coracoid has occurred, the procedure is nearly the same, except that I make the traction a little more in the direction backward and upward, so as to disengage the head from under the process.

The dislocation upon the dorsum scapulæ, as I have before remarked, I have never seen but in three instances, and only one of those was a recent case justifying the attempt at reduction. It had been dislocated five days, and had resisted an attempt at reduction in the hands of others. The subject was a female, and I could obtain no very satisfactory account of the mechanical mode of the injury. It was easily recognized, there being a tumour beneath the spine of the scapula, as Sir A. Cooper describes, of the size of the hemisphere of a small orange, the limb being shortened and thrown forward. A depression existed under the anterior margin of the acromion. I failed to effect the reduction in the method usually recommended, and which I had deemed the best. I did not then practise extension from the opposite wrist. I made it by carrying the arm through a slit in the sheet. The extension was effected from the wrist, and when continued for some time, I sought to throw the head into place by manipulation—that is, I placed my knee against the back of the neck of the bone, and, swinging the arm backward, endeavoured to prize the head into its place. I repeated the effort several times, but with no satisfactory result.

I then carried a band over the front of the shoulder, one tail under the axilla, the other above it. These I united, carried them backward and inward obliquely, and secured them to the wall. Then I made traction strongly from the wrist almost directly forward. Without much difficulty I thus drew the head of the bone forward over the margin of the glenoid, and had the satisfaction to see it slip into its place.

Much has been said of late of the sufficiency of *manipulation*. to the exclusion of extension, in the treatment of dislocations. By employing new terms, and ignoring the precepts of old surgeons, claims to originality have been set up. Although the term is modern, the method of manipulation has been practised for centuries. It is, in my opinion, ridiculous to throw away the advantages derived from extension for the sake of magnifying the importance of a particular method of manipulation. Mothe, a long time ago, reduced the dislocation of the shoulder into the axilla by placing the patient in a supine position, and carrying the arm outward and finally upward, parallel with the head and neck, making traction at the same time.

It is well known that a recent dislocation into the axilla, may sometimes be reduced by placing the knee in the axilla, one hand being placed on the shoulder, and the other using the humerus as a lever.

The supine position of the patient on a table is a very convenient arrangement in all dislocations of the shoulder, and is well calculated to promote the efficiency of the method which I recommend. I am not sure that it is not the very best, especially when chloroform is employed.

It will be observed, then, that I advise the combination of traction and manipulation. It would be as absurd to reject traction in all cases of dislocation, as it would be to attempt the reduction, by manipulation alone, of the fractured femur, where overlapping and shortening had occurred.—*American Journal of Med. Science, July* 1861, *p.* 20.

42.—ON THE TREATMENT OF FRACTURE ON THE LOWER EXTREMITY OF THE RADIUS.

By Dr. ALEXANDER GORDON, Professor of Surgery, Queen's College, Belfast, &c.

[The radius is perhaps broken more frequently than any other bone. The fracture usually occurs at or about an inch above the carpal end.]

The fracture is usually transverse, and both fragments—that is, the upper end of the lower fragment, and the lower end of the upper fragment—are displaced forwards and inwards. Of this double displacement, I regard the inward as of secondary importance, because, if we counteract the forward displacement, the fragments, in resuming their natural position, will incline outwards; just in the same way as a straight or slightly curved piece of wood, when broken, with the fragments still in apposition, will resume its straight or slightly curved form when the deflection is counteracted. This fact, I think, has probably been too much overlooked hitherto in the treatment of this very common fracture; and hence too much importance has been attached to the necessity for counteracting the action of the pronator quadratus and other muscles by maintaining the hand in a state of adduction. Adduct the hand as we may, the deformity and its consequences will persist, if the forward displacement be not thoroughly rectified.

The practice hitherto adopted may be said briefly to have consisted of the application of a pad resting against the palmar surface of the forearm, extending as far as, or below, the lower end of the upper fragment and supported by a splint; of graduated compresses upon the posterior surface of the carpal fragment and carpus, and over these of a straight or pistol-shaped splint, and of adduction of the hand. Dupuytren applied the ordinary apparatus for fractures of the forearm, and relief chiefly upon an ulnar splint, to which the hand was held adducted, evidently overlooking the main displacement forwards.

Nelaton applies a compress, doubled at its lower end, so as to form a thick border, which he places immediately above the lower end of the upper fragment, and over that a splint, which extends beyond it, leaving between this splint and the lower end of the forearm a space which corresponds to the lower fragment; in fact, on the palmar surface of the lower fragment there is no pressure exerted. Posteriorly, however, he places across the posterior surface of the carpus and the carpal fragment of the radius two or three graduated compresses, of a wedge shape, with the base directed outwards, and over these a splint. Thus, according to Nelaton's plan, the lower end of the upper fragment is pushed backwards by the palmar compress and splint, whilst the carpus and carpal end of the lower fragment are pushed forwards, and inclined towards pronation.

A little reflection will show us, especially when we examine attentively the conformation of the forearm, when placed midway between pronation and supination, that any pad applied in front, and covered with a splint, will exercise its chief pressure along the centre of the forearm, and will not be sufficiently oblique to restore or maintain fully and effectually the concavity of the radius. The splint which I use has attached to its radial border a piece of wood, so bevelled that it fills up and fits accurately the natural concavity of the radius. The fracture renders the palmar surface of the radius convex, instead of being concave, its normal form. The surface of the attached piece of the splint is also convex, and covered with tow, a piece of spongio-piline (the best material), or some other soft substance. The splint being thus covered, is then applied to the forearm. The convexities of the splint and radius are mutually brought into apposition; about half an inch, or perhaps a little more, of the lower end of the radius, is unsupported, because of the alteration in its form. A thick pad, similar to that used by Nelaton, is now applied over the posterior surface of the carpal fragment of the radius and carpus, and over this a splint, extending from the upper and back parts of the forearm to the metacarpus.

The action is very simple. The radius, at the seat of fracture, is convex,

instead of being concave; the convexity of the splint is pressed against the convexity of the radius. The lower ends of the carpal fragment, the carpus and metacarpus being inclined unnaturally backwards, are pressed towards the palmar surface, acting as a lever, whose fulcrum is the convexity of the radius. The displacement forwards of the radius gradually yields; and if the pressure by the bandages be cautiously and continuously maintained, the convexity of the splint slowly and steadily forces the displaced fragments backwards, until at last the convexity of the splint rests in the concavity of the radius; and just as the inclination forwards of the fragments disappears, so does the displacements inwards; and so perfectly does the radius regain its normal form, that in most cases, it would be difficult to detect the seat of fracture. In one case, I tried in how short a time the splint would reduce the deformity under pressure from straps and buckles. The patient was a boy, 13 years of age, who "had sprained his wrist" nearly three weeks previously. The inclination forwards was even greater than usual. The splint was applied at ten o'clock a.m.; at eight o'clock p.m. of the same day, the straps were tightened by one of the resident pupils of the General Hospital: the patient in the mean time, made no complaint of pain; and on the following morning the radius had regained its shape perfectly. Yet I do not recommend that the correction of the deformity should be left to splint and pressure alone. I merely mention it as a fact repeatedly observed, that the splint if properly applied, and aided by a tight bandage, will, as it were, mould the radius upon it, and restore its natural form.

A very important question arises at this stage of the inquiry. Can the methods of treatment recommended by Dupuytren, Nelaton, Dr. R. W. Smith, and others, restore the natural form of the radius, or can they maintain it when it has been restored by the manipulation, until firm osseous union has taken place? I believe they cannot, and for this simple reason, they do not afford a sufficiently solid mechanical support effectually to counteract the action of the supinator radii longus, the adductors of the thumb, and the other muscles, when there is a tendency to displacement; and if they do not, are those disastrous consequences which too frequently follow the accident to be referred to the injury inflicted at the time of its occurrence; or are they the result of inflammatory action subsequently set up in consequence of alterations in the natural relation of the parts?

When the radius is restored to its natural form, and thus maintained, the "stimulus of imperfection" ceases, and, as in other fractures, very little serious inflammatory action follows. I have so often witnessed relief of the pain, which was very severe before the application of the splint, that I regard pain as indicative of displacement rather than of fracture. In some persons, especially those in whom the general health is not good at the time of the accident, the subsequent inflammatory action may be great, and the result of the primary injury. When once it has commenced, it may continue until it ends by impairing the functions of the limb permanently. Still my firm conviction at present is, that in most cases the inflammation is not of primary, but of secondary occurrence, and that it is evidently due to the non-restoration of the natural form of the radius. If we examine a case carefully by dissection, in which there has been a slight inclination forwards of the concavity of the radius, we shall see changes sufficient to account for it. Do not the same train of symptoms arise in cases of fracture of the fibula, in which, after the consolidation of the fracture, the weight of the body is thrown more inwards upon the tarsus than it was before the patient met with the accident?

I have before me six specimens of the radial fracture, and in none of them has the natural concavity of the radius been restored. Dr. R. W. Smith, in his admirable treatise on Dislocations and Fractures in the vicinity of Joints, says: "In not one of the twenty specimens which I have lately examined, has this object" (the restoration of the natural concavity of the radius) "been accomplished: in some it has been so far effected that the carpal surface looks directly downwards, but in none of them does the posterior surface exceed the anterior in length; in those which present the near-

est approach to the natural form of the bone, these surfaces are of equal length."

Now, what are these alterations which result from the changed position which the carpal surface of the radius occupies?

The inclination forwards of the upper end of the carpal fragment causes its carpal surface to look downwards, and slightly backwards, which is also further increased by the greater absorption of its posterior half, evidently showing that the movements of the carpus upon the carpal surface of the radius are directed more towards its posterior border than natural; or in other words, the centre of motion between the carpus and carpal surface of the radius is displaced backwards. Indeed, whilst the length of the palmar surface of the lower or carpal fragment seems to be little, if at all, altered, the posterior surface is shortened one or two lines by the progressive absorption of the posterior half of the carpal surface. One of the effects, then, of this accident is to wear away, if I may use the expression, the posterior half of the carpal surface, and cause shortening of the posterior surface, in some instances, by two lines at least; and leading to increased capability of extension, and diminished capability of flexion of the hand upon the forearm; also alteration in the line of action of the extensor muscles behind, of the flexors in front, and of all the other muscles which pass from the forearm to be inserted in the hand.

I have seen absorption of the vertical thickness of the scaphoid and semilunar bones. In one of the specimens of this accident which I have before me, the vertical thickness of the scaphoid and semilunar bones is unquestionably diminished; but as there is evidence of the presence of that morbid action to which the name of chronic rheumatic arthritis is given, the alteration may be as much the effect of this morbid process as of the accident. From the inclination inwards of the upper end of the lower fragment, the styloid process of the radius ascends, drawing with it the hand, and giving to the extremity the appearance as if the hand had suffered a slight partial dislocation outwards, causing projection of the lower end of the ulna, elongation of the internal lateral ligaments, and increased mobility of the carpal end of the ulna.

We may now perceive very plainly, from the changes in the natural relations of parts, and consequently in their actions, that the ligaments and sheaths of the tendons sustain a sort of chronic sprain, which, exciting inflammation, ultimately ends in greater or less impairment of the functions of the forearm and hand; that these occurrences are almost entirely referable to the displacement of the lower fragment of the radius; and that every mode of treatment which does not effectually restore the concavity of the radius must necessarily be followed by those distressing consequences which most surgeons have regarded as unavoidable and irremediable. That these distressing sequelæ may be almost entirely prevented, I have not the slightest doubt; but, if so, this can only be accomplished by a splint accurately fitting the concavity of the radius, and effectually maintaining it. The treatment of fracture of the lower end of the radius will then be as easy as that of any other fracture, and unattended with any serious consequences as regards the future usefulness of the limb. I have often been told by practitioners that they have efficiently treated fractures of the lower end of the radius by compresses in front and behind, covered by straight or pistol-shaped splints. To this I reply: That your patient has had a fracture of the lower end of the radius, I admit; that you have set the fracture, restoring the natural form of the bone, I also acknowledge; that compresses and splints have been applied is another fact; and also that the patient has regained the perfect use of the limb. All these facts I admit. But I cannot admit, from what I have seen of this accident, that compresses, supported by straight or pistol-shaped splints, are adequate to restore the concavity of the radius and maintain its natural form, if there be a tendency to displacement, because they are not adequate to the correction of the displacements; and, being mechanically imperfect, deformity of the radius and its disastrous consequences must result.

Every practitioner who has carefully observed cases of this accident, and treated them according to the most approved methods hitherto adopted, must, if possessed of ordinary candour, admit the existence of the serious consequences which too frequently attend it, and which have been faithfully described by Dr. R. W. Smith:—"The sequela of this injury is a source of great inconvenience to the patient, and of annoyance to the surgeon, who is often unjustly blamed for its occurrence; the practitioner will therefore act with prudence in warning his patient, at the commencement of the attendance, that stiffness of the wrist-joint, and an incapability of flexing the fingers, during a period of several months, are by no means unfrequent results of a fracture of the lower end of the radius." (p. 139.) It would be presuming too much to affirm at present, that these untoward circumstances can be entirely prevented. Still, I am bound to state, from considerable experience, that, under proper treatment, they are very rare; so rare, indeed, that their occurrence neither need be anticipated nor dreaded.

In the formation of the splint, there are several points which should be carefully observed. The breadth of its lower or carpal end should not exceed that of the forearm at the wrist; it should not project so much internally as to be on a level with the inner border of the forearm. On the radial side, it should project beyond the radius; and the bevelled piece, for filling up the concavity of the radius, should be attached half an inch, at least, internal to that border. By this arrangement, the ulnar side of the forearm sustains the pressure of the bandage or straps. The reverse is the case on the radial side. The radial border of the splint alone is pressed upon; this pressure forces the bevelled portion inwards upon the concavity of the radius and pushes it backwards, whilst, at the same time, the outer border of the radius is protected from pressure. If the ulnar margin of the splint projected beyond the ulnar border of the forearm, it would be pushed outwards when we applied the bandage; and the bevelled portion would be removed from the concavity of the radius, and it would be left unsupported. The upper end of the splint should be well hollowed to adapt itself accurately to the convexity of the upper part of the palmar surface of the forearm.

In this, as in other fractures, I prefer broad straps, with buckles, to the ordinary calico or linen bandage, as they are more easily removed, and less liable to loosen. Indeed, for some time past, in the treatment of fractures generally, I have entirely discarded the ordinary circular bandages, using in their stead broad straps, with buckles attached; and whether we regard the comfort of the patient, or the satisfaction which the straps afford to the surgeon, the advantages are so obvious, that calico or linen bandages cannot bear comparison with them.

Another point to which my attention has been particularly directed for some time, has especial reference to the diagnosis of injuries of the wrist and ankle-joints, and may be of some importance to the junior practitioner. Fractures of the lower end of the radius are usually caused by falling upon the upper part of the palmar surface of the hand, when the patient instinctively extends his arms, whilst falling, to prevent his face coming in contact with the ground. If, after falling in this manner, a patient complains of having sprained his wrist severely, and is immediately afterwards unable to move it, unless with great pain, it is the radius and not the wrist-joint that has suffered. I cannot bring to my recollection a single instance of severe pain caused in this manner. I do not mean to deny the possibility or probability of the wrist being sprained in this way; but I wish to impress upon the junior practitioner the importance of a maxim which I have laid down for my own guidance, and it is this:—When a patient comes to me and says, "I have sprained my wrist, and am unable to use my hand," I assume the injury sustained to be a fracture of the radius and not a sprain, until I have satisfied myself to the contrary. When a patient requires my assistance for a severe sprain of the ankle-joint, caused by the foot being twisted either inwards or outwards, I assume that he has fractured the fibula, and that the whole injury is a fracture and not a sprain, until, by the most careful examination, I have satisfied myself to the contrary. Sprain of the wrist, from falls upon the palm

of the hand, is a very rare accident. I do not remember having met with a single instance of it. Sprains of the ankle-joint, from twisting of the foot, I also regard as very rare accidents; whereas fractures of the radius and fibula are very common.—*Edinb. Med. Journal, May* 1861, *p.* 999.

43.—COMPLETE RESTORATION OF THE TIBIA BY THE PERIOSTEUM, AFTER ITS REMOVAL.

By M. Maisonneuve.

In the month of August, 1855, M. Maisonneuve was consulted for a young man, whose right leg was in a frightful condition. Its size was enormous, and deep ulceration was present over its entire surface, and it was but too obvious that the whole shaft of the tibia was in a state of necrosis. The patient stated that some two years before, while engaged in gymnastic exercises, he had met with a severe fall, that deep-seated aching pain in the leg had been the consequence, together with tumefaction of the limb, and subsequently abscesses, which had gradually brought about the present condition—viz., copious and fœtid suppuration, enormous increase of the size of the leg, extreme emaciation, hectic fever, &c. The parents, moreover, declared that all the resources of medicine had been exhausted, that several of the most eminent surgeons of Paris had unanimously recommended amputation, and that M. Velpeau, to whom, as a last resort, the matter was referred, had not only informed the family that the operation was indispensable, but further, that it was urgent, and that all hope of preserving the limb must be abandoned.

Despite these unfavourable opinions, emanating from such high authority, M. Maisonneuve, trusting in the regenerative power of the periosteum, proposed to extirpate the bone beneath that membrane, an operation which was consented to, and, in M. Maisonneuve's words, was performed as follows:—"The patient having been fully placed under the influence of chloroform, I performed along the entire length of the tibia, in an extent of about fourteen inches, a section penetrating to the bone through the periosteum, which was already thickened, and lined with a new, soft, and spongy osseous secretion. At each extremity of this long incision, I divided the parts transversely, so as to be able, by turning over the flaps on each side, fully to expose the seat of disease. I thus ascertained that the entire shaft of the tibia was mortified, the articular extremities alone remaining healthy. I then proceeded, without delay, to separate the dead bone from the adjacent structures, and succeeded, not without difficulty, in extricating it completely. The consequences of this long and arduous operation were of a truly remarkable simplicity. The traumatic fever was extremely moderate; the suppuration, hitherto copious and fœtid, was speedily replaced by a healthy puriform secretion, and, extraordinary as it may appear, the young patient was able, so early as the fortieth day, to rise and walk with crutches, as if he had merely been affected with simple fracture of the limb. An entirely new bone had been formed, and so complete was the restoration, that had I not preserved the shaft of the tibia, removed six weeks before, I myself could scarcely have credited the fact."

This bone, which M. Maisonneuve placed before the Academy, is twelve inches in length, fifteen lines in thickness at its upper extremity, and eleven lines in diameter at its lower end. The three surfaces are smooth and compact in the two lower thirds, and rugged and swollen in the remainder. The patient is now healthy and vigorous; the formerly diseased leg differs in no respect from the other, except by the scar, which is now the only memento of the formidable process above described; he can, run, leap, or exercise as if he had undergone no surgical operation whatever, and the most careful inspection fails in detecting which leg was the seat of the disease. This remarkable instance of restoration of bone suggests the query, how many patients may be found who will survive the operation?—*Jour. de Med.; and Dublin Hospital Gazette, June* 1, 1861, *p.* 174.

44.—ON ANTERIOR AND POSTERIOR CURVATURES OF THE SPINE.

By WILLIAM ADAMS, Esq., Surgeon to the Royal Orthopœdic and the great Northern Hospitals, &c.

These two classes of cases, viz., diseases and distortions, or deformities of the spine, were by the older Surgical authorities frequently considered as one and the same form of disease, but the wide separation between them, and their easy diagnosis—except, perhaps, in one form, viz., rotation of the lumbar vertebræ accompanied with severe pain, to which I shall hereafter especially allude—are too well known to render any observations necessary from me. I would however, remark that in modern times, the distinction between these two classes of cases, viz., diseases and distortions of the spine, has been drawn rather too broadly, inasmuch as it has long been customary for surgical authorities to describe the distortions or deformities of the spine as merely functional affections, arising from muscular debility, &c., and unaccompanied with structural changes except in severe cases of long standing—or what may be called the later stages of these affections—whereas I shall endeavor to prove to you that it is anatomically impossible for any distortion of the spine, such as lateral curvature, however slight, to exist without being accompanied with structural alterations proportionate to the extent and duration of the curvature; and when these structural alterations are considered in connexion with the constitutional conditions upon which they essentially depend in a large proportion of cases, though their immediate causes may be of a mechanical origin, such as the irregular distribution of weight, &c., it cannot be doubted that such affections may be ranked among the simpler forms of disease, with more reason than they can be placed in the same category with the simple contractions and deformities which take place at the moveable articulations of the extremities from debility, position, and other causes indirectly affecting the articulations; and in which we know the joint surfaces preserve their integrity in form and structure, though they may deviate in their relative positions to each other. I make these observations because, whilst I fully concur in the propriety of preserving for practical purposes, broad lines of distinction between these different affections. it is important pathologically that we should determine where such broad lines shade off into each other.

The spinal column, then, is liable to *distortion*, *i. e.* fixed deviation from the erect position, in various directions so as to produce external deformity. The distortion may take place in the *antero-posterior* direction, either as an exaggeration of the natural curves of the spine which become so much increased as to produce external deformity; or the natural curvatures of the spine in the antero-posterior direction may be reversed, especially in the dorsal and cervical regions, *i. e.* the dorsal curve may become depressed so as to project anteriorly, curving forwards instead of backwards; the cervical curve may project backwards instead of forwards; and the lumbar curve frequently become straight instead of curving anteriorly. Or distortion may occur as a lateral deviation, though I shall presently explain to you that the deformity described as *lateral curvature of the spine* does not depend upon a true lateral deviation, but rather upon a peculiar twisted, or contorted, condition of the spinal column, in which an external lateral deviation of the spinous processes generally, but not invariably, forms a conspicuous feature. In this affection, however, the bodies of the vertebræ always deviate laterally, even when the apices of the spinous processes—upon the external examination of which surgeons generally, but erroneously, depend as a test whether the spine is straight or not—remain unaltered in their relative position in the median line of the body. So far, then, as it relates to the altered position of the bodies of the vertebræ, the term lateral curvature is sufficiently correct for practical purposes, and the lateral direction of the deformity is rendered still more apparent by its effect in producing a prominence of one shoulder, or of one hip as it may occur in the dorsal or the lumbar regions, or of both shoulder and hip when it exists as a double curvature. I would

therefore propose to continue the use of the term *lateral curvature of the spine*, because it carries with it the idea of a lateral *distortion*, as distinguished from the sharp posterior, or *angular curvature* produced by destructive disease of the spine.

Before describing the ordinary form of *lateral curvature* of the spine, I would observe that deviations of the spine either in the antero-posterior, or the lateral directions, may co-exist with destructive disease of the spine, affecting the intervertebral cartilages and the bones, more especially in the early stage; or they may be consecutive to such diseases, and remain as permament distortions after the destructive disease has been completely arrested. For example, when destructive disease occurs in the dorsal region, as we frequently see it in children and sometimes in adults, the posterior convexity of the spine is very much increased, and there may be great difficulty in deciding upon the existence or non-existence of disease. At a later stage, however, a sharp, angular projection of one or more spinous processes takes place, and all doubt as to the existence of disease is removed; but occasionally it happens that several intervertebral cartilages are affected without caries of the bones, as exhibited in preparation in St. Thomas's Museum, No. E 20 and E 22, and in such cases no angular projection of the spinous processes would take place.

When destructive disease in the lower dorsal and upper lumbar vertebræ has become arrested with the ordinary result of angular projection backwards of three or four spinous processes, the spine becomes altered in form as a consecutive condition. in the middle dorsal region which is either depressed so as to form a straight line, or the natural direction of its curve may be reversed, and a concavity instead of a convexity may exist in the middle dorsal region.

Then, again, when destructive disease attacks the middle and lower lumbar vertebræ, it occasionally happens that the natural concavity in this region is considerably increased in the early stage of the affection. I saw a very remarkable instance of this kind in a young woman, who was admitted into the Orthopœdic Hospital under my late friend and colleague Mr. Lonsdale. The increase of the lumbar curve and hollowness of the loins was almost as considerable as we see it in the cases of anchylosis of the hip-joint at right angles after disease, or in cases of congenital dislocation of both hips; the spinal muscles on either side were very prominent and tense, and a deep depression in the median line existed between them. The nature of the case was obscure; destructive disease was not suspected, the usual symptoms being absent; and it was regarded as essentially a spasmodic or spastic muscular affection, and the propriety of dividing the muscles discussed. Mr. Lonsdale, however, applied a spinal instrument with crutches and a webbing band in front. In the course of a few months the increased lumbar curvature disappeared, and afterwards a posterior, or rather angular, projection of the spinous processes of the twelfth dorsal and the first and second lumbar vertebræ appeared. The disease ran a remarkably favourable course, the more severe symptoms usually met with when disease exists in this region never appearing, and the girl has remained in service; she still continues to wear a spinal support, and comes occasionally to the hospital. Casts from this case are preserved in the museum of the hospital.

Deviations of the spine in a lateral direction may also occur in the early stage of destructive disease, when it attacks the bodies of the vertebræ, or the intervertebral cartilages, laterally instead of their anterior aspects as usual; or in the later stages, it may exist as a consecutive curve, either above or below the angular projection, resulting from destructive disease in the lower cervical, or the lower dorsal and lumbar vertebræ.

Curvature of the spine, then, either in the anterior, posterior, or lateral direction, may co-exist with destructive disease of the spine in its early stage without any reliable symptoms or indications of such disease being present. It is as well you should bear this in mind, because, in the course of practice, cases will occasionally present themselves in which the diagnosis must be one of considerable difficulty, as well as of great importance, and we know

that disease of the spine in the early stage is at all times difficult of diagnosis; but, in the present lectures, it is not my intention to do more than advert to this class of distortions.

Curvatures or distortions of the spinal column, not connected with or dependent upon destructive disease of the bones or intervertebral cartilages, generally depend either upon muscular debility associated with some unfavourable constitutional condition, or upon the long continuance of some faulty position, the result either of acquired habit, or of some occupation; or they may depend upon both these causes combined. This is essentially the class of cases which I now propose to bring under your notice, and will first offer a few observations on the antero-posterior forms of curvature, before describing the lateral deviation.

Anterior Curvature of the Spine—Lordosis, λόρδωσις from λορδόω to bend supinely, so as to throw the head back.—Curvature of the spine in an anterior direction, with the convexity in front occurs chiefly in the lumbar region, as an increase of the natural curvature, from several causes; but it may also exist in the dorsal region to a less extent, and more rarely it occurs in the cervical region.

In the lumbar region this increase of anterior curvature is found—

1st. As a peculiarity in the natural conformation of the individual—often existing in several numbers of the same family, and hereditary. It is more common in some countries than in others, but in England I have most frequently observed it in short people, with long bodies and short legs, *i. e.* belonging to the rachitic type of development, and whose bony development exhibits some indications of a rachitic tendency.

2nd. As a constant effect of rickets in its more severe forms, in which the entire skeleton indicates the existence of this affection in the stunted growth of the individual from arrested development of the bones—more conspicuous in the leg bones from their normal rapid rate of growth during early childhood as compared with the spine and other bones of the body, hence the disproportionate shortness of the legs in these cases—and in various curvatures especially affecting the leg-bones and the pelvis. In severe rickets, I need hardly explain to you that the pelvis becomes flattened from above downwards, from the inability of its bones to support the superincumbent weight, and the sacrum assumes a horizontal direction. As a necessary result of this alteration in the direction of the sacrum, and the deviation in the axis of the pelvic cavity, an increase of the lumbar curve of the spine takes place to preserve the equilibrium of the body, and in these cases of rickets a very marked *lordosis* is the result.

3rd. As a constant effect of congenital dislocation, either of one or both hip-joints, an affection which occurs more frequently than is generally supposed. About twelve cases, or probably more, have come under my notice since this first affection was first pointed out to me by Dr. Carnochan, of New York in the year 1845, when that gentleman, being in England, brought a case of congenital dislocation of both hips, in a boy, aged 18, to St. Thomas's Hospital, for the purpose of demonstrating this affection, to which he had paid much attention. The external characters of the dislocation, and its effects in altering the conformation of the chest and abdomen, in consequence of the tilting forwards of the pelvis, and the production of lordosis to an extreme degree in the lumbar region, were so strikingly illustrated in this case, that under the direction of Mr. South, an entire model of the boy was made by Mr. Kierney, and is now in the museum of the hospital.

This form of dislocation, in which the heads of the thigh bones are generally displaced upwards on the dorsum ilii, or upwards and backwards, appears to be very seldom diagnosed. Commonly no attention is directed to the condition of the hip-joints till some time after the period of walking, and then, when only one joint is affected, the limping of the child induces the surgeon to suspect the existence of hip-joint disease, and the case is treated accordingly. This error in diagnosis has occurred in several instances which have fallen under my observation in private practice.

When dislocation of both hips exists, the real nature of the affection is

also generally overlooked, and I have known it said that the child would grow out of the peculiar waddling gait which characterises the walk in these cases. It is, then, in congenital dislocation of both hips that extreme incurvation of the lumbar region takes place, as a compensative effort to preserve the equilibrium of the body, disturbed by the tilting of the pelvis which necessarily results from the abnormal position of the heads of the thigh bones.

4th. Either co-existing with destructive disease of the lumbar vertebræ in the early stage, as in the case already described, or as a consecutive curve after destructive disease had become arrested, and angular curvature, *i.e.*, posterior projection of one or more spinous processes, produced in the middle or lower dorsal regions. In such cases the lordosis in the lumbar region is necessary to preserve the equilibrium of the body, and is, of course, persistent through life.

5th. As a constant effect of anchylosis of the hip-joint, with the thigh in a flexed position. After strumous disease of the hip-joint, rheumatic inflammation, and several other affections, anchylosis of the hip-joint, either ligamentous or bony, frequently takes place, with the femur in a flexed position, in most cases at an angle of about forty-five degrees, but sometimes at a right angle with the pelvis. The angle is generally much greater than is apparent at first sight, because in standing or walking, the patient brings the foot to the ground, by increasing the natural curvature of the spine in the loins, and in these positions, therefore, lordosis is produced, but it is more or less completely removable when the patient sits down. In such cases we have a greatly increased amount of flexibility in the lumbar region.

Treatment.—Surgically speaking, *lordosis* occurring in the lumbar region is of very little importance, as in its worst forms it is only a part of a more important affection, and very seldom becomes the subject of treatment; but it may sometimes assist us in the difficult diagnosis of destructive disease occurring in the lumbar vertebræ, as it might have done in the case above described, had we known of the occasional co-existence of these affections, and sometimes, though very rarely, it may become the subject of special treatment. In connexion, however, with the treatment of the diseases, such as rickets and caries which sometimes produce this form of curvature, I would especially direct your attention to a few practical points.

In the first class above described, viz., that dependent upon natural configuration or peculiarity, either of family or race, of course there could be no attempt at surgical interference.

In the second class—the rachitic form—no special treatment is indicated, but with the view of preventing this, as well as the more important pelvic distortion which accompanies it, I recommend as a general principle in cases of rickets, that the children be allowed to stand or sit as short a time as possible, *i.e.*, that standing or walking for any length of time must not be allowed, and that reclining or complete lying down be encouraged. It is a very serious, though common, practical error to send children in whom rickety curvatures have commenced, into the country, or to the sea-side, and encourage them to run about all day long; such children generally return with improved health, but with the curvatures more confirmed. This is very good treatment for slight cases of knock-knees and bow-legs, which children certainly do grow out of when the general health and strength are improved, but but a great mistake in cases of true rickety curvatures, which we know are immediately produced by mechanical causes acting upon imperfectly nourished bones. I need not allude to the influence of muscular action, either in causing or increasing rachitic curvatures. The bones of the legs and the pelvis become distorted simply by the weight of the body, which they are unable to sustain, and, therefore, I remove the weight of the body as much as possible; substitute exercise in the horizontal for exercise in the erect position, and rely upon constitutional treatment, especially cod-liver oil with steel, and the hypophosphite of lime, &c., for promoting the cure of the disease, if so it may be called.

Children may be induced to take a great deal of exercise, as amusement,

by pulling themselves up and down an inclined plane with ropes, pulleys, and weights, properly attached. They should be kept in the open air, in the country, or at the sea-side, as much as possible, and the reclining position in a properly-arranged carriage be encouraged. In many cases it may be impossible to carry this plan out as fully as could be desired, but if the principle be admitted, it may in all cases be adopted to a greater or less extent. We know that rickets, as a general rule, undergoes spontaneous cure with the increasing age of the child, and the alteration of its diet; and that the bones become even stronger than healthy ones, but that the deformities remain persistent through life. It may be said that rickety children grow out of the disease, but they grow into the deformity. The danger in females of the pelvic distortion, of which the spinal curvature or lordosis is the least important consequence, I need hardly advert to, but I believe that all the rachitic distortions may be, to a great extent, prevented by the treatment I have mentioned.

In the third class, i. e., lordosis in cases of congenital dislocation of the hip-joint, it is very doubtful whether any permanent diminution of the spinal curvature can be effected by mechanical means, though I have seen it attempted, and with some appearance of temporary benefit. Possibly the lordosis may be diminished by the constant use of a spinal instrument from early childhood to the completion of growth, but as the hip-joint articulations must remain in their abnormal condition, it is very doubtful whether any permanent advantage would be obtained by such means.

In the fourth class, i. e., lordosis existing during the progress of destructive disease in the lumbar region, or as a consecutive curve after destructive disease has subsided and angular curvature been produced in the middle or lower dorsal regions—there can be no doubt of the very great advantage of mechanical support during the progress of caries or destructive disease in any region of the spine, and the case of lordosis co-existing with caries in its early stage in the lumbar region, previously mentioned, affords a good illustration of the advantage of this plan of treatment. In all surgical works, without any exception so far as I have noticed, the necessity of the patient lying down during the progress of caries or destructive disease of the spine, is strongly insisted upon; but since my connection with the Orthopœdic Hospital, now nearly eleven years, I have invariably opposed this rule, and adopted the practice of applying mechanical support to the spine in these cases, by means either of a leather or steel apparatus, according to the age, and allowing the patient to walk about when so disposed. This treatment I find to be equally applicable to all stages of caries or destructive disease of the spine, from its commencement, which we may in most cases diagnose before any angular projection has taken place, and even when the destructive process is evidently advancing, provided the patient be able to bear the support, and is disposed to walk. The mechanical support enables such patients to walk about with comfort, and does not in any way interfere with, but I believe essentially promotes, the proper constitutional treatment of the disease. During periods of increased pain such as may occur in the stage of abscess, confinement to the bed is dictated by the patient's feelings, but as soon as the child (most of these cases are seen in children) is disposed to walk again, I allow him to do so. I never employ any local counter-irritation. All abscesses should be opened early by a valvular or subcutaneous opening, which I think is much better than allowing them to attain a large size, and the patient to suffer from the tedious process of pointing and bursting. In old cases of lordosis in the lumbar region, or consecutive to caries and angular curvature in the dorsal region, no advantage is to be derived from treatment, the curvature being a natural compensating effort to maintain the equilibrium of the body.

In the fifth class, i. e., lordosis consecutive to hip-joint disease, the curvature is also a natural compensating effort to maintain the equilibrium of the body, and there are no indications for surgical interference.—*Med. Times and Gazette, Oct.* 19, 1861, *p.* 395.

ORGANS OF CIRCULATION.

45.—TRAUMATIC ANEURISM OF THE FEMORAL ARTERY.

Case under the care of Dr. THOMAS J. RAWSON, in Carlow County Hospital.

[This case was treated first in the hospital, ten years ago. The circumstances under which the accident occurred, are related.]

David Lowe, (age at that time 49) the subject of the present report is a millwright by trade; he was sitting one morning in July, 1852, at a fire in the parlour of a public-house in Carlow, smoking calmly and watching with a philosophical eye the flies as they ran up and down the chimney-piece. He had been filling his pipe, and when he cut the tobacco he put the knife, "an ordinary clasp one," up on the chimney-piece with the blade open, but unfortunately for himself he put it up carelessly, and after oscillating on the edge of the slab for a moment down it came. Mr. Lowe was of a naturally active and energetic temperament, and as the knife was passing in its downward course between his legs, he brought them suddenly together with a jerk in the hope of catching it, and he succeeded in effecting his object perfectly, and something more besides, for he not only caught the knife, but he caught it at the moment its handle and point were fair across between his two thighs, and in bringing them together he drove it right into his left thigh under the sartorius, and wounded the femoral artery in Hunter's canal, of which immediate evidence was given by profuse hemorrhage; he called for assistance lustily, and Dr. Connor, being the nearest, was soon on the spot, but when he arrived finding there was a sheet torn into bandages and wrapped round it and all hemorrhage appearing to have ceased, he judged it better to leave as it was, and so it remained until, from his own account, the external wound was quite healed, which took place in two or three days, but it left behind it a large tumour, pulsating vehemently, and he was accordingly removed to the infirmary.

When Dr. Rawson first saw him, there could be no doubt, there was a traumatic aneurism of the femoral artery, the tumour covering three-fourths of the middle third of the thigh on its inner aspect. He, therefore, applied Carte's compress, and after a weeks' use the tumour began to decrease visibly, and the pulsation in it to lessen very much; it was, therefore, continued, and he was gradually getting better, when his unfortunate active and energetic temperament stepping in, he got weary of the compression and restraint, and left the infirmary directly against the wishes of Dr. Rawson; the tumour, however, at this time was not more than six inches round the base, and the pulsation was very faint indeed, so he seems to have gone about his ordinary occupations, and to use his own expression, did what he liked for a couple of months, but at the end of that time, the tumour being on the increase and the pulsation with pain and uneasiness returning, he applied again at the infirmary.

Dr. Rawson then advised him to go to Dublin and see what could be done for him there; he went to the Richmond Hospital, and to his surprise and disappointment the compress was again applied; he said he might not have come all the way from Carlow to get that done, so not believing he was getting better, he came home and applied a third time at the infirmary. Dr. Rawson now determined to tie the artery, but when the day of operation came, Mr. Lowe's courage failed him, and he would not permit it to be done; the compress was, therefore, applied again, and with such success, that in about three weeks the tumour became reduced to the size of a very small hen egg, while the pulsations in it had almost entirely ceased; he could now walk about with the greatest ease, so he left the infirmary and he returned to his avocations, pursuing them with perfect comfort to himself for nearly ten years, until one evening in June last, his unfortunate active energetic temperament got the better of him once more, and he got down into some water to work at a wheel that was out of order, and exerted himself very much, which he needn't have done, and when he came out he found his foot quite

numb and cold, and he couldn't warm it. He had no whiskey in the house, or he says the sequel would have been different, so he got into bed, and having suffered dreadfully the whole night, he came in the morning to the infirmary. On his admission, his leg showed deficient circulation and warmth, while the foot exhibited all the signs of incipient gangrene. He was very weak and low; the whole limb was wrapped in cotton and camphorated spirits, and he got wine and bark, opium and generous diet, under which treatment his leg gradually improved; the foot, however, getting worse, till on the fourth or fifth day sphacelus was too apparent at the toes, it spread up pretty quickly, and in a few days *the line of demarcation* was fully established at the ankle-joint, and the foot rapidly separated; the tendons were snipped through, and at last Dr. Rawson removed the foot, there being trifling hemorrhage from two small arteries which speedily yielded to cold.

He is now progressing most favourably; his appetite is very much better, and his tongue and pulse both greatly improved, which improvement is, in a great measure, due to himself; he is invariably cheerful, even while suffering extreme pain.

In trying to account for the recent gangrene, Dr. Rawson suggests that a portion of fibrine being detached from the aneurismal clot, by the blood which was circulating with very much increased rapidity from the exertion he was making, was carried down to the popliteal and being unable to pass its bifurcation plugged it, allowing barely sufficient to supply the leg, but not enough for the foot, that after some days the collateral circulation enlarging in the leg it recovered; but the foot, at that time, was too far gone to profit by this resource of nature, and so unavoidably perished.—*Dublin Hospital Gazette, Aug.* 1, 1861, *p.* 229.

46.—*On the Treatment of Popliteal Aneurism by Compression.*—[In most instances in which compression is used, the patients complain of the pain they suffer, and the length of time necessary to effect a cure. With regard to these objections, however, Mr. Erichsen observes, that the amount of pain depends very greatly upon the skill and care with which the apparatus is applied and managed, as well as upon the instrument used. (Mr. Erichsen uses Carte's elastic instrument). He further shows, that, in point of time, the cure is about the same, whether from the ligature or compression.]

Professor Pirrie, who has taken the greatest interest in the subject of the compression treatment in aneurism, and who purposely visited Dublin to satisfy himself by personal observation of the advantage of "the bloodless cure of aneurism," says, in his 'Principles of Surgery':—"The result to my mind was, the firm belief that the treatment by compression is a safe, simple, successful, and *almost painless* mode of curing aneurism."

The great success which the Dublin surgeons have met with in this mode of treatment, accomplished too without the suffering of much pain, would show that there is something in the minutiæ of the application of the compressing force which is not as yet quite understood here. Either the pressure on the arterial trunk is too great, and hence the inability of the patient to bear it; or else the apparatus is too complicated for the due regulation of pressure in accordance with the feelings of the patient.

Statistics have proved that the risk to the patient from the use of the ligature is greater than that from compression.—*Lancet, May* 25, 1861, *p.* 509.

47.—LARGE GLUTEAL ANEURISM; LIGATURE OF THE GLUTEAL ARTERY.

Case under the care of Prof. Syme, at the Edinburgh Royal Infirmary.

In the 'Lancet' for June 8th was noticed a case in which Mr. Syme tied the internal iliac artery for a spontaneous gluteal aneurism. The ligature separated on the twenty-fifth day after the operation, and the patient con-

tinues to do well in every way. This will make the seventh case in which the internal iliac has been tied, in most of the cases with success.

By a singular coincidence a second case of gluteal aneurism has occurred at the Edinburgh Royal Infirmary, also under the care of Mr. Syme. There was a large aneurism of the left hip, of traumatic origin. Seven years ago the patient, a middle-aged man, received a wound of the hip from a pruning-knife accidentally driven deeply in, which was followed by great bleeding. The wound closed, but an aneurism formed, which latterly had become greatly enlarged. It was as large as a man's head at the base, occupying the whole hip, and rose into a large blunt cone. Friday, the 14th, was a field-day at the Surgical Hospital, the large theatre being crowded by students and members of the profession, assembled to witness a bold operation which has not been seen for sixty years, since the intrepid John Bell performed it in a similar case in the same hospital. The description which John Bell has given of his case has been regarded as exaggerated or coloured; but no one will think so who witnessed Mr. Syme's operation last week. The only respect in which Mr. Bell's narrative transcends is in regard to the length of his incision, which was two feet in length; but the tumour he described as of "prodigious" size.

Mr. Syme commenced the operation by plunging a bistoury into the tumour. This was followed by a gush of arterial blood, which spouted to a considerable distance, but was immediately arrested by the operator introducing the fore-finger of his left hand, the wound having been calculated for this. With his finger in the sac, Mr. Syme now explored the interior, hoping to discover the situation of the artery by the entering current. Nothing, however, could be felt but a confused mass of clots. The operator then enlarged the wound cautiously until the whole hand could be introduced, which was now thrust in up to the wrist. As the hand was being used to search the interior of the sac, it was so held that the wrist was nearly sufficient to occupy the wound; but during this procedure several alarming gushes of blood took place, bespattering the operator and the bystanders, and most appalling to the onlookers, although in no way discomposing Mr. Syme. Compression of the abdominal aorta was tried, but as the patient lay nearly over on his face, this could only be done by pressing the abdomen up with one hand and the spine down with the other, and, as in John Bell's case, it appeared to do no good. Meanwhile nothing satisfactory could be made out by the hand; only masses of irregular and very dense coagula could be felt, from amongst which the blood came welling up. At this time the situation appeared a very critical one, and to those who were unacquainted with the operator's resources it seemed that he had only to choose between standing there for ever plugging the wound with his hand, or to withdraw it and let the patient's blood gush out.

Mr. Syme again took the knife, ran it swiftly up and down, laying freely open the whole cavity, and scooped out the clots, one of which was of enormous size; the assistants, ready with sponges, darted them into the bottom of the sac, and the serious bleeding was at an end. All this was done with the greatest rapidity and precision, Mr. Syme stating afterwards that in taking this step he had perfect confidence in his asssitants, on whose intelligent aid so much depended. The bleeding from the artery could now be commanded by direct pressure; not, however, Mr. Syme stated, when made against the bone, but when made into the sciatic foramen. The mouth of the artery was now seized with artery forceps, and a ligature carried round it and tied. All bleeding and risk were now at an end. The edges of the wound, which was directed downwards and outwards, were brought together and covered with a pledget of lint and a folded towel, but no bandage or compression was applied. The patient was under chloroform, and bore the operation well. The audience seemed surprised when Mr. Syme said that not much more blood had been lost than after a large amputation; but he reminded them that a large part of the blood which had escaped was that contained in the aneurism. In John Bell's case, the patient had fainted from loss of blood, and was supposed to be dead before the sac was laid freely open. Mr. Syme

also remarked that after the firm fibrinous coagula were removed, the whole of the cavity was found to be lined by a distinct and perfectly smooth membrane.

These who witnessed the cases in which Mr. Syme operated in the same manner for traumatic axillary and carotid aneurisms, say that the bleeding was more alarming and the difficulty greater in this gluteal case. Those who were fortunate enough to see this operation will not easily forget the scene. No surgical operation probably ever appeared more alarming; none which more required the combined and high exercise of courage, caution, self-reliance, and promptitude. Of this the large assemblage, so soon as the patient was removed, testified their feeling, contrary to the usual rule of the hospital, by greeting the operator with loud and prolonged applause. The patient continues to do well.—*Lancet, June* 22, 1861, *p.* 610.

48.—THE TREATMENT OF VARICOSE VEINS AND ULCERS BY MEANS OF BLISTERS.

Case under the care of Mr. URE, at St. Mary's Hospital.

The treatment of varicose veins by the application of blisters is so simple that it commends itself to notice. In the following case it was successful, whereas the previous use of needles had failed. Mr. Ure considers that the blisters cure by causing contraction of the veins and the deposition of fibrine into the surrounding areolar structures; and he has found the cure to be permanent, even in persons of advanced years. Blisters are most suitable in those cases where the vein is unequally dilated, and thicker in some parts and thinner in others than natural.

Caroline C., aged 48, a cook, married, mother of one child; admitted May 23, 1861, for varicose veins of the left leg, which had been the source of more or less annoyance for seven or eight years past, accompanied with an ulcer of the size of a florin, situated on the lower third of the leg, which was painful at night. The inner saphena vein presented a dilated and tortuous condition, and here and there knots were felt along its course, feeling at times very tense, as if about to burst, associated with pain which prevented sleep at night. She was an hospital patient about a year previously, and was treated by needles, which obliterated the circulation through the distended veins for a period of three months; after which the varicose condition returned to the same extent as before. She employed water dressing to the ulcer, and the condition of the leg occasionally improved; but whenever she over-exerted herself the ulcer became worse.

Her general health on admission was tolerably good, with the exception of occasional headache and giddiness since the "change of life." Her complexion was of a deep-red hue; pulse natural; tongue furred; bowels torpid; has suffered from piles. Ordered a blister over the ulcer, quinine mixture thrice a-day, and an electuary of senna with compound jalap powder.

May 27th. Ordered a blister, four inches square, to be placed over the cluster of varicose veins at the upper part of the leg.

30th. Condition improved; another blister to be applied to the varicose veins at the inner side of the knee, as they are bagging at that situation.

June 3rd. Improving in all respects; appetite good; dilation of veins materially reduced. Ordered another blister.

13th. A fourth blister ordered.

24th. Leg very materially amended by the treatment: ulcer nearly well. She no longer experiences the irksome aching in the limb which she did previously.

July 1st. Sore healed. A final blister, four inches by three, applied to the lower part of the shin at the inner side, where there are still some slight remains of dilatation.

5th. The patient discharged, perfectly cured of the varicose veins and ulcer.—*Lancet, July* 20, 1861, *p.* 61.

49.—ON THE SURGICAL TREATMENT OF VASCULAR TUMOURS.

By P. C. Price, Esq., Surgeon to the Great Northern Hospital, Assistant-surgeon to King's College Hospital, &c.

[Perhaps no form of Surgical disease has called forth more ingenuity in its treatment than have nævi, and the various forms of vascular tumours. The various methods now in use are in every way superior to those formerly practised.]

For the cure and removal of superficially situated tumours depending on an anastomotic arrangement of blood-vessels, it is all important that the means employed should be of such a kind as will insure the attainment of certain definite results.

According to the nature of the aneurismal tumour, must be the treatment employed. In some cases it will be amply sufficient merely to prevent the access of blood to the anastomosing channels. At other times, it will be necessary to excite adhesive inflammation in the substance of the vascular structure, while, not unfrequently, it will be compulsory to cause the most destructive forms of inflammatory action—sloughing and ulceration,—ere the disease can be eradicated. And, lastly, it may be advisable to remove the tumour by the knife or ecraseur—excision—on account of the dread of the above named processes being accompanied with an amount of risk, which both the patient and surgeon are anxious to avoid. The means for obtaining these various ends may be considered *seriatim*.

When it is desirable to prevent the access of blood into the anastomosing vessels of the tumour:—

Cold and astringent applications. Among the most enthusiastic advocates for the use of cold applications, was Mr. Abernethy. This distinguished surgeon believed that many forms of aneurisms from anastomosis might be considerably lessened in size, if not absolutely cured, by cold liquids, locally and continuously applied. The diminution they cause in the amount of blood entering the tumour, and the reduction in the temperature of the circulating fluid, were believed by this observant practitioner, to be sometimes sufficient to induce gradual shrinking, condensation, and obliteration. The cases adduced by Mr. Abernethy were examples of that form of aneurism which Mr. Wardrop designated *Subcutaneous Nævi*, and, in some instances, it is probable that the mere application of cold lotions, &c., may, for a time, prevent the rapid development of such tumours, even if absolute cure be not obtained. My own experience will not, however, allow me to admit that much hope is often to be entertained of a cure resulting therefrom, for I have frequently had occasion to operate on nævi of all denominations to which cold applications had been for a considerable period enthusiastically, but to no satisfactory purpose, applied.

Dr. James Arnott recommends the treatment of certain species of nævi by the local application of ice, and it is more than probable that the use of extreme cold may, occasionally, prove advantageous, if only in arresting further growth and development.

My friend Mr. Walton informs me, however, that he has, on many occasions, attempted to obliterate capillary nævi, by the application of intense cold, but in no single instance, has he succeeded, although every endeavour was used to make the means employed successful.

Should it be desirable to employ extreme cold to any form of nævi, great care must be taken in its application, for unless such be observed, sloughing and other serious consequences are very apt to occur.

Pressure, &c. Mr. Abernethy was the first to draw attention to the advantages to be derived from the application of pressure, especially when combined with cold and astringent lotions, in the treatment of certain varieties of nævi. The forms of tumour which he considered best adapted for this kind of treatment, are the spreading, irregular subcutaneous aneurisms from anastomosis; and in support of the success which he obtained, he adduces two cases, which certainly appear to have yielded to the measures employed.

One was a large spreading nævus, situated on the upper eyelid and forehead. To this, constant pressure and a solution of alum were kept applied. The other case was attended with still more satisfactory results. A large subcutaneous nævus, involving the arm, shoulder, and back, was in the space of three months almost entirely reduced by constant pressure, exercised by means of well applied bandages and strappings. There can be no doubt that this plan of treatment when judiciously applied, is often valuable in arresting the progress of, if not exactly of use in curing, some kinds of nævi. The cases in which pressure is of most avail, are those which come under the denomination of subcutaneous nævi, where the principal anastomoses of the vessels take place in the cellular tissue, and I have occasionally known these tumours yield to such means, when appropriately employed. The best way to apply pressure to isolated nævi of this description, is that suggested, I believe, by Sir B. Brodie, viz., by means of discs of ivory, or thin lead, somewhat larger than the tumour, adapted and confined with a certain force *in situ* by strips of sticking plaster. I have in one or two instances, in which all operative measures have been refused, applied pressure by means of a copper or silver coin firmly bound and kept in position by strapping or bandages.

Mr. Lacey, of Poole, has suggested that an elastic material should be used, while other surgeons have advocated the application of various ingenious contrivances, such as spring pads, plaster of Paris casts, &c. But though pressure sometimes proves of direct value in lessening, and even curing subcutaneous nævi, yet it is very often applied for a considerable space of time, without the least apparent benefit; indeed I have, on more than one occasion, known it do harm, by inducing irritation in the tumour, and inflammatory conditions of the covering, and partially implicated integument. In cases of arterial capillary nævus (Teleangiectasis) it should never be thought of, as it is very likely, even in the course of a few hours, to induce sloughing and troublesome hemorrhage. I may quote the following case, (selected from among many) as showing how the application of pressure is sometimes not only of no avail, but positively injurious. In the month of May last, I was asked by Dr. Hadaway, to see a child about eighteen months of age, the subject of a subcutaneous congenital nævus, the size of a florin, situated over the right brow. Futile attempts had been made to destroy it with nitric acid. Subsequently, pressure was applied for some weeks by means of a piece of metal, confined *in situ*, by sticking plaster. This occasioned very considerable pain, great congestion of the neighbouring veins, and, in addition, seemed to irritate and cause rather an increase than diminution in the disease.

The growth appearing to be fed by one or more arteries of medium calibre, I strangulated it with a ligature passed through its base, in the manner to be presently described, taking care to avoid including any portion of the skin in the threads. In the course of a week, the nævus sloughed away, and a small granulating surface remained, which rapidly cicatrized.

Pressure may sometimes be conveniently employed to reduce the size of a nævus, ere more definite means are practised. For whatever purpose it is used, it is always necessary to bear in mind, that a well regulated and gentle compression is far more likely to lead to diminution, and, perhaps, to obliteration, than one less discriminatively and carelessly applied.

[In the treatment of these cases it may be necessary to resort to *deligation* of the largest vessels, especially when compression leads to marked cessation of the flow of blood. For this purpose the metallic pin and the twisted suture may be most useful; but in general other means must also be resorted to. To cause inflammation in the vascular structure of the tumours is one of the most successful means of curing them.]

On looking through the records of surgery, abundant evidence will be found of the great attention that has been bestowed on the treatment of vascular tumours by the production of more or less intense inflammatory changes. With what amount of success various plans capable of producing such changes can be employed, may be advantageously noticed.

In that form of disease which is termed capillary aneurism, considerable good may oftentimes be effected by the use of certain applications which, while inducing certain mild inflammatory and other changes, do not lead to marked destruction of component tissues.

It sometimes happens that the mere application of certain astringent and slightly caustic solutions, causes a complete metamorphosis of the vascular structure. *A strong solution of alum* or *perchloride of iron* has not unfrequently been found sufficient to cause shrinking and withering of the red vascular tissue of a teleangiectasy.

I have succeeded in obliterating such forms of vascular growths by one or two applications of a moderately strong solution of *iodine*. In one case, an infant, a patient at the Great Northern Hospital, this plan answered every expectation. Two small vascular nævi on the chest, entirely disappeared after the second application of the tinct. iodinii co. No actual destruction took place, although, from the shrivelling and shrinking induced, a small white scar resulted.

I have seen a case in which my friend, Dr. Gibb, of Portman street used iodine to a capillary nævus of large size, situated on the back of the arm. The result is all that could be desired.

But while this plan of treatment sometimes succeeds, it will occasionally be found to fail, although every care and attention be bestowed. Under such circumstances, other applications may be tried before resorting to the use of the ligature.

Ointments of various kinds enjoy with some practitioners great repute, and are used in the treatment not only of capillary but subcutaneous vascular tumours. Hickman has strongly recommended the application of *tartar emetic* in the form of an ointment or plaster, and, there can be no doubt that the production of certain inflammatory changes thereby induced may prove sufficient to destroy the growth.

Dr. Cumming has adduced eight instances in which "vascular nævi" were successfully treated by means of a plaster, composed of fifteen grains of tartar emetic mixed with one drachm of galbanum plaster, spread on a piece of thin leather cut accurately to the size of the tumour. About the ninth day, pustules having completely involved the diseased structure, the plaster was removed, when, in the course of time, sloughing having taken place, cicatrization followed, without the production of extensive scars.

In the subcutaneous form of aneurism from anastomosis Mr. I. B. Brown, and other surgeons, have had recourse to tartar emetic, and, it appears, with varying results. I cannot, however, recommend this plan of treatment, as it is one which requires much care, and is very tedious and painful. Several days elapse ere the plasters can be removed, and during this period the itching and irritation are often severe. The amount of sloughing is also sometimes greater than necessary, while the scarring which results is more extensive than that which follows the use of equally or more effective measures.

A mixture of *collodion and corrosive sublimate* is often used on the continent as a destructive application to small vascular nævi. Drs. Assandri and Cosfeld strongly recommend such an application, and in certain instances, there can be no doubt that it proves very effectual. *Collodion* by itself is sometimes used to cover small, spreading, highly vascular nævi, and its constant application occasionally leads to cure. The brilliant red colour of the growth gradually subsides, and in the course of time, the arterial structure is replaced by a white cicatricial tissue. Dr. Browning, of Sheffield, has recorded three cases in which good results followed this plan of treatment. I cannot, however, say that I have experienced much satisfaction from its employment, for, in more than one instance, from its failure, I have been obliged to have recourse to other means.

Some small highly vascular capillary nævi may be gradually destroyed by occasionally pencilling their surfaces with *creasote*. Dr. Bujalsky, has succeeded in this way in obliterating such growths.

When these various measures are found inapplicable, it may be advantageous to produce adhesive inflammatory changes in other ways.

Thus, Mr. Hodgson, Mr. Cuming, and other surgeons, many years ago, suggested the gradual destruction of congenital nævi by the *innoculation of vaccine lymph*. Such a plan has been very frequently adopted, sometimes with the best wished-for results, and at other times, with little or no advantage.

Destruction of capillary nævi by some potent caustic is oftentimes the most convenient and suitable plan to adopt. A circumscribed spreading telangiectasy may, by being touched once or twice with *caustic potassa*, be entirely destroyed. An eschar is formed, including the whole of the vascular tissue; this, in a few days, drops off, and a granulating surface remains, which rapidly cicatrizes. Great care is, however, necessary in using this caustic, as it is liable to implicate the sound tissues immediately bordering the tumour. Certain forms of limited capillary nævi may be effectually destroyed by one or two applications of *strong nitric acid*. This corrosive liquid is best applied by means of a finely pointed piece of wood. Care should be taken that not only the superficial but deeper structures of the growth are included in the application.

Although small vascular tumors can in this way be satisfactorily destroyed still the plan of treatment is not appropriate for larger growths, especially when seated in the scrotum, &c. I have known the disease when occupying this structure, instead of being lessened by the use of acid, irritated thereby. Acid and indeed any caustic applications, cannot, with any degree of certainty, be employed for the destruction of the subcutaneous aneurism from anastomosis. If used, extensive sloughing is not unlikely to jeopardise even the life of the little patient, while, even if the disease be cured, the scar which results is often extensive.

I have seen more than one instance in which the scarring after the use of acid has been far greater than what results from the application of the ligature. I may mention the following case:—

In the summer of 1859, I assisted Mr. Furgusson to remove from the arm of the child of a lady a small nævus. About two years previously, another surgeon had, by means of acid, destroyed a similar growth, occupying the nape of the neck. The tumor was small, but the scar which resulted was nearly, if not quite, as unsightly as the original mischief.

Several applications of the acid are often necessary if much cellular or fatty tissue be mixed with the true vascular structure, and this is one reason why great discretion is needful in advising such a method of treatment.

I was consulted by a practitioner in the early part of the present year regarding an indurated nœvus. Strong nitric acid had been used on several occasions, but each application seemed rather to irritate, and lead to increase of the growth than promote its destruction. I accordingly strangulated it with the ligature, and, in less than a fortnight scarcely any deformity remained.

Besides caustic potassa and nitric acid, other caustic applications may be selected, but, in appropriate cases, one or other of these will be generally found effectual.—*London Medical Review, Aug. and Sep.* 1861, *pp.* 69, 119.

50.—ON THE RADICAL CURE OF VARICOSE VEINS.

By John Wood, Esq., King's College Hospital.

A female had been subject for years to varicose veins of both legs, which were so painful that she applied for something to be done to cure them. Mr. Wood's operation consists in the application of a pair of needles, invented by him for this purpose. The needles, of the thickness of an ordinary hare-lip pin, are bent at right angles, about an inch from the head, forming, at the bend, a loop for the reception of the point of the fellow needle. The head, or blunt extremity, is also formed into a loop to serve for the purpose of retention by a strip of lint or plaster. The point of one needle is straight

and spear-shaped, as in the hare-lip pin; that of its fellow is curved as in the ordinary surgical needle for sewing up a wound.

The method of application is as follows:—The skin over the vein being pinched up and raised from the vein by the finger and thumb, the straight needle is passed close under the skin, between it and the vein. The finger and thumb are then adjusted so as to raise the vein from the fascia a little on one side of the needle. The curved needle is then passed through the same punctures in the skin as the former one, but in an opposite direction and under the vein, which is thus included between the needles. The needles are then locked on to each other by passing the points of each through the loop at the bend of the other. The punctures are then protected from pressure by the interposition of a piece of lint or wash-leather, and the needles pushed in so that the loops are close to, but do not press upon the punctures. The points of the needles are then cut off with pliers. The bent head of one needle is then turned upon its fellow until it lies on the limb in the same direction. This will be found to twist the shafts across each other in such a manner as to compress laterally the included vein, and at the same time slightly to twist it. The needles are then held firm in their places by a strip of lint twisted figure-of-eight-wise, and a strip or two of sticking-plaster, care being taken to prevent all chafing. They are left in a week or ten days, until so much effusion has taken place as permanently to obstruct the current in the vein, and by progressive absorption from *acupressure on both sides* to cause complete obliteration. In the many cases treated by Mr. Wood in this manner, there has been no suppuration whatever in the track of the needles, when care has been taken to prevent chafing or irritation. A drop or two of serum, forming a sugar-like crust, is the only discharge.

This plan, remarks Mr. Wood, possesses the advantages of being almost entirely free from pain (the skin not being included in the pressure); of giving rise to no irritation or suppuration, which is invariably the case, he says, when apparatus is used which presses into the punctures, or includes the skin, or requires subsequent twisting or tightening. It has also the advantage of a complete, equable, and constant lateral and linear acupressure without movement or disturbance; and so is insured an obliterating action on the vein, proceeding equally from the two sides. By this means, he adds, the patient is insured against the danger of putrid matters or pus getting into the circulation through the vein operated on.

In the ordinary method of passing one needle under the vein and a twisted thread or wire over the skin, it will be seen that the acupressure is on one side only, the soft tissues of the fascia, fat, and skin, being the resisting force on the other. The effect is similar to that of pressing with a stick upon a soft cushion. The coats of the vein are absorbed on one side only, and when half cut through the current may be established on the other side of the needle, and the operation fail from this cause, or purulent matter pass into the half-opened vein.

The patient after the operation remained entirely free from pain. The dressing was not disturbed, except once to ascertain the condition of the punctures on one leg, until the tenth day, when the needles were withdrawn with great ease. No suppuration whatever had occurred. The punctures were almost exactly in the same condition as on the first day. No blood followed the withdrawal of the needles. A hard line in the centre of a [illegible]verse depression was felt after their withdrawal, no swelling or hard [illegible]dation in the veins below, the blood having found its way through the [illegible]l venous channels.—*Med. Times and Gazette, Oct.* 12, 1861, *p.* 377.

[illegible]*llet Lodged in the Heart for Twenty Years.*—Dr. Balch, of [illegible]ace, New York, details the following rare case in the 'American [illegible]d. Science':—In 1840 a boy, aged about 14, was shot in the [illegible]he bullet entering through the upper border of the trapezius, [illegible]from the acromion. The ball, by those who examined him,

was supposed to lie about four or five inches from where it entered, in a direction towards the cavity of the chest, near the sternal end of the clavicle. In about six weeks he resumed his work, and gradually quite regained his health. In 1845 he was attacked with severe pneumonia on the right side, and then was first noticed a tumultuous action of the heart, which continued to increase after his recovery—its pulsation being seen and heard at a distance of ten or twelve feet. Another attack of pneumonia carried him off in 1860. At the autopsy the right internal jugular was found enlarged, and the external was entirely closed, about a quarter of an inch from where it entered the internal jugular. In the right subclavian artery, at the thyroid axis, was a large ossific deposit. The heart, two or three times its natural size, was very soft and flabby; and although on the outside no scar was discernible (the pericardium was, however, very adherent on the right side), on opening the right ventricle, a leaden bullet was found imbedded in its walls.—*Med. Times and Gazette, Aug.* 31, 1861, *p.* 229.

ALIMENTARY CANAL.

52.—A RARE INSTANCE OF CONSERVATIVE SURGERY.

By Dr. J. Nichols, Savile-row, London.

The length of time that parts separated from the body retain their vitality, and consequently their capability of re-uniting with those from which they have been separated, is, I believe, not correctly appreciated either by the profession or the public; but that it is much longer than is generally supposed the following case will show.

In the summer of 1846 (I was then living in Leicester-place) a servant drove up to my door in a street cabriolet—she had come from Wellington-street, Strand,—and requested me to return with her to see her master, a gentleman of more than seventy years of age, who had fallen down and seriously injured himself. Anticipating some occasion for strapping, lint, &c., I stayed long enough to provide myself with them, and then returned with her as quickly as the crowded state of the thoroughfares would permit. On arriving at the house, I was hurried up to the drawing-room, where I was met by my patient, who, holding a handkerchief to his face, said, "Doctor, I have cut my nose off." I was at that moment rather incredulous, but his daughter soon removed all doubt as to the nature and extent of the injury by showing me the separated portion, which she had picked up from the floor. It was black, cold, and covered with grit and dirt. On examining the wound, I found that the whole of the fleshy end of the nose, together with the alæ and septum, were clean cut away, and the white end of the cartilage exposed. The upper lip was divided transversely throughout its whole extent, and hung down over the mouth. It appeared that this gentleman, on going upstairs had stumbled near the upper step, and, trying to recover himself, had fallen forwards against a wooden flap placed at the drawing-room door, the sharp edge of which had come in contact with his nose, first compressing it and then separating it from the face.

For a moment I hesitated what to do, but thinking the separated part would be as good a dressing as any other to the exposed surface, and that the patient's hope (though I had none) of its reunion would give time for him to reconcile himself to its ultimate loss, I determined on readjusting it. This was easily enough done. The grit was washed from it, and, being carefully replaced, it was retained *in situ* by adhesive straps. The edges of the wound of the lip were brought together, and kept so by similar means.

On calling the next day, I observed that the end of the nose—which I had purposely left exposed—had lost the deep-black colour that it had when replaced, and bore evident signs of circulation going on in it. There was no discharge from the wound. On the next day, appearances were the same; and on the third, the dressings were removed, when I was as much astonished

as gratified to find that union had taken place throughout the whole extent, and the scar that was left was scarcely perceptible. The lip had also united.

Now the time that had elapsed between the separation and the re-adjustment of the divided parts could not have been less than three quarters of an hour—an important fact, the knowledge of which cannot be, in my opinion, too widely disseminated; and when I reflect on the numerous cases of mutilation, the result often of drunken violence, that appear at our police-courts, in which no effort is made at restoration, I take some blame to myself for not having published the case before. The late Dr. Addison was called in to see some member of this family, and I showed the case to him. He examined the course of the cicatrix very carefully, and observed, "You ought to publish the case."—*Lancet, Aug.* 24, 1861, *p.* 195.

53.—*Ulcerated Throat.* By ELLIS WESTCOTT, Esq., Westminster Hospital.—I have found the undermentioned of great value, and never knew it to fail. The following lotion to be applied night and morning with a camel's hair brush:—Nitrate of silver, ten grains; pure nitric acid, three drachms; to five drachms of distilled water. Also a gargle, to be used frequently, consisting of gallic acid, ten grains; tincture of capsicum, half a drachm; to six ounces of compound infusion of roses. And a tablespoonful of the following mixture to be taken three times a day:—Dilute nitric acid, two drachms; syrup of oranges, half an ounce; to eight ounces of infusion of chiretta.

If these be persevered with for a short time, I am convinced that a perfect cure will be established.—*Lancet, May* 18, 1861, *p.* 500.

54.—ON THE MODE OF OPERATING FOR THE RADICAL CURE OF HERNIA.

By JOHN WOOD, Esq., Surgeon to King's College Hospital.

[The patient in this case was a sailor; the hernia was a right oblique scrotal hernia of ten months' duration.]

An incision directed obliquely downwards and outwards was first made through the skin of the scrotum over the middle of the hernial sac, with a small narrow-bladed knife. The edges of the skin were then separated from the subjacent fascia by carrying the blade of the knife flatwise under the skin to the extent of three-quarters of an inch from the margin of the incision. The denuded fascia was then invaginated by the fore-finger of the right hand into the inguinal canal, carrying the sac, before it. The lower border of the internal oblique was then felt for, and raised upon the finger. The curved hernia needle was then carried along the finger at its thumb side (*i. e.*, towards the patient's linea alba) and passed through the conjoined tendon from behind forwards till the point of the needle was seen to raise the skin of the abdomen about one and a half inches above the pubis. The skin of the groin was then drawn inwards as far as possible, before the needle was pushed through it. A piece of stout copper wire silvered, and tapered off at both ends to a flexible degree of thinness was then attached by one end to the eye of the needle, and drawn back with it through the scrotal incision, and then detached and left in the wound. The invaginating finger was then carried outwards behind Poupart's ligament, or external pillar of the ring, as high up as possible. The aponeurotic structures were then lifted upon the finger, and the needle passed through them on the finger along its opposite border (*i. e.*, that nearest the ilium of the patient). The point of the needle was carried through the same aperture in the skin of the groin as in the first instance. The other end of the wire was next fixed to the needle, drawn back with it into the scrotum, and also detached. The sac of the hernia, at the scrotal incision, was then pinched up from the cord behind by the thumb and forefinger of the left hand, and the needle passed across it in

front of the cord, in the same manner as in passing a needle for the cure of varicocele, but within the limits of the scrotal incision. The inner extremity of the wire (*i. e.* that which included the conjoined tendon) was then fixed to the eye of the needle, and drawn through the sac across the front of the cord. The two ends of the wire were next drawn down till the loop or bend was close to the upper puncture, the loop being held by the forefinger of the assistant. The ends of the wire were then twisted firmly down upon the fascia and sac in the lower puncture, and cut off at a point about five inches distant from the skin. By traction upon the upper loop the twisted ends of the wire were then drawn up into the inguinal canal, invaginating the sac and fascia, and leaving, when drawn tight, about two inches protruding from the lower puncture. The upper loop of wire was next twisted firmly down into the puncture in the skin of the groin until the sides of the canal were drawn firmly together. The lower or cut ends of the wire were then bent into a small hook, and curved upwards, so as to meet the upper or loop end curved downwards to meet it in a corresponding degree. The hook was then fixed into the upper loop, a pad of lint inserted between the wire and the skin, and a spica bandage and compress put over all, so as to exercise firm pressure upon the posterior wall of the canal. The spermatic cord is left behind the wire with a small portion of the neck of the sac covering it. This part of the sac is obliterated by the pressure of the wire backwards upon it.

The above method of operating is the one now practised in most cases of scrotal hernia by Mr. Wood. It is simple and easy in application, and has proved very decidedly successful in the cases in which it has been employed hitherto. There is less pain and much less discharge than in the cases where the ligature thread and hard compress were employed. The use of wire also gives the valuable advantage of enabling us to employ direct contact and pressure upon that part of the posterior wall of the hernia canal and sac close to the cord which is not included between the two ends of the wire, and so to effect its complete closure by adhesion at that point.

Mr. Wood has now operated in thirty cases, and up to the present time has had only six failures. The first case was operated on more than three years ago, and was shown at King's College Hospital a short time ago quite cured, and wearing no truss.

Since the operation the case has done uncommonly well. There has been remarkably little suppuration. Some swelling of the testis showed the effect of the pressure in the inguinal canal. The wires were untwisted and removed with great ease and little pain sixteen days after the operation. The patient is now almost ready to be discharged (three weeks after the operation) with great consolidation of the parts.—*Med. Times and Gazette, June* 1, 1861, *p.* 578.

55.—ON A NEW OPERATION FOR THE RADICAL CURE OF HERNIA.

By Prof. Chisholm, of South Carolina.

This operation, by Prof. Chisholm, of South Carolina, is thus described by one of his pupils:—"These and other considerations have induced Dr. Chisholm not to rest satisfied with the successes gained even by his modification of Wood's operation, but to add a still greater improvement, simplifying the entire procedure, and obviating nearly all the objections which have been, or may be urged to Wood's.

"The new operation is as follows: The scrotum having been invaginated upon the finger—as the only mode of guiding the needle in its passage—a long, strong curved needle, fixed firmly in a handle, and armed with silver wire, guided by the finger, transfixes the scrotum at the apex of the invaginated portion, passes through the internal column, and appears through the skin of the abdomen, when one end of the wire is drawn out. The point of

the needle is then drawn backwards, and disappears again in the canal. Its direction is then changed. Whilst still embedded in the scrotum, and guided upon the finger, its point is made to traverse the external column of the ring, near Poupart's ligament, lifting the skin of the abdomen. By gliding the skin upon the needle, the point appears through the small puncture made by the first passage of the needle, when the other end of the wire is seized, the needle is unarmed and withdrawn through the scrotum. The finger is now removed from the canal, and the two ends of the wire being drawn upon the loop, it dissects the cellular tissue up to the columns, which it hugs closely. By twisting the ends of the wire the columns are felt approaching until they are brought into such close apposition as to allow nothing to pass between them—the spermatic cord in its exit filling up all the available space remaining of the ring. When the ring is felt closed, the twisted wire is drawn firmly outwards, and clipped off as close as possible to the skin, so that, when traction on the skin of the abdomen is removed, the gliding back of the integuments to their normal positions conceal completely the ends of the small loop of silver wire. The scrotum has already fallen back to its pendent position, and the only trace of an operation having been performed is in the two small punctures, one in the scrotum and the other in the abdomen which require a careful search to find them, and which will heal up in a few hours, hermetically incarcerating the silver wire.

"A moderate inflammation follows the operation, without much pain or swelling, and without any fear of suppuration. The wire is soon embedded in a lymphy deposit, which will not only enclose it, thus isolating it from the tissues, but, at the same time, agglutinates the columns together as an additional security to the success of the operation. The patient is kept quiet in bed four or five days, opium being given to insure rest and prevent action of the bowels. When the inflammatory stage has passed, a cathartic is administered, and the patient can quit his bed, resuming his occupation in a few days. The silver wire remains a permanent application. An essential element in the success of the operation is that the loop encircle the columns of the ring near their points of attachment to the pelvis. Otherwise, the columns cannot be approached, the ring remains open, and the results can only be negative. If this step of the operation be carefully followed, a radical cure can be nearly guaranteed."

The advantages of the operation are the short time the patient is detained in bed, the slight amount of inflammation produced, the dispensing with the use of a truss, and the security against relapse, consequent on muscular effort —the reliance being placed, not on newly-formed adhesions, but on the silver wire. When the protrusion is large and the ring voluminous, several points of suture may be applied through the same punctures. The objection that the wire will always act as a foreign body is shown by experience to be unfounded, as it may remain harmlessly embedded in the tissues for any length of time. Four cases only have as yet been so operated upon, and that only since Nov. 1860.—*American Medical Times*, No. 5.

To the above we may append an account of a mode of operating found useful by Dr, Agnew, of the Philadelphia Hospital, and described in the North American Medico-Chirurgical Review (January, p. 117), in the following terms:—"The instruments necessary for the operation consist, first, of two semi-cylinders of steel, 3 inches long and 1¼ inches in circumference, which can be separated from each other by a screw in the handle of the instrument, and on the internal face of the lower blade of which are two parallel, longitudinal grooves; secondly, of a spear-pointed needle, slightly curved at the extremity, and supported on a bone handle; and, thirdly, of several stout needles of from 2 to 2½ inches in length.

"A portion of the scrotum is carried in followed by the metal cylinder, and thrust up to the internal ring. The blades are now separated by means of the screw, and the long needle, armed with a thread of silver wire, is carried along one of the grooves to the upper end of the cylinder, and then made to pierce the exterior parietes of the inguinal canal. The thread is then removed from the needle, the latter withdrawn, and the other end of the wire passed

through its eye, when it is passed along the other groove and made to emerge a short distance from the first. It is again unthreaded, and after its removal the two ends of the wire are drawn up tightly and twisted over a small roll of lint. This effectually holds up the small plug of integument to the very summit of the canal, and as the silver thread manifests but little disposition to ulcerate out, it may be allowed to remain for a considerable time. The second and most important step of the operation consists in screwing the handles of the instrument completely together, (thus separating the blades in the canal to their greatest possible extent), and then, carrying across the canal, between the blades four or five threads, at equal distances from each other. The first thread should be composed of silk, and be introduced as near the internal ring as possible. The remainder should be of silver, the last one being close to the external ring. These transverse threads can be lodged in the canal with great accuracy by this method, the cord being protected from injury by the posterior blade of the instrument. The latter being withdrawn, the patient is confined to his bed, the parts being protected by a compress and roller, and the wires should be removed as soon as the plastic exudation has bound the invaginated plug to the walls of the canal, which in the cases operated upon required twelve days."—*Medical Times and Gazette, March* 30, 1861, *p.* 339.

56.—COMPLICATED CASE OF OBLIQUE CONGENITAL INGUINAL HERNIA.

By Dr. William J. Wilson, Surgeon to the Clay-Cross Collieries, Derbyshire.

On the 16th of April, 1861, at two a. m., I was summoned by Dr. Houghton, the assistant-surgeon to the Clay-cross Works, to a patient suffering from strangulated oblique inguinal hernia, whom I found drunk, collapsed, pulseless, and vomiting, apparently in great pain, and very unruly. On examination, I discovered a firm, oval tumour, the size of a hen's egg, in the left groin, and considerable fulness along the course of the inguinal canal. I entertained doubts as to the character of the tumour, but the patient not being in a position to give me its history, and the bystanders knowing nothing more than "that it had been down once before, and had been put back without cutting," I did not feel justified in leaving my drunken patient with such a suspicious tumour in his groin.

Dr. Houghton at once administered chloroform, and it acted as a direct stimulant to the nervous system and to the heart's action, as, indeed, I believe it almost invariably does in collapse the result of injury. The patient having been brought fully under the influence of the chloroform, I applied the taxis, but without success, and then proceeded with the usual operation for the relief of oblique inguinal hernia. I found it necessary to divide some constrictions at the internal abdominal ring, after which a portion of the tumour was returned into the abdomen with an unmistakable gurgle. A tumour, however, still remained in the groin, between the scrotum and the external abdominal ring. I re-introduced my finger into the inguinal canal, and found only the spermatic cord, and that the communication with the abdomen was quite patent. Now came the question—What is this tumour? Not an undescended testis, for there are two in the scrotum. Is it, then, a hydrocele of the cord? I applied the knife, divided a fibrous membrane, and then there exuded a number of fine, vermiform, yellowish filaments, each accompanied by a congested vein; and on squeezing the testicle (?) in the scrotum, these filaments and veins protruded all the more from the cut in the inguinal tumour. I now felt certain that I had cut into a portion of the testis, and believed that the inguinal tumour was the globus major of the epididymis, enlarged by disease, and that the vascular filaments were coni vasculosi. The gut being fairly returned, however, I felt only a temporary pang at having wounded the testis, and was content to labour for a few days under the imputation of having operated on a testicle in mistake for a hernia.

Eight hours subsequent to the operation I obtained the following history from the patient: He has always had *two* testicles in the scrotum, and has remarked that the left one (corresponding to the groin operated on) has always been a little smaller than the other. Never observed a tumour in his groin, except on one occasion, when "it came down" while he was lifting a weight. It was returned by my late assistant, Mr. Carter; and he had leeches applied to his abdomen, and took pills and medicine. On the present occasion, he remembers sneezing, then "feeling something give way in his groin," which caused such great pain that he fell to the ground. "Drunk and incapable," there he lay until found by his friends, at two a.m.

The history was simply that of a hernia of the gut. Had I then erred as to the tumour left in the groin? Had I left a lump of omentum there? I felt certain that I had not erred, and that my patient's history was not correct. Both Dr. Houghton and myself had passed our fingers into the abdomen; both had felt that the inguinal canal contained nothing but the spermatic cord; both had seen the spermatic ducts (?) ooze through the cut in the inguinal tumour when the testis (?) in the scrotum was squeezed; both of us had felt the tumour, and traced the spermatic cord passing into it. How could we be mistaken? The sequel will show how far we were correct in our diagnosis.

Nine P.M. Swelling and intense tenderness of the testicle (?) in the half of the scrotum corresponding to the groin operated upon; pain and tenderness along the inguinal canal and over the lower part of the abdomen; anxious expression; pulse 120, small, and wiry; great nausea and faintness; slight bilious vomiting; no action of the bowels. I diagnosed *orchitis*, consequent on the puncture of the portion of testis (?) in the groin; and peritonitis, the result of the strangulation of the bowel and of the operation; and I saw in this orchitis (?) a confirmation of my opinion as to the nature of the inguinal tumour. Ordered twenty leeches to the tender parts; calomel and opium every two hours; and the effervescing citrate of potash with prussic acid.

17th. The peritonitis has become more general, but the scrotal tenderness is much less marked. Ordered a repetition of twenty leeches, and to continue with the calomel and opium. Flatus has passed freely per rectum, but no stool. Vomiting continues, but the ejecta have not the slightest suspicious appearance or smell.

18th. Dr. Booth, of Chesterfield, kindly met me in consultation, and to him I stated every particular relative to the history, the operation, and the subsequent treatment of the case; the main points of my statement being, that a knuckle of intestine had been returned, completely and satisfactorily, but *no sac opened*, and I believe there was none; that the remaining inguinal tumour was *part of*, if not *the* testis; and that I had satisfied myself of this by opening it. I asked whether the symptoms called for any further operative interference? Dr. Booth replied that *the testicle was certainly in the scrotum;* but that he would not hold himself responsible for what was in the groin. Believing my statement as to the complete return of any abdominal protrusion that might have been present, he did not consider the existing symptoms more urgent than frequently occurred after the operation for hernia, and he should not advise an operation, but should rely on calomel and opium, fomentations, &c.

19th. Ten A.M.: Has passed a tolerable night, and is improved in every respect. The abdominal and scrotal tenderness has almost entirely disappeared; the wound in the groin is sloughy and painful. I administered a copious enema of warm water. It returned, in about half an hour, slightly discoloured and smelling very offensively.—Ten P.M.: Vomiting has been very urgent for the last two hours, and the ejecta have a fecal appearance.

20th. Vomiting decidedly fecal and very copious. Bowels not moved. Complains of great pain about the umbilicus and left groin. The patient insists that "there is an obstruction," and prays that it may be removed.

I now began to feel that a second operation was imperatively demanded, and that in all probability we should find a knuckle of intestine had again

been forced into the inguinal canal (rendered unusually patent by the recent operation) during the act of vomiting, no pad having been applied in consequence of the tumour left in the groin.

21st. Two P. M.: In the presence of Dr. Booth, Dr. Houghton, Mr. G. O. Siddall, and two medical students, I turned up the flaps made at the previous operation, and exposed a tumour, of the size of an egg, thickly coated with recent lymph. After scraping off some of this lymph we found the tumour to consist of two distinct parts, the upper portion being a knuckle of inflamed intestine, and the lower the testis (?). The intestine was flaccid, and overlapped the upper portion of the other tumour. I enlarged the incisions made on the previous occasion, so as to expose the whole of the inguinal canal, and found that by simply breaking down the recent lymph which connected the bowel with the canal and the internal ring I could readily pass my finger into the abdomen. Observing this, together with the flaccidity of the bowel, I felt that the hernia was *retained* in the canal by the inflammatory products rather than *strangulated at its neck*. Partly with my finger, partly with an edgeless knife, I broke through the remaining adhesions and then returned the bowel with the greatest facility. The gentlemen present now examined, with their fingers, the tumour left in the groin and the one in the scrotum, but without satisfying themselves as to which was really the testis. The prevailing opinion was that the *testis was in the scrotum*. Each tumour felt as though a "cord" passed into it. The examination was merely tactile, the inguinal canal having been ascertained to contain nothing but the spermatic cord, and the communication with the abdomen to be quite free. The vomiting and sense of "obstruction" were relieved by the operation; but the patient died thirty-six hours afterwards, in a state of collapse, no hiccough having occurred, and the bowels not having acted.

Autopsy.—I regret that a thorough post-mortem investigation was not permitted, and that I was compelled to pledge my word that none other than the parts previously operated on should be examined, and that these should not be removed from the body. Dr. Booth assisted at the examination. To the eye and to the touch there appeared to be three testicles, two in the scrotum and one retained in the left groin. The inguinal canal and groin were now laid open. The proper inguinal canal contained, apparently, merely the cord, and was lined with a layer of recent lymph. In the groin was a tumour coated with lymph, which lymph was found to conceal a thin layer of blood-vessels and (what looked like) nerves, connected by a very fine membrane. This layer passed into the scrotum. The tumour proved to be an undescended testis, with its epididymis and cord. So far, then, with the exception of this nervo-vascular (?) membrane, the relation of parts was such as would ordinarily exist in a case of congenital inguinal hernia prior to the descent of the testis into the scrotum, and prior to the occlusion of the processus vaginalis from the general peritoneal cavity. The testis and epididymis were quite healthy, and neither of them wounded. No doubt "the fibrous membrane" covering the testis, which I divided on the occasion of the first operation, was the peritoneum, or, to speak strictly, the processus vaginalis, thickened as a hernial sac becomes thickened when long exposed; and the filaments which I evacuated, and mistook for "coni vasculosi," were a portion of the nervo-vascular (?) membrane which I have alluded to. I explain the reason of my not exposing the testis thoroughly at either of the operations thus:—The incision through the skin was a long one (and subsequently enlarged by a shorter one made at an acute angle.) The deeper incisions were confined to the *upper portion of the wound*, and the intestine was reached without opening any sac of which I was cognizant. This, of course, is usual in congenital hernia. Having returned the gut thoroughly, the testis felt (through the remaining undivided layers of tissue) like a distinct tumour, as indeed it was, and I did not feel justified in doing more than making such a puncture or (if necessary) incision as should satisfy me as to its nature. I felt satisfied that the filaments which exuded on making this puncture were "coni vasculosi," and so made no further dissection. I was wrong, and no doubt my explanation may not be considered satisfactory. I

rest satisfied with having done my duty to the best of my ability. It is impossible to place "old heads upon young shoulders" with any prospect of forming a successful combination. The testis was now removed from its position. Behind it we found a corrugated, crumpled-up, thick substance, coated with fibrine. The canal became contracted immediately below the testis, so as to form a neck, through which the substance just named passed, as also the film from the front surface of the testis. The finger could be passed through this contracted portion of the inguinal canal into the scrotum, and here the canal expanded into a globular cavity, which was filled with some membranous substance, which we presently examined. We laid open the scrotum, and exposed a glistening, oval, fibrous body, about the size, and having really somewhat the appearance, of a testicle. It had a neck too, which during life, and even on tactile examination after death, passed very well for a spermatic cord. We opened this curious body, and found it to be a strong fibrous envelope, about the eighth of an inch in thickness, lined with recent lymph, and containing a globular something, also coated with lymph. This indefinite something we found to be an undeveloped, highly vascular portion of omentum, crumpled up like a veil into a roundish mass, with a process or neck passing from it through the contracted portion of the capsule; the main portion then passing behind the testis, and becoming firmly attached to the spermatic cord and to the posterior wall of the inguinal canal, whilst a thin film passed in front of the testis, and contributed to form the "coni vasculosi," which I evacuated at my first operation. The fibrous envelope became much thinner at its neck, and gradually lost itself in the lining of the inguinal canal.

Remarks.—I take it that in this case the communication between the general peritoneal cavity and the processus vaginalis lining the inguinal canal and scrotum had never been closed; that a portion of omentum passed during fœtal life into this vaginal process prior to the descent of the testis; that the fibrous envelope consisted of peritoneum, fascia transversalis, and intercolumnal fascia, blended together and developed into a strong covering for the protection of the exposed portion of the peritoneal cavity and of the misplaced omentum; and that the testis remained in the groin, because its arrival in the scrotum had been anticipated and its place occupied by the protruded omentum. The canal of the processus vaginalis, when examined after death, was found to be contracted, first, at the internal abdominal ring; secondly, just above the testis; and, lastly, at the point where it formed the neck of the scrotal tumour. The contraction at the internal ring no doubt explains the reason why the hernia of the gut was not constant. I would again allude to the omentum. Is it possible that the testis passed, during fœtal life, between the layers of the great omentum? It certainly was between them when we examined the body. The appearance, too, of this omentum was very deceptive and peculiar. The thin film in front of the testis (in the groin) looked just like a number of veins and nerves connected by fine fibro-cellular tissue, but coated on the external surface with soft, recent lymph. Posteriorly, this fine, vascular membrane was, as I have said, corrugated and crumpled up into a rounded substance about the size of the little finger, which expanded above and became firmly connected with the spermatic cord and the wall of the inguinal canal, and likewise expanded below so as to form the globular mass found within the fibrous capsule in the scrotum.

I must again express my regret that we were not allowed to open the abdomen; and I shall be most happy to afford information on any points connected with the case which my readers may think it worth while to inquire about.—*Lancet, June* 15, 1861, *p.* 583.

57.—PERFORATION OF THE INTESTINE.

By MM. Velpeau and Verneuil.

When reporting the discussion which recently arose in the Society of Surgery, on the course to be adopted after the operation for hernia, when

the intestine is perforated, we venture to assert that whatever the extent of the perforation, the reduction of the intestine with or without suture must be deemed irrational; nor are we disposed to think that the exceptional instances adduced in France and England in support of a contrary opinion can invalidate the teachings of reason, backed by observation.

At the meeting of March 13th, M. Velpeau related five cases in which he had returned the perforated bowel into the abdomen with success. In the patients cured by this method, the eminent Professor stated that the intestinal convolution was healthy, and presented but a minute aperture. In one instance the perforation had probably been consequent on tractions exercised on the incarcerated intestine, and a second case was followed by stercoral fistula, which closed spontaneously. Notwithstanding these felicitous results, M. Velpeau did not present, as an example to be followed, the course he had adopted under a fortunate concourse of circumstances which may not again be met with. "Indeed," said he, "one consideration must not be lost sight of: the operation for strangulated hernia may be descanted on at much length, all the known cases may be brought forward, and all probable incidents as far as possible delineated; and yet it will remain impracticable to lay down absolute rules for the guidance of the surgeon. Each operation presents its own special difficulties, which the practitioner is called upon at once to decide on. No invariable maxims can be propounded applicable to all cases; each stands by itself, and requires to be considered separately."

M. Velpeau, it may be seen, is but moderately partial to artificial anus. He conceives it is much too lightly spoken of. In many instances when well established it closes spontaneously; but in many also it becomes an incurable infirmity, not to mention the disastrous occurrences which are occasionally observed before consolidation is effected. We should not slur over the disadvantages, or even the perils, attendant upon the formation of artificial anus, and we cannot look upon this method as the final expression of surgical progress. But we must not forget a view which is, for the present, paramount; namely, that of the various primary or secondary contingencies which endanger the life of patients after the operation for strangulated hernia, when the bowel is perforated, the most immediately formidable is stercoral effusion. Hence the precept of non-reduction is, unquestionably, that which is most applicable to the greater number of cases, and must continue to prevail until a careful inquiry into the instances of exceptional success resulting from the opposite procedure shall have thrown light upon the circumstances which may, in such cases, lead to a cure without resorting to the formation of artificial anus.

The necessity for this aperture being now granted, its establishment must be proceeded with so as to meet the objections which have been raised to the method; and none better than M. Verneuil has pointed out the indications to be satisfied under the circumstances.

"The intestine must be secured so as to obviate its sudden return into the abdominal cavity, and to permit it, on the contrary, to retrocede by the slow and gradual mechanism instituted by nature in fortunate cases. Both extremities should be fixed in appropriate relative positions, were it but to promote their future union, or to enable the surgeon to discover them again, without difficulty, for the purpose of anaplastic operations, should they not have a tendency to unite by the unaided agency of nature.

"The whole of the suspicious portion of the bowel should be left in the external wound, and there secured, whether the perforation occupy the summit, the body, or the pedicle of the convolution; hence the necessity of a most careful exploration of the intestine, and of exposing the part which has suffered constriction.

"Not only should no perforated or mortified spot be returned into the abdomen, but if any portion of the bowel appears to be in a state of vitality, it should not be reduced, as it might subsequently give way. As the distance to which gangrene of the intestine may extend around a perforation or a mortified spot is uncertain, it is always safer to keep it in the external wound, and there fix it with every, even apparently unnecessary precaution.

"The extent of bowel maintained outwardly should be such as to leave between the peritoneum, at the back of the ring, and the artificial aperture, a distance sufficient to obviate all infiltration, and to prevent the inflammatory action, which must occur in the wound, from spreading to the serous lining of the abdomen.

"The intestinal aperture should be adequate to the free escape of the contents of the intestine, the obstruction of the bowel being the chief danger of strangulation, and the essential cause of the local or general symptoms, and the efficacy of the artificial anus being entirely proportionate to the freedom of the evacuations.

"Therefore, without taking into account the contradictory and exclusive precepts of authors, the surgeon will use the knife, guided merely by the view of securing the free egress of the discharges. Incision of the ring will, in most instances, be useful for this purpose, and also for the complete inspection of the strangulated convolution and the proper securing of the intestine in the wound.

"The greatest precautions are indispensable during the operation to prevent passage into the abdomen of stercoral matter, injurious gases, and gangrenous fluids. The external wound will be kept open, and proper attention must be paid to the unimpeded issue of the contents of the intestine during the necessary time."

These precepts show that many of the procedures hitherto recommended for the treatment of strangulated hernia with gangrene of the intestine are imperfect and unsafe. That which M. Verneuil conceives to be preferable, consists in the suture of both ends of the intestine, or of the margin of the perforation to the wound of the skin, the diseased portions being removed or not as the case may require. As this plan would, however, increase the chances of permanency of the artificial anus, it would be advantageous to prevent, if not the formation of the spur, the size of which depends upon the extent of the destruction of the intestine, at least the union of the mucous membrane of the intestine with the integument. For this purpose M. Verneuil advises a margin, destitute of epithelium or epidermis, to be left between these parts which may granulate, become gradually contracted, and finally bring about the occlusion of the abnormal orifice. Careful dressings and, if necessary, superficial excisions would doubtless be further conducive to this desirable result.—*Journal of Practical Medicine and Surgery.—Med. Circular, Aug.* 21, 1861, *p.* 135.

58.—ON SOME DISEASES OF THE RECTUM.

By James Lane, Esq.

Mr. Lane drew the attention of the Harveian Society to the subject of hemorrhoidal disease, his remarks being founded on the experience obtained during a three years' connexion with St. Mark's Hospital for Diseases of the Rectum. Subdividing these complaints in the usual manner into external and internal, he commenced by briefly describing the nature of the two conditions, giving his views with respect to the structures involved, and an account of the leading symptoms in each. In speaking of the treatment of swellings situated external to the anus, he said that when they were caused by the rupture of a dilated hemorrhoidal vein, the best and most effectual remedy was an incision, to allow the coagulum to escape; and that when they were formed by enlarged and inflamed portions of skin, excision with knife or scissors was the only method which could be depended upon for effecting a permanent cure. He strongly condemned the use of the ligature, which, notwithstanding all that had been written on the subject, he still found to be not unfrequently employed in such cases. With reference to the *curative* treatment of internal hemorrhoids, two principal methods had been practised—excision and the ligature; to which might be added a third—namely, cauterization with nitric acid. Excision, which was formerly the common practice, possessed many and great advantages: the tumours could

be readily, rapidly, and completely removed; the operation was attended with less pain, both at the time and subsequently, than was occasioned by the ligature; it produced less irritation, and was followed by more rapid recovery. It had, however, one very serious disadvantage—the liability to dangerous hemorrhage, enhanced by the great difficulty in getting at the bleeding surface to arrest it. This danger had been felt to be so great as to outweigh all its other advantages, and had led to its almost complete abandonment in the present day, and to the substitution of the ligature in its place. In every respect, however, except in avoiding the risk of bleeding, the operation by ligature was inferior to that by excision. Such being the case, it would be admitted to be a great desideratum if an operation could be contrived which should combine the principal merits and avoid the principal demerits of both. Such an operation had been devised by Mr. Salmon, and had been practised by him with great success for many years. He did not think this operation was by any means so generally known as it deserved to be, and one of his reasons for bringing the subject of hemorrhoids to the notice of the society was for the purpose of explaining its nature. It might be said to be a sort of compromise between excision and the ligature. It consisted of a separation of the hemorrhoidal tumour from the subjacent parts for about the lower three-fourths of its extent, leaving it attached by the remaining upper fourth, which was then included in a ligature. The tumours were drawn down by means of a hook with four prongs, contrived for the purpose, and the division of the lower part of them was made with scissors. These structures were always supplied by vessels which descended from above, close beneath the mucous membrane, and the trunks being necessarily included when the upper part of the tumour was tied, all danger of bleeding from the divided branches was avoided. By this proceeding, the smallest possible amount of tissue was included in the ligature, and what was included was as far as possible removed from the anus. This latter circumstance he believed to be a great advantage, for the membrane an inch within the anus possesses little of that acute sensibility which characterizes the integument close to that aperture.

The operation being thus far completed, another point of importance was the management of the œdematous and everted collar of integument which always surrounds internal hemorrhoids when they are protruded. The protrusion from within is necessarily accompanied by an eversion of the skin with which it is continuous, and it by no means follows that there is any superfluity of this latter tissue. To remove all the apparently superfluous skin would be followed by a greater evil than the disease itself—an irremediate contraction of the anal aperture after cicatrization had taken place. The rule which he had found it desirable to follow was, to remove all irregularities and all pendulous portions of skin freely, but not to meddle with the circular fold which surrounds the protruded parts. It might be expected that inconvenience would in like manner result, in the interior of the rectum, from the free removal of mucous membrane, which was a necessary part of these operations; such, however, he had not found to be the case. He had, indeed, in some few cases, where the disease was extensive, seen a decided narrowing of the lower end of the rectum for some weeks after the operation; but it had in every instance gradually subsided, and all inconvenience had disappeared. The cicatrices of mucous membranes did not appear to have that tendency to contract, or to remain firm and rigid, which was found to be the case with cicatrices of the skin. In Mr. Salmon's operation, the adoption of which he strongly recommended, he had found the subsequent pain and irritation were very much lessened; and in evidence of this he mentioned that he had very rarely found it extend so as to involve the urinary organs; whereas retention of urine, according to his experience, was the rule, rather than the exception, after the ordinary operation with the ligature.

A circumstance which he thought well worthy of note was the very slight risk attending operations on the rectum, if anything like proper care were taken in the selection and management of cases. He had during the last

three years and a half performed 166 operations for hemorrhoids, 202 for fistula, and 97 for fissures, polypi, and procidentia—465 in all. He had assisted at about an equal number in the practice of his colleague, Mr. Gowlland, making a total of upwards of 900 operations, the results of which had been carefully recorded, Amongst them there had not been one instance of pyæmia, and not one instance of erysipelas; in fact, excepting in four cases, there had not been a single complication at all of a serious character or deserving of notice. These four exceptions were cases of tetanus, which occurred in the spring of 1858, within a very short period of each other. Two had occurred in his own practice, and two in that of Mr. Gowlland. They all followed the operation for hemorrhoids, and all four were fatal. He thought, however, that he was justified in considering them as accidental, especially as numerous deaths were occurring from tetanus in other hospitals in London at the same period, and the disease might therefore fairly be said to have been epidemic at the time. In one hospital which he mentioned, nine deaths had taken place from tetanus within a period of two months. It had never been noticed by other surgeons that operations on the rectum were especially liable to tetanus, and in his own subsequent experience, which extended to several hundred operations, he had seen no other example of it. He thought there were few operations in surgery which afforded with so much certainty such complete and permanent relief from serious disease at so small a cost of suffering and risk.

With respect to the treatment of hemorrhoidal tumours with nitric acid, his experience did not lead him to recommend it for general adoption; but neither was he disposed to condemn it indiscriminately, as some of its opponents had done. No doubt much injury might be caused, and had been caused, by its careless employment; but he had himself used it in twelve cases, and his objection to it was rather on the score of its inefficiency than on any active injury which he had found to result from it. He believed it to be well adapted for those cases in which there was an abnormally vascular condition of the mucous membrane of the rectum, without the development of large or distinct tumours which could be protruded; but, under the opposite circumstances, repeated applications of the acid were necessary to reduce the diseased parts to the level of the surrounding surface, and the treatment then occupied a much longer time, was much less likely to be permanently successful, and was in reality much more severe than the operation by the ligature, or that modification of it which he had recommended.

Mr. Brown had tried the plan of tying internally hemorrhoids as recommended by Mr. Lane, and although it possessed many advantages, still he had occasionally found considerable contraction of the anus after their removal. He (Mr. Brown) preferred simply dividing the skin where it joined on to the mucous membrane before applying the ligatures; carrying out, in fact, the golden rule laid down by the late Mr. Copeland, "cut skin, tie mucous membrane." Mr. Brown always uses ligatures of twine in preference to silk. With respect to the observation of the author that there was more bleeding from hemorrhoids in women than in men, Mr. Brown explained that it arose frequently from a suppressed action of the menstrual discharges, or from the cessation of that discharge at certain changes of life. Mr. Brown could confirm the statement of the author as to the safety of all operations of this kind, as he had performed a large number, amounting to many hundreds, and had never met with a single death or a case of pyæmia.

Mr. Lane did not altogether concur in Mr. Copeland's maxim,—always to cut skin and tie mucous membrane. He would always cut both were it not for the danger of bleeding; and he thought the next best thing was to cut as much as was safe and tie the remainder. He believed it to be sound surgery never to remove tissue with the ligature when it could be safely done by a cutting operation.—*Lancet, August* 3, 1861, *p.* 112.

59.—ON THE TREATMENT OF CERTAIN FORMS OF PROLAPSUS ANI WHERE THE LIGATURE IS NOT ADMISSIBLE.

By Henry Smith, Esq., Assistant-Surgeon to King's College Hospital.

In the 'Med. Times and Gazette' for Nov. 17, 1860, are some observations on the most efficient means of treating severe cases of prolapsus of the rectum where the ordinary method—by the ligature—could not be adopted. I entered at some length into the pathology of the disease, and described the varied conditions under which it is so frequently met with. It was particularly insisted upon that prolapsus of the rectum was associated, in many instances, with some serious organic mischief, and that the use of the ligature in such cases would be attended with considerable danger. In lieu of the ligature I recommended the combined method of treatment—viz., the application of the nitric acid to the prolapsed membrane, and subsequent removal of tranverse folds of integument from around the margin of the anus. I purpose now to detail a few cases where this plan was carried out:—

A gentleman, aged 54, was sent to me on Dec. 7, by Mr. Fereday, of Dudley, who, in a letter which the patient brought informed me of the existence of some organic disease, and the appearance of the gentleman at once indicated something more serious than a prolapsus of the rectum. however serious. There was a lividity of the face, and an amount of dyspnœa, which betokened something wrong with the heart or lungs. He informed me that, for several years. he had suffered from prolapsus of the rectum, which had been preceded by hemorrhage for some time. This symptom, however, had passed by. During the last two years his prolapsus had become so much worse that he was in almost continual suffering, the bowel not only protruding at the closet but coming down when he began to walk about. The pain after his evacuations was very severe, and he was not always able to control them. Moreover, as soon as he returned the protruded bowel it came down again, and the consequence was, that he could not sit without pain, and has latterly been rendered quite wretched from this cause alone, without taking into account his other ailments. On examination, after the exhibition of a copious injection, I found a large quantity of the mucous membrane of the rectum protruded. It was highly vascular, somewhat thickened, and in a granular state. There were no distinct hemorrhoidal tumors. Around the prolapsus externally was a large quantity of thickened and relaxed integument, and the anal orifice itself was very capacious, and the sphincter was much weakened.

On examining him generally I found that he suffered, on slight exertion, from difficulty of breathing, and that the action of the heart was very irregular, the pulse being frequent, weak, and intermitting. He informed me that, not long since, he had suffered from œdema of the legs, and that his liver had been much enlarged, but that both these latter symptoms had become much decreased. It was hoped that, as his general health had been improved, the prolapsus would give less trouble, but the contrary took place, and accordingly Mr. Fereday submitted the case for my opinion as to how far in such critical circumstances, any operative proceeding would be justifiable.

I informed the patient at once that the prolapsus was so severe that the only mode of treatment which could effect a cure was the ligature, but that in his damaged condition I could not recommend it, as it would of necessity be attended with considerable danger to life, but that I would adopt a proceeding which would, in all probability, give great relief, and which would be unattended with danger. I, however, informed him that in such a bad case he must not expect the same cure as would follow the ligature. He determined to undergo the treatment proposed, and accordingly on the same evening I gave him an injection, and having got the prolapsus membrane well down, applied the strong nitric acid to it very carefully.

9th. He suffered pain for two or three hours after the acid had been applied, but on evacuating the bowels this morning the pain was gone,

and the protrusion was less. I therefore applied the acid freely again this evening.

11th. A severe attack of vomiting and retching—to which he has been subject—followed the application of the acid, but it has subsided, and the protrusion is less.

13th. This day I removed from around the anus the thickened folds of skin and mucous membrane, by making four separate incisions at right angles to the orifice, dipping the points of the scissors well inside the sphincter; a good deal of oozing of blood followed this operation, but it was arrested by returning the protruded membrane well within the sphincter.

16th. The bowels were opened to-day by castor oil, only a slight protrusion occurred, and it was readily replaced.

19th. The bowels having been well acted upon by castor oil, and the mucous membrane having been protruded, I again applied the nitric acid. I find the wounds made in the operation are nearly healed.

22nd. I applied the acid again.

24th. The bowels were operated upon naturally the first time this morning—the protrusion returned almost immediately by the pressure of the sponge; the wounds made by the scissors are quite healed.

26th. I applied the nitric acid for the fifth and last time to-day, after the bowels had been well acted upon by castor oil, the protruded membrane returned immediately with the least pressure, the sphincter ani contracts in a much more healthy manner. He is to return into the country in a day or two. I have desired him to take a dose of sulphur in milk at night in order to get a regular action of the bowels, to bathe the anus with the sulphate of iron lotion, and to pass a bougie every day after an evacuation.

In a month after his return, this gentleman wrote to me, "I have much cause to be thankful for the freedom I already enjoy from sufferings so long before endured," and two weeks afterwards he again wrote to me saying, "You will, I am sure, be glad to hear that since I last wrote to you I have several times walked from one to two miles without causing any return of my recent complaint;" and after referring to the annoyance still caused by his dyspnœa, he says, "I am, however, quite a new creature;" and with regard to the object aimed at by the treatment of bracing up the parts and getting the sphincter to a more healthy condition, the following remark shows its efficacy. It refers to the bougie, after the employment of which he says, "The immediate contraction of the parts is so great that I have some trouble to get the bougie out again."

It is impossible, by any description, to give a fair picture of the distress which was caused to this poor gentleman by the constant and continued descent of the bowel, and therefore the relief of which he wrote in such thankful terms was especially gratifying and striking; and I could not bring forward a case better calculated to illustrate the importance of having some means as a substitute for the ligature where this treatment could not be put in force without great danger to life. It is a maxim recognised, I believe, by all surgeons of experience, that when there is any organic disease existing either in the viscera of the abdomen, in those of the chest or in the brain, the operation of the ligature should not be performed; for whatever be the explanation of the matter, there is not a doubt that the stimulus of the operation sets up an increase of disease in the organs already affected, and may destroy life. A remarkable instance of this I mentioned in my last paper, and I think it very likely that had I applied the ligature in the case just detailed, some fresh disease would have been excited in the liver or heart, and a fatal result might have ensued. Had not we possessed any other means of remedying the very severe prolapsus, this patient must have lived in misery, and have been content with the use of mere palliatives which had already been exhausted upon him.

There are one or two details connected with the treatment of this case which it may be well to allude to more particularly. It will be seen that I applied the nitric acid both before and subsequent to the removal of the pendulous folds around the anus. I generally recommend the nitric acid to be

used first, and the treatment to be finished by the employment of the scissors; but this gentleman, although he bore the treatment with courage and patience, had a great dread of that part of this proceeding, and therefore I hurried it on more quickly than I generally do. It is better to avoid using the nitric acid afterwards if possible, because the wounds made by the cutting instrument are likely to be disturbed and re-opened by the attempts of the patient to bring the bowel down and by the manipulations of the surgeon; but if there be a doubt about the matter, it is much better to apply the nitric acid on one or two occasions after the use of the scissors than to leave any of the protruded membrane thoroughly untouched. This lesson was forced upon me by a very unpleasant circumstance in my practice, where I treated a severe prolapsus on the same plan. I applied the nitric acid on one or two occasions before the loose folds were cut away, and as I thought sufficiently, and did not apply it afterwards. The consequence was, that in a few days after the treatment was, as I supposed, properly finished, some of the symptoms returned nearly as badly as before, much to the annoyance both of my patient and myself. This accident, however, happened before I had the experience of the treatment I possess now. If the nitric acid be applied with care, it gives but little pain and does no mischief. It therefore may be used on several occasions, if necessary, without rendering the treatment very severe.

There is one other point to which I wish to allude, and that is as to the use of the bougie after the treatment was finished. This is a most important matter to attend to, and should not be neglected either after the use of the ligature or after the plan I adopt; its daily introduction after the evacuation of the bowels assists in replacing any portion of membrane which might be protruded, and the pressure of the instrument exerts a most beneficial action on the parts, preventing return of the disease. The use of a bougie is also beneficial in this way, it prevents the possibility of too great contraction of the anus occurring. This stricture might occur if the operation is not done with care, or if too much integument is cut away; and in my work on the rectum I have mentioned a case where an ill-judged and badly-executed operation was done by a Physician-Accoucheur, and was followed by severe contraction of the anus. Had a good sized wax bougie been used in this instance for some time after the operation, this sad mishap would not have occurred.

I will now detail another instance showing the value of my mode of treatment where the ligature would have been out of the question.

I was requested by Dr. Tunaley, on January 6, to visit with him an old naval officer who was in his eightieth year, in very good health with the exception of a prolapsus of the rectum, with which he was confined to his bed. On examination I found a considerable tumour consisting externally of thickened and pendulous folds of skin, and internally of flaps of mucous membrane prolapsed in a very vascular state and not thickened. The history of this case was like that of many others: he had had the disease for many years, and had worn a very well made truss which generally kept the bowel up, but a few days before I saw him, as he was walking out, the bowel prolapsed by the side of the truss and he could not return it. Severe symptoms came on, he was treated by Dr. Tunaley for some days, and the parts having become more quiescent it became a question as to whether something effectual might be done. At such an advanced age it was of course out of the question to apply the ligature, but I stated to Dr. Tunaley my views about the combined operation with the nitric acid and scissors, and it was determined to put the treatment in force. Accordingly, the bowels having been previously well acted upon, I applied the nitric acid to one portion of the prolapsed membrane: the application produced scarcely any pain at the time or afterwards.

8th. The nitric acid was applied freely to the other portion of prolapsed membrane; this was not productive of any suffering.

10th. The acid having been found to have acted well in diminishing the prolapsus, I first benumbed the part with ice and salt, and then removed four flaps of the thickened integument from around the anus.

12th. Patient has only suffered very slightly. Wounds are healing well, and there has not been any prolapsus.

15th. Bowels have been well acted upon by castor-oil without the occurrence of any prolapse. The wounds have almost entirely healed. I recommended him to take an electuary occasionally, and to pass a wax bougie three times a-week.

26th. I heard from Dr. Tunaley to-day to the effect that our patient was quite well, and had had no unfavourable symptom whatever.

I have narrated this case because it illustrates the value of the treatment adopted where a condition obtains, viz. that of extreme old age, which is generally considered by most surgeons as forbidding the employment of the ligature. Had there not been some other means at our disposal this old gentleman, who was for his time of life a remarkably healthy man, would have been compelled to suffer great discomfort for the rest of his days; for it will be seen that his truss had failed to give him that relief which it had done for some time. And I do not think that any surgeon would like to recommend the employment of the ligature in a person eighty years old, however free from disease his internal organs might be; but the combined proceeding of nitric acid and the use of the scissors is so free from danger that it may be recommended with the greatest confidence even in persons of very advanced age. It is true that sometimes it will happen that we are obliged to submit our patient to the ligature even when they are at a time of life when such a proceeding becomes very hazardous; but there are exceptional circumstances which render it necessary.

It will be seen that the instances detailed above were uncomplicated, though severe instances of prolapsus of the rectum, and about which there cannot be any doubt in the minds of those surgeons who have witnessed this treatment, as to the beneficial results which will ensue from it when undertaken with care. But cases will every now and then present themselves, in which the prolapsus is complicated with the existence of hemorrhoidal tumours, and, indeed, seems to be entirely produced by their mechanical weight. In such a case there is really some difficulty in knowing what to do when there is a contra-indication to the use of the ligature in the existence of some organic disease or old age. But I am glad to say that even in such a case I have found the proceeding adopted in the uncomplicated instances followed by very beneficial results.

A gentleman, aged seventy, consulted me on June 7. He informed me that he had suffered with piles and prolapsus for seven years, and that he had tried various remedies, but latterly the disease had increased, and become very inconvenient, in consequence of his not being able to return the bowel properly, so that, immediately he began to walk about, protrusion took place. On examination, I found that a protrusion was easily produced by the patient straining, and that it consisted of a large portion of the mucous membrane on either side, but slightly changed in its character. In addition to this, and just within the sphincter, were two or three distinct hemorrhoidal tumours, evidently of old standing. They were of a blue colour, chiefly venous, and there was no tendency to bleed. The anal orifice was very capacious, and the integument around was loose and abundant. This gentleman would not hear of the ligature, so I told him that I would do my best to get rid of the prolapsus at all events, but that I was afraid I could not destroy the hemorrhodial tumours. He readily acceded to submit to the treatment proposed, and accordingly, on the same evening, I applied the nitric acid to a portion of the prolapsed membrane.

June 11. The acid caused no pain, so I applied it to another portion of the diseased membrane.

15th. No suffering produced. He says the prolapsus is less, so I applied the acid again.

19th. I applied the acid again freely.

22nd. Prolapsus less. I this day excised from around the margins of the anus, three triangular folds of loose skin. There was no bleeding, and in the course of a few days the wounds healed up. He was for some little time

a good deal annoyed by the hemorrhoidal tumours descending whilst the bowels were evacuated, and getting nipped by the now tightly contracting sphincter; but he wrote to me as follows, a month after his return into the country:—"The prolapsus seems cured, at all events it causes no inconvenience. The piles are frequently troublesome after much walking, but less so than formerly; and they are either considerably smaller, or at least they do not protrude so much. I am sure, sir, that you will be gratified to learn that your skilful treatment has been attended with results so far good, and as far as I can see, likely to be lasting."

Now, I must confess that this report was more favourable than I could have anticipated, for although I expected a diminution of the prolapsus and a more natural condition of the parts altogether, I did not think I should hear that the prolapsus seemed cured, for the hemorrhoidal tumours not being of that nature which would be remedied by nitric acid, and not daring to cut them off, I saw that they would probably hinder the good results of the treatment, but although my anticipations were to a certain extent verified, the words of the patient himself are the best evidence, that a very considerable amount of benefit had ensued from the remedial measures employed. The chief part of this benefit was, I doubt not, produced by the removal of the loose folds of skin around the anus, as the orifice was remarkably capacious.

It must be understood, however, that I do not recommend this plan of treatment in such cases of severe prolapsus attended with distinct hemorrhoidal tumours, for unless the latter are got rid of the prolapsus in general will persist; still, if the orifice of the anus is very capacious, and the integument around it is very relaxed, the operation of removing three or four transverse slips subsequent to the employment of the nitric acid will produce so much contraction of the parts, that the prolapsus may be almost entirely removed.

If space permitted, I could relate other cases where the combined treatment by nitric acid and subsequent removal of the loose tissue around the anus had been attended with excellent results; but I think it is not necessary to go into the particulars of other cases, and I will conclude my observations by a few directions as to the detail of the treatment, and the conditions under which it should be applied. And in the first place I must again distinctly state that my object is not in any way to discard the ligature in those cases where there is no contra-indication to that proceeding. I look upon it as most efficient, productive of remarkably little suffering and danger when properly performed, and giving much less trouble to the surgeon than the other plan; but when, from the causes mentioned, ligature cannot be adopted, the treatment in question may be put in force. In order, however, to obviate failure, which is very likely to occur, attention must be directed to one or two points. It will be useless to attempt to do any good with nitric acid in those cases where the prolapsed membrane has become much indurated and thickened, or where conjoined with the prolapsus, there are two or three large hemorrhoidal tumours of a blue colour, prominent and well defined. If the ligature is not admissible, or the patient will not submit to it, the best plan will be, in such a case, to trust to the effect of removing the folds around the anus, but the incisions must be more extensive, and half an inch or more of the diseased mucous membrane should be removed in at least four different directions, and as Mr. Syme tells us, this operation alone may be attended with effectual relief, but when the mucous membrane is very voluminous, and the muscular tissue of the bowel is prolapsed as well, failure is likely to follow this method. In such a case the only thing to suggest is, that the patient should wear a well-made pessary, which will give great comfort.

When the mucous membrane is not indurated, is highly vascular, and readily bleeds, however voluminous it may be, the nitric acid, if sufficiently and carefully used, will almost unfailingly do good, sometimes the prolapsus will quite disappear; whilst in others the only symptom of benefit is the readiness with which the part is reduced by the patient after the bowels

have been evacuated. When this is the case, we may be certain that the remedy is acting well, and it should be repeated until the whole diseased membrane is touched.

The second part of the operation, consisting in the removal of the loose folds around the anus, requires great care and nicety. It should be practised a few days after the last application of the acid in the following manner:—Whilst the patient is kneeling on a chair or lying on the side of the bed, whichever gets the best light, the surgeon seizes a fold of the loose integument with either a pair of broad-bladed dressing forceps or an ordinary hooked forceps, about half an inch from the anus, and holding the fold well up with his left hand he takes a very sharp pair of scissors, curved on the flat, and beginning his incision from the outside, excises the part, carrying the point of the scissors well into the anus so as to remove portions of mucous membrane as well as skin—thus the incision is made transversely and not longitudinally, as recommended by Mr. Curling and Mr. Quain. I generally make three or four such incisions. The effect of this is to produce a healthy and permanent contraction of the anus and lower part of the rectum, so that the good result already produced by the nitric acid, and which is not sufficient of itself to remedy the disease when severe, will be greatly enhanced, and nearly as good a cure will follow as if the ligature had been used. What will not take place if only one of the measures is adopted, will be produced by the combination of the two methods—just as in like manner we find that one drug, however powerful, will fail to produce any good effect, whilst a combination of two or more possessing similar qualities will accomplish the object desired.—*Med. Times and Gaz., Sept.* 14, 1861, *p.* 266.

60.—ON FLAT ULCERS OF THE RECTUM.

By William Coulson, Esq., Surgeon to St. Mary's Hospital.

Ulcers and fissures of the rectum are amongst the most painful of the minor affections with which surgery is concerned. Where, however, any difficulty exists in such cases, it usually arises in the diagnosis rather than in the treatment. Commonly, when the seat of fissure is discovered, the division of the fibres of the sphincter by the knife offers a ready means of cure. In cases of flat ulcer, however, the same result may be obtained by milder means, as in a case treated this year by Mr. Coulson at St. Mary's Hospital. The patient was a man aged 26 years. He had been the subject of piles for some years, which he attributed to an early habit of taking strong purgative medicines. Three weeks before admission he had felt a violent throbbing in and after action of the bowels; this was followed by the discharge of about an ounce of blood and continued tenesmus. These symptoms were renewed at each action of the bowels, until admission, the pain being severe, and lasting some hours. On examination, a somewhat deep and irregular ulcer was found by Mr. Coulson to exist in the mucous membrane within the sphincter. It was ordered that the bowels should be regulated and maintained in a condition of laxity by determinate doses of castor oil, that careful precautions for cleanliness should be observed, and that an ointment should be applied composed of a drachm of the nitric oxide of mercury to an ounce of lard. Under this simple treatment the patient made a rapid recovery, and was discharged, cured, at the end of a fortnight.—*Lancet, August* 17, 1861, *p.* 160.

61.—ON ABDOMINAL OR PELVIC ABSCESS.

By Frederick C. Skey, Esq., F.R.S., Surgeon to St. Bartholomew's.

[Probably all abscesses on a large scale, are rather the result of low, than of an exalted vitality. Accordingly, pelvic abscess commonly occurs in persons of impaired constitutions, reduced by low living, accidental illness, or

any debilitating cause. The general situation in which pelvic abscess occurs, is the iliac fossa, but it is situated within the general walls of the abdomen.]

It appears in the form of a firm, not necessarily a hard, swelling, very distinctly perceptible on pressure over the above region. If small, its presence is only readily detectable by comparison with the opposite fossa, into which the ends of the fingers sink on moderate pressure. In this respect, however, there is a difference appertaining to the varieties in the form and quantity of the contents contained within the abdominal walls, especially of fat, and the greater or less laxity of the walls themselves. Occurring in young women shortly after parturition, its presence is remarkably distinct. When large, the swelling is apparent to the eye as well as to the touch, and it extends across the abdomen towards the mesial line, and upwards in the direction of the umbilicus. In such cases the swelling is prominent, and as it increases in magnitude, it encroaches on the intestines, which are pushed across to the opposite side. In many cases pain is not a prominent feature, and when present, it is usually not severe, but is dull and aching rather than acute in character.

In its early stage I have known this form of disease to be mistaken for two other varieties of swelling—malignant disease of the pelvis, and scybalæ in the colon. From malignant disease it may be distinguished by the general uniformity of the swelling, and by the less serious constitutional signs of health undermined; and judging from liabilities, malignant growths of or from the pelvis are far more uncommon than chronic abscesses. These examples of pelvic abscess have presented themselves to my observation more commonly on the right than on the left side. I am not aware whether scybalæ collect more usually on that than on the left; but certainly they are more palpable and more readily detectable in the head of the colon than in the descending part of the intestine, which is placed in less proximity to the abdominal walls than that on right side. But scybalæ are limited in their relation to the front abdominal walls by the caput coli, and moreover are moveable, whereas the mass, which gradually resolves itself into the abscess, presents to the hand the sensation of a large and solid deposit, firmly fixed, and considerably larger than the intestine itself. The disease progresses very slowly, and often requires weeks for its development. As a general rule, it becomes soft, or, in other words, is converted into an abscess, which occupies its original site; in other cases the sac yields, and the matter extends in one or more directions, of which the most common is downwards along the track of the femoral vessels. I have seen three examples in which the matter made its way backwards through the sacro-ischiatic foramen, presenting the dimensions of a formidable abscess on the buttock, the upper part of the thigh, or over the region of the trochanter major. Occasionally the matter will extend across the abdomen behind the peritoneum, and take the course of the vessels of the opposite side, where it may point through the abdominal muscles above Poupart's ligament. In the case of a lady to which my attention was recently called, a large collection of matter formed underneath the integuments of the right iliac region. The abscess had been preceded by a large mass occupying the fossa beneath it for a period of many weeks. After having evacuated the sac, I detected an opening through the aponeurosis of the external oblique muscle sufficiently large to admit the end of the forefinger, through which the matter had escaped from the abscess below. The outer cyst contained about a pint of brownish pus, both in colour and consistence very unlike the fluid contained in a psoas abscess.

Two remarkable examples of this disease have been under my treatment in St. Bartholomew's Hospital within the last few months. The first was that of a boy about fifteen years of age, who fell down on the ice in January last, and struck his left trochanter severely. Considerable pain followed the injury, which was deemed rheumatic; and he was admitted into the above hospital and placed in a medical ward. While there, a swelling formed over the *right* iliac fossa, for which he was removed into a surgical ward, and came under

my care. He had an abscess pointing both above and below Poupart's ligament. The upper projection was very prominent, and threatening shortly to burst its way through the skin; but I preferred to evacuate the abscess through the lower opening. About six or eight ounces of matter escaped. For some days he appeared much relieved by the operation; but he gradually lost his appetite, became hectic, and died in three weeks. On examination, it was discovered that the fall had occasioned a rupture of the fibrous capsule of the left hip-joint, and fracture of the acetabulum. Matter had formed in this region, which had extended across the abdomen behind the peritoneum to the right side, and made its way through the abdominal parietes where I had punctured it. During life the boy did not complain of any pain or even discomfort on the left side; and what is worthy of remark is, that during the last week of his life he lay over entirely on his left hip.

When these pelvic abscesses are large and their progress towards maturity is unusually slow,—when they are placed deeply within the pelvis and the matter is bound down by the pelvic fascia, they seldom fail to involve contiguous bone; in which case they generally prove fatal.

All that we can hope for, all that the best resources of art can achieve, is to change the chronic into an acute abscess, to advance the formation of pus, and to compel the abscess to select that locality through which it can most readily discharge its contents on a surface of the body. To effect this the appetite must be improved, and gratified with as large a quantity of nutritious food as can be digested; force and vigour must be given to the pulse by means of stimulants,—and the capacity for stimulants in these cases of debility is very great,—while the lungs should be supplied with an ample quantity of fresh air for the thorough oxygenation of the blood.

If there be one therapeutic agent more valuable than another in promoting suppurative action, it is bark, and it should be given throughout the treatment in full quantities. At the earliest moment at which fluid can be detected near the surface the abscess should be freely opened. It commonly points through the abdominal muscles, but the rule equally applies should the abscess point towards the rectum, or, when occurring in the female, towards the vagina, or on the nates or region of the trochanter.—*Lancet, April* 27, 1861, *p.* 406.

ORGANS OF URINE AND GENERATION.

62.—ON INCISIONS FOR STRICTURE COMPARED WITH DILATATION.

By John Gay, Esq., Surgeon to the Great Northern Hospital, &c.

I believe with Mr. Syme, that a stricture, not of traumatic origin, rarely if ever becomes impenetrable to a skilfully plied bougie or catheter (I invariably use the catheter); and, moreover, that when once passed, the catheter, if its use be steadily and cautiously persisted in, and combined with appropriate general and local treatment, such as leeching, opium, steel, &c., will ensure as complete a cure as can be effected by any other known means.

The advantage of this method over external division is its comparative freedom from risk; and I cannot but regard the argument in its favour as irresistible when we come to reflect that, out of every hundred, according to received tables, on whom division has been practised, six individuals have perished. This rate of mortality would be insignificant if the disease itself were mortal; which is not the case at that period of life in which such a procedure could for a moment be justified; or, perhaps, if there were an equivalent in the number of actual cures. It is true, that strictures do occasionally reach such a pass as to lead their sufferers to "protest that life is no longer desirable"; still, I cannot think that such a protestation, independently of some killing circumstances about the disorder itself, becomes, in any case, an admissible plea for an experiment in the shape of a possibly fatal operation.

It may have been observed that I do not exclude external division from the list of stricture remedies, believing that the real aim of the surgeon should be to "utilise all"; but I do object to that estimate of the operation according to which it is regarded as a simple, safe, and effectual method of cure; and is, in consequence, adopted, as I have much reason to believe it still is by some, without either that regard to necessity, or that amount of caution which should ever be observed when life hangs on the issue.

Of the simple internal incision recommended by Mr. Thompson, I have had no experience; but, when confined to the constricting band or tissue, I can see no objection to it; believing that whilst, on the one hand, it can do no harm, it is very likely, on the other, to relieve congestion, inflammation, or irritability, and efficiently to prepare the way for what, in the end, will still be the "wedge" in the treatment, namely, persistent dilatation.

I come now to speak of dilatation, having long adopted Sir Charles Bell's view that "the permanent cure of stricture is to be accomplished by dilatation.....by such a stretching of the narrow part of the canal, that it grows large under the operation, and has no disposition to contract." ('Institutes of Surgery.')

I speak from some considerable experience, and that of the treatment of the worst forms of stricture, when I say that I have very rarely met with a case that has not been amenable to the use of the catheter, with such adjuvants as opium, leeches, steel, &c., when especial circumstances demand them.

Space forbids my treating of different kinds of strictures in detail, or their pathology; but it must be remembered that that idea of an urethral stricture is miserably imperfect, which reduces it to the simple narrowing of the passage. A stricture is a structural lesion; and is not only the consequence of some disorder of the urethral tissues, but becomes itself the centre, and, perhaps, the seat of disorders specifically its own, such as inflammation, congestion, hemorrhage, spasm, &c. And it is most constantly the case that the patients suffering from stricture do not become conscious of its existence, or at least do not awake to its importance until their attention is drawn to it by its having called into painful activity one or the other of these accessory forms of disease.

It is not, then, the stricture *per se*, except in urgent cases, that should first claim the surgeon's notice, but its incidental affections: and all mechanical treatment should be withheld until these are brought under control by appropriate management, or, if the delay can be borne, altogether overcome.

The most general causes of failure in the use of the catheter are, as far as I have seen:—1. Its inopportune employment; and 2. Impatience. To the first I have just alluded. With regard to the second, I should say that in no case should the catheter be used with any but the slightest force; and this should not be persisted in but for a very short time. It is better to make several efforts at intervals of one, two, or three days, than overcome the resistance of a stricture by one effort, if that should necessarily be a violent one, and such as would lacerate or do other injury to the urethra. Gentle and regularly repeated efforts will eventually ensure success in, I believe, the most aggravated cases. I am in the habit of observing two rules when attempting to pass a catheter through a stricture, of the greatest service. 1. Invariably to keep the convexity of that part of the catheter between the curve and the point well back against the posterior wall of the urethra, the point being tilted slightly forwards and upward at the same time by a finger commanding the handle side of its curve; bearing in mind the anatomical relation of the urethra to the pubic arch, as it passes through the triangular ligament; and 2. To desire the patient at the same time to make an effort to void his urine, whereby all resistance to the introduction of an instrument, on the part of the extrusor or detrusor muscles of the urethra, is overcome, and a channel, though ever so small, is, of necessity, formed.

In passing, I might remark that this is an invaluable test of the *existence* of stricture, especially in young subjects.

After the catheter has once passed through the stricture, it does not follow

that it will be admitted on subsequent attempts. These may fail from many causes, and may fail even again and again; but, after a time, the failures will be less signal, until, at length, they cease altogether.

There should never be a hurry to substitute a larger instrument for one that passes. Sometimes a small instrument will dilate, and that with permanency, more quickly than a large one; although, on many accounts, the latter is preferable to the former in the generality of cases. I am assured that, by the gentle but patient and persistent use of catheters, well adjusted, as far as their size and shape are concerned, to the capacity of the strictured passage, and to the normal or abnormal course of the urethra in its entire length, most strictures can be overcome, and the urethra restored to its original condition, with as much security against relapse as by any other plan of treatment that has hitherto been devised, and with less risk of injury to the patient; and that the call for the knife is exceedingly exceptional.—*Brit. Med. Journal, June* 8, 1861, *p.* 603.

63.—ON THE IMMEDIATE TREATMENT OF STRICTURE OF THE URETHRA.

By Barnard Holt, Esq., Senior Surgeon and Lecturer on Surgery at the Westminster Hospital.

[At first sight we should be afraid of the practice of Mr. Holt in *splitting up* a stricture by force, but the following paper and cases are important, and deserve serious consideration. He says:]

My attention was early attracted to the prevailing defects in the treatment of strictures. I could not but observe the tediousness of the treatment by ordinary dilatation, occupying many months before an average instrument could be introduced into the bladder, and that even when the dilatation was accomplished, the constriction generally returned, so that perpetual surgical care was required.

Being deeply impressed with the unsatisfactory nature of the prevailing methods of curing these distressing maladies, about seven years ago I adopted a more energetic mode of treatment, and invited the notice of the profession to a new "Stricture Dilator." Its use was at first limited to simple dilatation, which was readily effected by graduated tubes passed between the blades without the withdrawal of the original instrument. Experience, however, soon showed me that, as a general rule, when dilatation was carried much beyond the degree produced by ordinary bougies, "stricture fever" was induced. I therefore determined, though with some apprehension as to the consequences, *to split the stricture* by passing the largest sized tube at once, and thus immediately to enlarge the contracted part of the canal, so that it might receive a catheter equal to the entire breadth of the urethra.

Fearing the effects of the urine being permitted to come in contact with the laceration thus occasioned, I kept a gum elastic catheter in the bladder, but as this measure gave rise to considerable irritation, I determined to content myself with simply splitting the stricture, drawing off the urine, and not again using the catheter till two days after the operation. After that interval an instrument of the same diameter as that used at the time of the operation was again employed, and its use was continued, first on alternate days, and subsequently at longer periods of delay.

The course of experience has shown, indeed, that instances occur in which it is necessary to use a catheter one size less than that first passed after the operation, but such cases are exceptions. The instrument by which this simple process is accomplished consists of two grooved blades fixed in a divided handle, and containing between them a wire welded to their points, and on this wire a tube (which, when introduced between the blades, corresponds to the natural capacity of the urethra) is quickly passed, and thus ruptures or splits the obstruction. The simplicity of this apparatus is obvious to all, and the history of above a hundred cases proves that its use is unattended with any of those serious complications,—viz., hemorrhages, false

passages, infiltration of urine, perineal abscess, fistulæ, swelled testis, &c., &c.—which too often accompany the other operative process devised for the relief of these organs. The forcible distension caused by the dilator affects the morbid obstruction only; the healthy portion of the canal is not disturbed, the slightly vascular character of the strictural deposition and resisting tissue gives rise to but very inconsiderable bleeding, and the subjacent adventitious texture perfectly obviates infiltration into the surrounding cells. A perusal of the following cases, which are extracted as salient examples from a long series which were witnessed through their whole career by the students and surgeons who favour the practice of the Westminster Hospital by their attendance, will, I think, corroborate the points which I wish to establish, and I hope justify me in upholding as proved the following conclusions :—

1. The operation is of the most simple kind, and any one who can pass a bougie through a difficult stricture is competent to perform it.
2. It is not attended with hemorrhage, infiltration of urine, abscess, or any serious local mischief.
3. In the majority of instances the relief is immediate.
4. The occurrence of rigors or any other constitutional disturbance is very rare, and the patient is seldom confined to bed longer than from twelve to twenty-four hours.
5. The urethra is immediately made permeable by a catheter of full size, which may be ever afterwards passed at discretion.
6. This method is available in every kind of stricture where a canula of any size can reach the bladder.
7. When the after treatment is judicious and attentive, the full capacity of the passage is always maintained.
8. In all cases of neglected treatment the stricture yields to this method more promptly than to any other.
9. It being impossible that any but the diseased tissue can be divided, the splitting of the stricture has a decided superiority over any cutting operation.
10. And to sum up the great advantages in one proposition, the process is facile, speedy, prompt in effects, and free from every danger, immediate or remote. The course of general treatment will naturally vary, according to the kind of obstruction, the number of strictures, and the occasional complications of contracted bladder, enlarged prostate, fistulæ in perineo, false passage, &c. In simple stricture, however narrow, the relief will be immediate, but in the more complex forms of these maladies, the size of the stream is not so directly increased as might have been anticipated, from the immediate enlargement of the canal. Notwithstanding, however, the stream may for a short time remain somewhat restricted, the patient is able to empty his bladder much more quickly and effectually, and has less frequent micturition. The limitation of the jet evidently depends upon the thickening of the surrounding textures, and swelling of the mucous lining; these morbid states speedily subside, and in a short space of time the patient can void his water in a normal manner.

Case 1.—*Stricture of Twenty-five Years' Duration—Severe Constitutional Disturbance—Recurrence of "Stricture Fever" after each attempt to introduce a Catheter—Retention of Urine—Hemorrhage—False Passage—Operation—Recovery.*—Thomas W., aged 50, a labourer, of dissolute habits, was admitted under my care, on Nov. 5, 1857. He had been the subject of stricture for twenty-five years, during which period the stream has been gradually contracting, and the urine is now passed guttatim. Five years since he was an in-patient of St. Thomas's Hospital, where, after considerable difficulty, a No. 1 catheter was introduced and retained, being replaced by others in succession until No. 6 could be passed, when he left. The contraction speedily recurred, and he was admitted into the Westminster Hospital as above. He is now greatly emaciated, with brown and dry tongue; pulse 110; countenance pallid; appetite defective; abdomen tympanitic; nights restless; and there is a constant escape of fæces during the straining to evacuate the bladder, an effort which he is compelled to repeat every hour

and a-half. The house-surgeon having twice failed in passing a No. 1 catheter, each attempt being followed by syncope and subsequent rigors, I saw the patient, and directed the nightly use of the warm bath, and prescribed salines with opium through the day, and castor-oil every morning. At the expiration of a week his general condition was sufficiently improved to justify the attempt to introduce a No. 1 catheter, when a stricture was detected five inches from the meatus, through which it was impossible to penetrate. Although the examination was conducted with the greatest gentleness, it was followed by a severe attack of "stricture fever," which was only relieved by the administration of opium. The urethra being irritable, and the patient much exhausted, another week was permitted to elapse, during which time quinine and stimuli were administered, but the second trial was likewise futile, and followed by precisely the same results as the first. After the lapse of a third week, and while the patient was fully under the influence of opium, a third attempt was made, but with no better success. I therefore determined to place the patient thoroughly under the influence of chloroform, when, after very considerable difficulty, a No. 1 catheter was introduced, and a pint of highly-offensive and turbid urine withdrawn. In the evening he had retention which, after great difficulty, was relieved by a No. 1 gum catheter; there was considerable hemorrhage, the patient losing from ten to twelve ounces of blood. He passed a restless night, and being unable to make water, the catheter was again had recourse to. (The house-surgeon had unfortunately omitted to retain the former.) As it was found to be impracticable to re-introduce it, the man was again anæsthesiated, and another attempt made, but although the catheter passed its whole length no urine followed, and it was evident a false passage had been made. Opium and the warm bath were had recourse to, and in a few hours a small quantity of bloody urine escaped and continued to dribble away during the day and night.

When I saw the patient on the following morning his tongue was dry and brown; pulse feeble and rapid; skin hot, dry, and blanched; and he complained of great tenderness upon pressure over the lower part of the abdomen; he was immediately ordered calomel and opium every four hours, and a turpentine fomentation over the seat of pain. For the next few days he continued in a very precarious state, but the symptoms gradually yielded, and in ten days from the retention the urine was free from blood; his health was, however so much shattered by the accompaniments of the retention, that a month elapsed before any further instrumental attempt was made. Former experience having shown the impossibility of introducing any instrument excepting while under chloroform, he was once more anæsthesiated, and the dilator with difficulty passed into the bladder; the large sized tube was then quickly passed, the stricture fairly split, and the dilator removed. A No. 12 catheter was next passed, and the urine withdrawn; the bleeding was very trifling. Ordered a mixture of quinine and opium, two grains of the former and ten drops of the latter for a dose. In the evening he felt chilly and uncomfortable, but had no shivering; the urine flowed with moderate ease, and was triflingly tinged with blood.

On the following day he was able to walk about the ward, the urine came away in a small but clear stream, the medicine was omitted, and his ordinary diet resumed.

On the second day after the operation, a No. 12 catheter entered the bladder without the least difficulty, the urethra was tender, but he did not experience so much pain as on former occasions, the after treatment was continued at increasing intervals, and he shortly left the hospital capable of passing his own instrument.

The foregoing is a case of very considerable interest, as embracing many of the most important points in connexion with stricture. The duration of the disease, the manner in which micturition was accomplished, the deplorable state of the patient's health, the supervention of "stricture fever" after every attempt at catheterism, the after establishment of false passage, and the profuse hemorrhage, are all evidences of the serious nature of the case

and consequently presented a severe test to the adopted treatment, and the perusal has, I hope, satisfactorily shown immunity from fatal consequences, rapidity of recovery, and complete restoration of the urethra to its natural size.

Case 2.—*Stricture of Fourteen Years Standing—of a Dense Cartilaginous Character—Situated about the Triangular Ligament—Urine, which was Ammoniacal and Purulent passed Guttatim—Operation—Recovery*—John W., aged 45, a labourer, was admitted in December, suffering from stricture of the urethra, of fourteen years' duration. Seven years since, the urine having been for some weeks previously passed guttatim, he had retention, and applied at St. George's Hospital, where after considerable difficulty a catheter was introduced. He continued an in-patient until No. 4 could be passed, and then left much improved both in health and manner of micturition. The stricture, however, gradually returned, and after several attacks of retention, and a generally increasing difficulty in relieving himself, he became an in-patient at the Westminster Hospital. At the time of his admission, the urine was passed every hour, with great straining, and frequently in drops, there was considerable hardness in the perineum, and his health was much damaged, as evinced by emaciation, pallor, loss of appetite, sleepless nights, and general feverishness, the urine was highly ammoniacal, and loaded with mucus and pus. A No. 1 silver catheter was attempted without success, there was complete obstruction at the triangular ligament, and although the point was firmly grasped, it could not be made to penetrate through the stricture. The patient was kept in bed, and the bowels regulated, and on the third day from his admission, another attempt was made, in the most gentle manner, but with no better result. Considerable irritative fever followed the two trials, and it was only after the expiration of two months, during which time six endeavours were made, that a No. 1. catheter was passed. The stricture was of the cartilaginous variety, about an inch in length, and gave that peculiar grating to the catheter so specially characteristic of cartilaginous obstruction. A large quantity of highly offensive urine was withdrawn, and the catheter was retained. On the following day a larger size was substituted, and on March 4, the patient having been placed under the influence of chloroform, the dilator was with considerable difficulty introduced, and the No. 12 tube immediately passed. A No. 12 catheter was then easily slipped in, and the urine withdrawn. On the following day there was no febrile disturbance, and the patient declared he made water better than he had done for many years. On the succeeding, or second day after the operation, the urine was passed in a smaller stream, and with some scalding, consequently a No. 11 catheter was used, the urine was much less fetid and less in quantity than before. The stream continued small for a fortnight, but the urine was expelled in a much shorter time, and the frequency of micturition had materially diminished, he now only required to relieve himself three times during the night, and the same during the day. The No. 11 having been passed on alternate days, a No. 12 was now taken, and used every third and fourth day in the manner already described. Six weeks after the operation he left the hospital, making water in a perfectly natural manner, having been taught to pass a No. 12 with ease.

The foregoing, one of a class that offers the greatest impediment to the introduction of a catheter, is an excellent example of a formidable cartilaginous obstruction situated at a part of the urethra which, corresponding to its curve, gives greater trouble in the introduction of an instrument than any other. The patient had been the subject of stricture for many years, his urine had for months previous to his admission been passed guttatim, his clothes were constantly saturated, and his health was materially damaged by the hourly necessity for relief—an evil which depended upon the stricture, as proved by the large quantity of urine that was withdrawn when a catheter was introduced. All these difficulties were immediately overcome, and a large-sized catheter could be ever afterwards introduced with perfect facility. The offensive ammoniacal condition of the urine gradually abated, that fluid assumed a normal character, and the bladder being no longer irritated by its

detention, the intervals between the time of micturition were prolonged. In a word, the patient was restored to complete comfort without having been detained in bed more than a day, and without experiencing a single unfavourable symptom, indeed, he did not suffer in the same ratio as during the attempt to simply introduce a catheter. To those who are uninitiated in the treatment of stricture, it might appear that by keeping a catheter in the bladder, and daily increasing the sizes, the same results would have been obtained, but experience proves this plan of treatment to be utterly futile, and that as soon as the catheter is removed, so soon does the stricture return.

Case 3.—*Stricture at the Bulb of Twenty-eight Years' Standing—Frequent Attacks of Retention of Urine—Infiltration—Fistulæ in Perineo and Scrotum—Enlargement of the Prostate Gland.*—H. R., a captain in the Navy, aged 58, consulted me after having for many years been suffering from the effects of stricture. He had had repeated attacks of retention of urine, and upon three occasions matter formed behind and in the scrotum, which eventually gave rise to fistula in those situations. At the time of his coming under my care, the greater portion of the urine escaped through these apertures, so that he was always compelled to make water in a sitting posture, or with the aid of an earthenware slipper held between the legs. His clothes were constantly saturated with these dribblings, and his health was materially damaged by constant straining, and the frequency with which he was compelled to make efforts to relieve the bladder. He had been under the care of many surgeons, both in London and elsewhere, but without obtaining amelioration to his sufferings. Having tried various sizes and kinds of instruments, I was fortunate enough, at the expiration of three weeks, to get a 00 silver catheter through the obstruction. This instrument was retained, and replaced by a larger size, until No. 3 could be passed, when the dilator was introduced, and the No. 12 tube immediately passed. The same after treatment was adopted as in the preceding cases, and in three months from his first visit he was enabled to pass No. 12 without difficulty. The stream was good, the intervals much longer, and his health better than it had been for the previous seven years.

Another very similar case may be added, viz.:—A gentleman, a solicitor by profession, was affected with a stricture of many years' standing. He had for ten years experienced great difficulty in emptying his bladder, and the urine was loaded with mucus. Upon the first examination the catheter was arrested at three inches from the meatus; after some trouble it passed on to a second obstruction, near the membranous portion of the urethra, through which it was impossible at the time to penetrate. Upon subsequent trials, however, I got through the second obstruction, and came upon a third, nearer the bladder, which was eventually overcome. The dilator was at once introduced, and the three strictures split. A No. 12 catheter was immediately pressed in, and the urine withdrawn. Considerable pain was for a time experienced in this instance in expelling the urine, which was only effected with great straining. The cause, however, of this suffering was speedily apparent, for the patient passed three small calculi, which had formed behind each of the obstructions, and were washed away when the urethra became enlarged.

[Mr. Holt, in another number of the 'Medical Times and Gazette,' concludes this interesting series of cases, and ends his paper by the following remarks:]

The foregoing cases have been briefly described, and no allusion or comparison has been made to the various kinds of treatment already published. Their perusal must have satisfied the most sceptical that in every instance where any kind of instrument can be passed into the bladder the urethra may, by the mechanical effect of the dilator, be immediately enlarged to its natural size; and that while in slight cases this enlargement is effected without suffering, or even the inhalation of chloroform, in the more severe forms that agent facilitates the operation and secures a perfect immunity from pain. It is quite true that if the after-treatment is not attended to the stricture will sooner or later recur, but as the patient by passing his own instrument has the control of the bladder, it can only return as a consequence of culpable neglect. The immunity from accidents having been

already proved, the surgeon need have no fear of those serious results which so frequently accompany any cutting operation, and it is no less extraordinary than true that whereas rigors ordinarily supervene after rapid dilatation, they form the exception where the stricture has been fairly split.

The method of performing this operation may be described in a very few words. The permeability of the canal having been once satisfactorily ascertained, the size of the meatus of the urethra is to be gauged by passing into it a sound that will conveniently fit, and the number of the sound so used is to be the number of the tube to be passed; this it is important to ascertain, so that the urethra may not be stretched beyond its natural limits, for while the urethra of one person will admit a No, 14, another will not admit more than No. 9.

The dilator having been previously well oiled, is to be introduced with the handle somewhat over the patient's left hip, and by keeping the convex portion gently pressing against the under part of the urethra, the point will glide along the upper portion until it is fairly beyond the triangular ligament, when, by bringing the handle to a right angle with the body, and gradually depressing it—but not so much as in the passage of an ordinary catheter—it will usually slip into the bladder; in fact, the same proceeding is to be adopted as in introducing a lithotrite for the purpose of crushing a calculus. Having reached the bladder, the dilator should be gently rotated, to prove that it is fairly within that viscus, and being thus assured, the surgeon is now to place the point of the tube he had previously selected upon the wire between the blades, and thrust it quickly onwards to the end. The stricture being now fairly split, the dilator should be rotated to still further separate the tops of the rent, and be then withdrawn. A catheter corresponding to the number of the tube being substituted for the purpose of removing the urine. The catheter is now to be taken out, and the patient sent to bed, with directions to take, every four hours, for the first day and night, a mixture containing in each dose two grains of quinine and ten minims of the tincture of opium. The facility with which this proceeding can be effected will of course depend upon the kind and number of the strictures, and the existence or otherwise of false passages or fistulæ in perineo. The urine being withdrawn, the patient does not require to pass water for some hours, and when compelled to do so, the stream is usually larger, and the urine passes with greater facility than before. On the second day from the operation the same catheter should be gently introduced; but if the patient complains of much scalding, it will be better to take one size less. This should be repeated every other day for a week, when the larger one may be substituted, and the patient be taught to pass his own instrument. Of course the time occupied in the after treatment must vary with the nature of the case, and in the more obstinate forms necessitates the employment of the catheter for some time, the intervals being gradually increased until it is not required to be used more than once in three, four, or six months, and in most instances not more frequently than once a year. The bowels should be relieved by a dose of castor oil taken early on the morning of the operation, and the patient should be directed not to pass water for two or three hours previously, in order, 1st, to facilitate the introduction of the dilator, and 2nd, to permit its free movement in the bladder.

Out of so large a number of cases in which this instrument has been employed, instances have occasionally occurred where the after treatment has been greatly neglected, and the patients have presented themselves again. In these cases it has always been found that the calibre of the urethra may soon be restored by the dilating power of the instrument, without again resorting to splitting, the uniting medium yielding much more easily than the structures which formed the original stricture.

In conclusion, it may be stated that the dilator is not only capable of splitting a stricture, but it affords the most effectual means of dilating it to any extent that may seem advisable to a prudent surgeon. All practical surgeons know the difficulty of passing a second catheter through a difficult stricture. This is entirely obviated by the use of the dilator, which, being once intro-

duced, is capable of either dilating the obstruction by the employment of a succession of tubes, or splitting the stricture by the passage of the largest tube at once.—*Med. Times and Gazette, Oct.* 19 *and* 26, 1861, *pp.* 398, 428.

64.—TREATMENT OF STRICTURE OF THE URETHRA BY SUDDEN AND FORCIBLE DILATATION.

By CHRISTOPHER HEATH, Esq., Surgeon to the West London Hospital, and the St. George's and St. James's Dispensary, &c.

[Mr. Heath confirms Mr. Holt's mode of treating strictures of the urethra, as illustrated in the preceding article, and gives the following interesting cases. He says:]

Whether a stricture can ever be said to be absolutely *cured* must remain an open question; but a method of treatment which so rapidly restores the urethra to its normal size, and allows of the patient's continuing in perfect health for many months afterwards, as in these cases, is certainly worthy of trial.

Cases 1 and 4 were strictures caused by repeated gonorrhœas, and were of the densest character, the latter being complicated by a *fistula in perineo*. Cases 2 and 3 were of a more intractable nature, being of traumatic origin, the tendency to recontraction being one of the great characteristics of the affection. In Case 2, the perineum had been perfectly riddled with fistulæ, which had converted it into an irregular, hardened mass, such as is seldom witnessed, which, however, rapidly disappeared upon the urethra being restored to its normal condition.

It will be noticed that in none of these cases was there any "urethral fever" after the operation, and in one only was there a rigor, which took place on the day *after* the operation, and was consequent upon the passage of an instrument, which might as well have been omitted for a day or two.

Case 1.—Mr. E. P., aged 25, came under my care in January, 1860. Has lived a very fast life, and had gonorrhœa repeatedly. Four years since after a long gleet, he first suffered from retention, which was relieved by the use of a catheter; but he underwent no regular treatment for the stricture, and since that time he has suffered occasionally from retention, and the stream has been decreasing in size. Two years ago a surgeon passed No. 4 with great difficulty to relieve retention; but this gave him such pain that he has never had an instrument passed since. He has been living carefully for the last year, and his general health has been pretty good, although he is thin and pale. He suffers from frequent nocturnal erections and emissions; the stream of urine is very small and twisted, and is passed with great difficulty and straining.

The patient having the greatest dread of pain, on the 14th of January, 1860, chloroform having been administered, I proceeded to examine the urethra. Found a very dense cartilaginous stricture (which could be clearly felt from without), about two inches from the orifice, and through this with very considerable difficulty I introduced No. 00 silver catheter (*i.e.*, two sizes smaller than the ordinary No. 1). Another stricture existed at the bulb, which gave some trouble; but this was at length overcome, and the catheter entered the bladder. Instrument tied in. Ordered half a drachm of tincture of opium.

Jan. 15th. Tried to replace No. 00 by No. 1, but failed (without chloroform), and therefore replaced No. 00. Has had no shivering nor inconvenience, except the presence of the catheter.

16th. Introduced No. 1, and tied in.

17th. Passed No. 2, and tied in.

18th. No. 3 tied in. Lies on the sofa, and suffers little inconvenience.

19th. No. 4.

21st. Chloroform having been again administered, I passed Mr. Holt's dilator (small size) through both strictures into the bladder, and then introduced No. 4 tube. Having withdrawn this instrument, I was then enabled

to pass the full-sized dilator while closed, and into it I at once introduced No. 10 tube, which required some considerable force, owing to the density of the stricture. The dilator having been withdrawn, a No. 10 silver catheter passed readily enough; the bladder was evacuated, and the instrument withdrawn. To take half a drachm of the tincture of opium.

22nd. Passed a very good night; has a little smarting when the urine is voided. Passed No. 10.

23rd. No. 10 passed readily.

25th. Walked to my house, where I introduced No. 10.

Feb. 1st. Has been prevented by business from coming for the last week, and has not used a catheter. I passed No. 10, which was a little tight.

5th. Urine quite clear; feels better than he has done for the last three years; nocturnal emissions much diminished.

13th. Has procured a No. 10 metal bougie, which he is to learn to pass for himself, and made his first attempt to-day. Nocturnal emissions have quite ceased, though he has occasional erections. I did not see the patient again, owing to his dread of having an instrument passed, until the middle of March, when he sent for me, and I found that he had contracted a gonorrhœa, and had orchitis. For this he had been treated by his brother, and the acute symptoms had gone off. I therefore strapped the testicle, and he was soon able to go into the country. Says that he passes a capital stream of urine without any difficulty, but will not let me pass an instrument. Up to the present time he has not required further treatment for the stricture.

For the opportunity of treating the following cases I am indebted to Henry Bullen, Esq., the surgeon to the Lambeth Infirmary. The patients having been more or less under this gentleman's observation up to the present time, it is satisfactory to find that their condition is exceedingly good, and that they have required no further treatment.

Case 2.—Thomas J., aged 57, was admitted into the Infirmary of the Lambeth Workhouse, in Nov. 1859, under the care of Mr. Bullen. His complaint was stricture and perineal abscess, which arose from an accident at the London Gas Works nearly six years ago, when he fell astride an iron pipe. Originally under the care of Mr. Cock, of Guy's Hospital, he was discharged after the successful use of the catheter about twenty times, relieved, but not cured.

At the time of his admission into the Infirmary there were several sinuses opening in the perineum, and one above the pubes, through all of which urine at times passed. The stream which came by the natural passage was exceedingly small and thread-like. With rest and medical treatment these sinuses have all healed, except one in the perineum, through which from time to time urine will find its way. The cicatrix is hard and knotty, and communicates to the fingers a sensation of cartilaginous density.

After some ineffectual attempts to introduce catheters of small size, he was by my advice placed under the influence of chloroform on the 23rd of February, 1860; and having passed a grooved director through the sinus into the bladder, I proceeded to force a No. 5 staff through the stricture and tough cicatrix as far as possible in the course of the urethra until it impinged upon the director, which served to guide it into the bladder. This was accomplished with considerable difficulty, and subsequently a No. 3 silver catheter was substituted for the staff, and was tied in.

During the succeeding night, owing to the patient's restlessness, the catheter slipped out. The dilatation of the stricture, which had been so far effected, proved, however, of the greatest service to him, as he was able to pass in consequence a pretty full stream of urine. Several ineffectual attempts were made to re-introduce an instrument, and therefore on the 17th of March I had him placed under chloroform, when I was enabled to introduce Mr. Holt's dilator with some difficulty. The stricture was successfully split up, a No. 10 catheter readily introduced, and the bladder evacuated, after which the instrument was withdrawn. A full dose of opium with hot brandy-and-water was administered immediately after the operation, under the influence of which he passed a good night.

March 18. A No. 9 catheter was used with comparative ease, it being found impracticable to introduce a larger size without force. In the afternoon he had a rigor, which was treated with the warm bath and hot brandy-and-water.

19th. Patient progressing favourably, and passes a good stream: catheter not passed, owing to patient's being asleep.

21st. No. 10 catheter was passed, and its use was continued occasionally for some weeks. The sinus in the perineum entirely closed, the thickening along the urethra totally disappeared, and the patient was able to resume his work.

Case 3.—John W., aged 61, admitted into the Lambeth Workhouse Infirmary, under the care of Mr. Bullen, on the 8th of December, 1859, suffering from stricture caused by falling off a ladder astride a piece of timber at Exeter, about twenty-four years ago. He was then under the care of Mr. James, of Exeter, three weeks, and was apparently quite well until 1842, when he was admitted into Guy's Hospital with a stricture, and was under the care of Messrs. Cooper, Key, Cock, and Callaway, who were unable to introduce a catheter into the bladder. In 1847 he was admitted into University College Hospital, under the care of Mr. Quain and Mr. Liston, and a No. 5 catheter was introduced by the latter surgeon on one occasion only, every subsequent attempt being unsuccessful. In the early part of 1859 he was admitted into the hospital at Melbourne, Australia, under the care of Mr. Ellis, and was discharged as incurable.

After several ineffectual attempts to pass a small catheter, he was placed under chloroform, and I succeeded in passing No. 1 silver catheter, which was tied in; and on following days Nos. 2 and 3, which were also tied in. Subsequently the instrument was removed for a few days, and the urine passed in a small stream.

On the 17th of March, 1860, the patient was again put under chloroform, and Mr. Holt's dilator having been introduced, the stricture was split up and a No. 12 catheter introduced. This was followed up by the use of an instrument on alternate days for a few weeks, when the man was discharged cured, and remains so, having been under observation up to the present time, being actively employed in the workhouse at his trade as a carpenter.

Case 4.—Henry L., aged 60, admitted into the Infirmary at Lambeth Workhouse, under Mr. Bullen's care, on the 10th of April, 1860, suffering from stricture and perineal abscess, the supposed cause being repeated gonorrhœas twenty-five years ago. He was admitted into St. Thomas's Hospital, and was under the care of Mr. South seven weeks, six or seven years ago. Was admitted into the same hospital a second time in November, 1858, and was under the care of Mr. Macmurdo four months; but a catheter was never introduced into the bladder.

May 3rd, 1860. He was placed under chloroform, and I introduced No. 4 silver catheter, after which Mr. Holt's dilator was employed, and the stricture having been split up, No. 10 catheter was readily introduced. The use of the catheter was continued for some time, when he was discharged cured, with the fistula healed, and up to the present time he has remained quite well. —*Lancet, August* 31, 1861, *p.* 203.

65.—ON INCISION OF STRICTURE OF THE URETHRA.

By Dr. James Arnott.

Notwithstanding the great attention and research which so common and so serious a disease as stricture of the urethra has naturally excited, it must be acknowledged that our means of removing it are, as yet, very far from being perfect. Those which are safe are often inefficient, and those which are efficient are so unsafe as to render their use justifiable only when the patient is exposed to more hazard from the disease than from the remedy. An investigation, however, into the sources of this danger, has shown that it is not an

inevitable concomitant of the essential part of the proceeding referred to; and the purpose of the present communication is to explain how, by employing in its performance an instrument constructed on an entirely new principle, danger may be altogether avoided.

Although a catheter or bougie will, in the majority of strictures, give great relief, this is rarely of long duration; and many cases occur of an unyielding, irritable, or elastic nature, in which these means of dilatation are of little avail. A persistence in their use may, indeed, keep the urinary canal open to a certain degree, but as they do not subdue the irritation caused by the presence of stricture, the long continuance and extension of this irritation too often undermine the general health, and produce organic changes in the bladder, kidney, and other contiguous organs, which embitter and shorten life.

Incision of the stricture by a sheathed knife passed along the urethra, was, in former times, a practice occasionally resorted to. In consequence, however, of the rude and hazardous modes of effecting it, this kind of treatment fell into disuse, and at the time of the publication of my Treatise on Stricture, it was almost forgotten. What was there said and suggested on the subject, again brought it into notice, especially in France, and though the instruments employed for practising internal incision have been hitherto constructed and used on a bad principle, much good, on the whole, has resulted from the revival of this method of treatment.

The imperfections of these methods of internal incision led to Mr. Syme's improved external incision, which was proposed as a substitute for them. But the hopes once entertained that permanent relief from the effects of severe stricture could thus be obtained with safety, have not been fulfilled. As has happened with respect to some other medical questions, the statistics published in favour of the perineal section have not been properly or fairly adduced. Although when employed in cases of little severity, and where dilatation would have sufficed, this operation has not often had an unfortunate termination, it has proved fatal in a large proportion of that severer class of cases in which alone its adoption is warrantable.

In France the perineal section has not been received as a substitute for internal incision; and so highly is the latter proceeding esteemed, that certain French writers speak of the palliative treatment of strictures by dilatation and its radical or curative treatment by this kind of incision, just as they discriminate between the mere tapping of a hydrocele and its effectual treatment by irritating injections. But although relief from successful incision may last longer than that afforded by dilatation, this difference of duration has not been the reason generally assigned for preferring it. It is only where, from the character of the stricture, relief could not be obtained by dilatation, that the majority of surgeons have deemed themselves justified in having recourse to a measure which, though it has proved less hazardous than the perineal section, has by no means been without hazard. Internal incision has often proved injurious or fatal by hemorrhage, infiltration of urine, and pyæmia. We are not indebted to its more zealous eulogists for our knowledge of these facts, but to the communications of impartial inquirers. M. Nelaton, for instance, whose position in Paris enables him to form a correct judgment on the matter, while admitting the necessity of having recourse to this proceeding in certain cases, states, in his recently published 'System of Surgery,' that "in more than half the number" of incisions there is hemorrhage, and often to an alarming extent. As this could not happen without the urethra being deeply or extensively cut, the other dangerous results can be easily accounted for.

If we reflect on the circumstances attending the most improved plan hitherto in use of cutting strictures from the interior, we cannot be surprised at these occurrences. The sharp blade having been protruded from (and at a considerable angle with) its sheath, after it has passed beyond the stricture, "is" (to quote some recently published directions) "firmly pressed on the floor of the urethra, and slowly and steadily drawn outwards about an inch or an inch and a-half." In this operation, the cessation of resistance to the progress of the knife furnishes the surgeon with the only reliable means of

judging whether the stricture has been divided or not; for the elasticity of the urethra, or the ease with which it may be stretched, renders any previous measurement of very little avail. Now, if the knife is not opened exactly at the back of the stricture, and shut exactly in its front, the sound portion of the urethra will probably be more or less cut by it, as it is almost impossible, in the curved portion of the canal, and at so great a depth, to keep the protruded blade precisely in its axis; and so long as the resistance to the extraction of the knife continues, the operator will suppose that the stricture is being cut. When again, in the course of its retraction, the blade has reached the posterior part of a hard stricture, it will drag this to a certain extent forwards before penetrating it, and, in so-doing, will cause a gathering into folds of the portion of the urethra immediately in front of the stricture, through which folds the knife will be apt to start on the completion of its way through the hardened textures, however straight its course may be.

If this exposition of the cause of the hemorrhage, &c., be correct; if the mischief proceed from the sound portion of the canal being cut, as well as the non-vascular, consolidated and contracted part, it becomes a very important question, whether some method cannot be devised of cutting the latter part exclusively. If the division of the contracting tissues could be exactly limited, the objections to internal incision would be removed, and a great desideratum in surgery supplied.

I have ascertained that this can be effected, either by employing a knife rendered so blunt as that, when employed in the usual way, it will only penetrate the part which is much pressed by it; or by employing a sharp knife in such a manner as that it will cut by pressure alone, or without any forward or backward motion. Both may be said to carry out the principle of incision being effected only on the application of great pressure.

In an essay on stricture, printed five years ago, I suggested an instrument of the first description; four years afterwards, Mr. Marshall proposed an urethrotome, closely resembling this instrument; and in March or April of the present year, a paper was read before the Academy of Medicine in Paris, by M. Maisonneuve, suggesting, for the third time, the application of the principle on which those were constructed.

In the 'Medical Times and Gazette' for February 2 last, I described and illustrated by an engraving, another instrument for this purpose, to which, as being very superior to the first, I shall, on the present occasion confine my attention. It consists of a flat tube, about seven inches in length, to one end of which a narrow and deeply-grooved director is attached, adapted for entering the stricture. Three long steel rods or wires, of a size that will pass through the flat tube, and one of which has, for an inch at the end, been sharpened into the form of a knife, constitute the rest of the apparatus. When it is used, the sharpened rod is passed along the tube into the grooved director, from which it is then pressed tranversely against the stricture by pushing one or both of the other rods or wires between it and the whole length of the side of the tube. A very sharp knife thus pressed directly against the stricture, if the breadth of the tube allows it to move through sufficient space, will cut by the pressure alone, or without any retractile motion being superadded; but if the space be too small for this amount of pressure, a blade not so keen is preferable, and then a slight backward or sawing motion is required, in addition to the pressure, for the incision of the stricture.

As an explanation has been given, in the foregoing paragraph, of the accidents which happen from the use of the common urethrotome, there will be no difficulty in comprehending why the new instrument must be perfectly safe. The knife in the act of cutting is parallel, instead of forming an angle, with the axis of the urethra; it moves in a small definite space, which may be precisely that occupied by the previously measured stricture; no dragging of the stricture is caused by its motion, or gathering of the urethra into folds, because both its direct or tranverse pressure on the stricture, and the end of the flat tube, keep it in its usual position, while the incision is being made; this motion is so limited, that even were it to displace the stricture,

it would hardly be beyond the tenth of an inch; and, lastly, when the knife is pressed out of the groove, no injury can be received by the sound or uncontracted part of the urethra, because, by giving no resistance, it is only pushed aside by its edge. In fact, the incision is as exactly limited to the stricture as it would be if the urethra were laid open and exposed to the surgeon's view. Unless the incision required should extend beyond its normal dimension, it is impossible to injure the sound portion of the canal, but the cases must be very few, if any, in which so extensive an incision is advisable. The sound portion may come in contact with the edge of the knife during incisions of strictures within this limit, but as there is no pressure upon it, there is no cutting.

The first case in which this urethrotome was used had been previously adverted to in page 18 of the Essay above mentioned. It is there stated that the patient had laboured under stricture for many years, and that frequent dilatation was required to preserve a tolerably free channel for the urine, and thus prevent irritation of the bladder. This dilatation could never be carried beyond a very limited extent, and as there was constant impending danger from persisting irritation, the application of some auxiliary means had been frequently and strongly recommended. Relief, however, being always obtained for a time by the dilatation of fluid pressure, and as I could not *then* assure the patient that incision was without hazard, he preferred the safer, though less effectual measure. At last an urinary abscess formed in the perineum, and the urine spread from it to the scrotum. When I saw the patient on his arrival from the country, he was prostrated by his sufferings. It was, notwithstanding, necessary to get rid of the urine and internal sloughs by extensive incisions; but as these were made under congelation in lieu of chloroform, the existing debility was not thereby sensibly increased. When the system had recovered itself from this perilous accident, the stricture was cut by the new urethrotome on two several occasions, with a few day's interval; and I was able afterwards to pass a flexible metallic bougie nearly twice as large as any that had been formerly used. Owing to the hard and callous nature of the stricture, the patient scarcely felt the cutting and the loss of blood did not, on either occasion, exceed a few drops. When I examined the urethra, nine months after the incision, there was no recontraction, nor had there been any return of distressing irritation. The patient continued to pass, from time to time, the same sized instrument which I introduced after the last incision.

I will now add some remarks on the construction and mode of applying the instrument, which a considerable experience in its use has suggested.

Although I have not had occasion to use so small an instrument, a steel director, appropriate to a very narrow stricture, could, no doubt, be made as small as the grooved staff commonly used in the perineal section. The knife fitting its groove must necessarily be very small, but if the knife be short, and if the supporting rod first passed under it be narrow, it will be sufficiently strong. There are few strictures, however, which cannot be previously dilated to the extent necessary to admit a larger silver director.

The flat tube to which the director is attached may have various breadths according to the degree of pressure desired. That which I have generally used is one-third of an inch broad. It must be remembered, however, that generally speaking it is only so much of the circumference of a stricture as is much pressed upon that will be divided by the knife; beyond this extent the pressure will be insufficient, and the stricture will yield to the knife instead of being cut by it. A knife separating from the director to the extent of two or three-tenths of an inch, even admitting that the channel of a narrow stricture is completely filled by the director, will not, perhaps, penetrate to more than half this depth. It may be proper, therefore, to make several incisions in different directions. If the same instrument be employed for these, the bulk of its director must be increased in proportion as the canal is opened, by a coating of sealing-wax applied after the metal has been thoroughly cleaned and heated.

In many strictures, however, one incision will be sufficient; they will

yield afterwards to dilatation. One would expect this to be especially the case in those consisting of an elastic band.

It is questionable whether, as respects permanence of relief, there is advantage from extending the incision through or beyond the whole substance of a stricture. In the perineal section the incision extends through the whole of the subjacent parts, and must necessarily have always one direction, whatever may be the situation of the opening through the stricture. The channel for the urine preserved afterwards by the catheter, must be formed as much by the soft and yielding adjoining parts as by the urethra itself; and the hardened tissues originally forming the stricture will bear, therefore, but a small part of the pressure of dilating instruments. It arises probably from the increase of the morbid tissues being thus unrestrained that strictures have sometimes appeared to be eventually more untractable after the perineal section than they were before.

When the tissues forming the stricture have, by length of its duration or other causes, become of a cartilaginous hardness, repeated strokes of the knife may be required for its division. This can be easily effected by the new instrument, and by it alone.

Although tissues when so consolidated can hardly be infiltrated by urine (and it is in such cases that incision is especially appropriate) I have, nevertheless, as a precaution against the possibility of such an occurrence, generally recommended that a catheter should be occasionally used during the two succeeding days. A large bougie, a fluid dilator, or the dilator constituted by a modification of the new instrument itself, should be daily passed for some time afterwards, to prevent the adhesion of the cut surfaces.

The ordinary mode of using this apparatus is as follows :—It is satisfactory, though not absolutely necessary, to have made a previous examination of the stricture, and to have ascertained its length and the situation of its channel as respects the axis of the urethra; its distance from the orifice should be marked on the flat tube. Then, having warmed and oiled the instrument and inserted the blunt rods in the tube in such a manner that one of them, having its end of a wedge-form, may, when protruding from the tube, afford a sloping surface, it is introduced into the urethra. As soon as the grooved director has been lodged in the stricture (which may be ascertained, amongst other ways, by noticing when one or both rods are forced backwards), the outer one is withdrawn, and again pushed into the tube on the other side of the remaining rod so as to press this outwards. If there is a difficulty in doing this; if the patient feels the rods distending the stricture; or if the instrument be firmly fixed in the urethra after the second rod has been inserted, there will be little doubt that the director has been properly placed, and that it is of a proper bulk. If none of these circumstances are present, it must be inferred either that the director is not within the stricture, or that it fills its channel so imperfectly, as to render it probable that the knife when introduced will not be pressed with sufficient force against the stricture to cut it. When the surgeon is satisfied that the instrument is properly placed, both of the blunt rods are to be withdrawn, and the sharpened one is to be passed along the groove as far as is deemed necessary. Then, while the instrument is held so that its axis may correspond with that of the urethra, the knife is pressed or driven against the stricture, by passing one or more blunt rods or wires between it and the side of the tube. If this pressure be alone insufficient to effect incision, it is necessary to give the knife a slight retractile or outward motion, after which it is pushed again into the groove and extracted. The case with which the knife has moved, its being soiled with blood, and the facility with which the blunt rods can again be introduced, are all evidences that the stricture has been cut. If these be wanting, the presumption will be that the stricture is peculiarly hard or very elastic, and that it will be necessary, in order to divide it, either that the knife should be repeatedly drawn over it, or be applied with greater pressure by means of a broader tube.

I trust it will not be supposed, from my having thought it advisable to give such minute directions, that this proceeding is one of difficult execution.

But, as the most simple surgical operation may be so imperfectly performed as to prevent the attainment of its object, I am anxious to prevent such a result by mentioning everything of importance relating to an instrument which has little in common with other urethrotomes. Besides, several of the above observations regarding incision by this apparatus have an equal bearing on the employment of those modifications of it which furnish us with an excellent dilator of ex-centric action, and a certain mode of applying caustic to the front or the channel of a stricture. For it must not be forgotten that, however valuable a perfectly safe mode of cutting stricture may be in those cases which resist dilatation, it ought not to be had recourse to until this has been properly tried and found inadequate; and it ought also to be borne in mind that, when incision has been employed, it is necessary to maintain the benefit resulting from it by the occasional use, for some time at least, of the most efficient means of dilatation.—*Med. Times and Gazette, Sept.* 14, 1861, *p.* 264.

66.—ON PHYMOSIS.

By P. C. Price, Esq., Surgeon to the Great Northern Hospital, &c.

[The author performs the operation for phymosis somewhat differently from most surgeons].

In young children he usually advised the exhibition of chloroform, as much pain was not only thereby avoided, but it was exceedingly difficult, otherwise, neatly to finish the operation, owing to the contortions into which the little patient generally twisted his body. The prepuce being put on the stretch, so as to remove the orifice as far from the glans penis as possible, the blunt-pointed blade of a pair of scissors is passed through the preputial orifice to the distance of about a quarter or half an inch towards the corona glandis, and the prepuce divided through its upper surface. This will allow the glans to be seen, covered, in all probability, especially if the phymosis be congenital, by the mucous membrane of the foreskin, which will be found more or less tightly adherent to the glans. Laying aside the scissors, this adhesion is to be separated by somewhat forcibly retracting the back portion of the prepuce, and by means of the thumb-nail breaking through such points of union, as may be more than usually firm. Around the fossa in front of the corona glandis will generally be found small patches, or masses, of hardened and otherwise altered secretion of the odoriferous glands. These being removed, the scissors must be again employed, and such portions of the redundant skin and mucous membrane cut away as will allow of the glans penis, after the edges of the wound are united, remaining uncovered. In removing the necessary portions of integument, the author generally contrives by shaping the incision somewhat obliquely from above downwards, to avoid wounding the artery of the frænum, whereby there is little risk of secondary hemorrhage and infiltration of the loose tissue with blood—a result which he has frequently known prove troublesome, and for a considerable time retard the healing process. If, however, it be desirable to loosen the connexion of the mucous membrane with the glans at that part which is called the frænum, instead of dividing the little artery which runs in this direction, it is better to separate with the scissors, the union of the frænum with the glans immediately in the line of junction, by which means the course of the vessel will be avoided. Mr. Price, although some surgeons do not follow the practice, is in the habit of keeping the mucous and cutaneous surfaces in contact by sutures, unless the patient on whom the operation is performed be very young. Sutures of the very finest silk are to be preferred to those in ordinary use, and made either of the same material or of metal, as they cause less irritation, and do not need removal before their destruction has been effected, in the course of a few days, by reason of the contact of the healing or other discharge. To prevent the urine, for the first few hours after the operation, from excoriating the edges of the wound, it is advisable to smear them with oil or cerate. In performing such an operation, less danger will

be run of wounding the glans penis—a result which the author has known to occur when the bistoury has been used. It must be recollected, that to obtain complete relief from a congenital phymosis, a mere slitting up of the elongated prepuce will not generally suffice; but a certain portion of the mucous membrane and cutaneous tissues must be removed.—*Lancet, May* 4, 1861, *p.* 436.

67.—LITHOTOMY—LATERAL OPERATION.

Case under Prof. FERGUSSON, at King's College Hospital.

The patient a boy about five years of age, was placed under the influence of chloroform. Mr. Fergusson, after having performed this operation in his usual manner by lateral section, observed that the stone had no phosphates about it—was of slow growth, about six months'. Mr. Fergusson on this occasion again insisted on the necessity in young subjects of cutting well into the membranous part of the urethra. If this precaution was not observed, he said, the prostate was liable to be pushed on end of finger into the bladder, thus inverting the order of things. Thus, not only the anterior, but posterior walls of urethra might be divided, and its continuity destroyed. Mr. Fergusson said, operators often when they have reached the groove of staff in membraneous part of urethra, as urine escapes, think they have reached the bladder. "No such thing: the membranous part of urethra, somewhat like a syphon, is generally, if not always, filled with urine—the groove ofstaff acting as a conductor of the urine. If you do not cut fully onwards into the prostate, so that you can easily pass your finger into bladder, a complication will assuredly happen. I consider the lateral operation as good as any other: other modes are introduced—I shall abide by the lateral cut. For simplicity of procedure, safety in execution, freedom of action, and ample space of opening—these circumstances all recommend the lateral operation to me before all the other modes, especially in children. I use scoop in children in preference to forceps; but small forceps may be used on some occasions. We hear of quick recoveries; we are told of patients walking about in the wards on the day following operation, or even in a few hours after. I caution you about these quick recoveries. *Above all things avoid quick recovery after lithotomy.* Give your patient rest, quiet, and *decubitation*, for a week, fortnight, three weeks, or even a month, rather than attempt quick recovery."—*Medical Circular, July* 24, 1861, *p.* 64.

68.—RADICAL CURE OF VARICOCELE.

Case under the care of HENRY LEE, Esq., King's College Hospital.

A patient, labouring under a very aggravated form of varicocele, was brought into the theatre on the 9th of Feb. last. It appeared that he had suffered for a considerable time from pain in the part, and in the loins. The testicle upon the affected side was evidently wasted; but the most remarkable feature in the case was the great distance that it hung below the other on the opposite side. The testicle on this (the left) side was so pendulous and loose, that when the patient lay on the operating table it hung over towards the outside of his left thigh. Mr. Henry Lee proceeded to operate in a way somewhat different from that in which the operation has been usually conducted. Having introduced a thin needle between the vas deferens and the enlarged veins, a figure-of-8 ligature was passed over the ends of the needle, and the bunch of veins was thus compressed. The same proceeding was repeated at the lower part of the scrotum. In each case a piece of thin leather was placed under the thread to prevent any pain that the pressure on the skin might occasion. A considerable extent of the skin of the scrotum and of the enlarged veins beneath it were thus included between the two needles, and the circulation in these parts in a great measure obstructed.

A thin knife was then introduced about midway between the two needles, and between the vas deferens and the enlarged veins. The knife was directed upwards, so as to divide the integument near the upper needle. A second incision was made to pass downward, so as to divide the skin near the lower needle. A wedge-shaped portion of tissue—skin, enlarged veins, and all—was thus removed, and a very considerable interval was left. Some extremities of divided veins, and some cellular tissue, were removed with scissors, and a clean surface left. It was remarked that although such an extensive surface was exposed in so vascular a part, yet no hemorrhage followed; this, in fact, was completely controlled by the acupressure of the needles. The needles were now drawn together and maintained in position, and thus approximated the edges of the wound, which was then dressed by a piece of dry lint, and the patient sent to bed.

Mr. Henry Lee remarked that cases of pendulous testicle, as they present themselves in practice, may be divided into three classes; 1, those in which the skin and subjacent tissues are relaxed; 2, those in which the veins alone are enlarged; and 3, those in which both skin and veins are relaxed and enlarged. The first may be remedied by the removal of a portion of the integuments only; the process of cicatrization braces up the surrounding parts, and the evil is thus remedied. In the second class of cases the obliteration of the veins is requisite, and this may be done by subcutaneous section, without any wound in the skin beyond that which is required to admit the thinnest knife. But in the third class, where the skin is much relaxed, the simple obliteration of the veins will not remedy the pendulous condition of the testes. In such instances Mr. Lee recommended an operation similar to that which he had now performed—namely, the removal of a portion of the skin, together with the subjacent enlarged veins. The acupressure of the needles he considered sufficient to ensure the patient against hemorrhage on the one hand, and any absorption through the veins on the other. The operation, therefore, although apparently a formidable one, was, he believed, free from danger.

The patient in the above case, we understand, left the hospital at the end of three weeks, and continued to present himself occasionally as an out-patient. The wound cicatrized very firmly, the enlarged veins were completely closed, and the testicle retained very much in its natural position; the testis also, which had been wasted before the operation, regained its normal size.—*Lancet, June* 22, 1861, *p.* 610.

69.—*On the Radical Cure of Varicocele.* By J. Redfern Davies, Esq., Surgeon to the Birmingham Workhouse.—[The following short case is one of some interest:]

The patient, aged seventeen years, was admitted into the infirmary on the 10th of December, 1858. He was the subject of varicocele on the left side, which had existed for the last three years, and to so great an extent as to prevent him from following his employment. Being desirous of radically curing him, I adopted the method of M. Ricord, which I had seen him perform with success; but instead of the hempen ligature which he used, I tried to do the same thing with silver wire—namely, between the vas deferens and the veins I passed, by means of a needle, a double silver wire, and by the same apertures in the opposite direction, behind all the veins, another wire. By engaging one free end of the wire through the loop of the other one, and doing the same on both sides, the veins were, by traction on the wire, compressed. This traction was kept up constantly by means of an elastic watch-spring, bent in the shape of a horse-shoe.

In ten days' time, thinking the varicocele cured, and wishing to remove the wires, I tried to do so, and found I could not, though it was easy enough with hempen ligatures. I therefore cut off the wires as short as I could, trusting to the innocuity of silver wires to cause no harm. In another week the punctures were well healed, and he was discharged, the varicocele then appearing perfectly cured.

Seeing him again in about a year's time, upon examination I found that all things remained as when he left; and he stated he had resumed his work immediately upon leaving. He himself was totally unaware of there being any wires in his scrotum.—*Lancet, July* 20, 1861, *p.* 60.

70.—NEW REMEDY FOR IRRITABLE CONDITIONS OF THE BLADDER.

By Henry Thompson, Esq., Assistant-surgeon to University College Hospital.

[Mr. Thompson has been trying the root of a grass called the *Triticum repens*, in irritable states of the bladder, with success. He says:]

My first acquaintance with it was derived about three or four years ago from a gentleman in the country, who was the subject of severe and long-standing stricture of the urethra, and who had long used it to relieve the frequent and painful micturition of which he was the subject. In this case no drug in the Pharmacopœia afforded so much relief as the remedy in question; but this circumstance I was at first inclined to attribute rather to imagination, or to a peculiar idiosyncrasy, than to the virtues of the plant. However, on his strong and repeated representations respecting its value, not only to himself but to some others whom he had supplied with it, I was induced, not until about a year ago, to prescribe it in a few cases, and subsequently gave it a systematic trial on a large scale, both in hospital and in private practice.

The form I have adopted has been uniformly the same. One ounce of the dried and cut stem is infused in a pint of boiling water for an hour. The liquor removed by straining has been given, unmixed with any other remedy, in quantities varying from twelve ounces to a pint during the 24 hours, in several doses. The taste of the infusion is rather agreeable than otherwise; it produces no nausea or derangement of the stomach.

From notes of the numerous cases in which I have employed it, making at the same time due allowance for the effect of other sources of benefit to the patient, I have arrived at the following conclusions respecting the indications for its use:—

In vesical irritability produced by inflammation of the prostate and neck of the bladder; in severe gonorrhœa, and especially when the inflammation extends backwards; in the pain and spasm caused by calculus, and by aggravated stricture of the urethra, as well as in some cases of obscure disease of the bladder, the good effects of the infusion have been very marked, and it has proved far more efficacious than the buchu, which may fairly be esteemed the most widely applicable and generally useful of our officinal remedies of this class in such circumstances. In cases of prostatic enlargement in elderly patients it has been of service, but less frequently than in the conditions above-named. It has also afforded great relief in renal calculus. A medical man practising in London, who has thus suffered, and very severely during many years, tells me that "after trying every approved remedy, it is the only thing that has rendered life endurable." This is one of four similar cases in which it has been more or less useful.

In short, wherever micturition is very frequent or painful, depending on hyper-sensibility of any part of the urinary passages from acute or subacute inflammation, with signs of its presence in the urine itself, the symptoms are mostly materially relieved, and the urine becomes clearer. If improvement is produced at all, it is generally very soon after commencing the medicine, and if none can be observed in four or five days, it is not worth while continuing to employ it.

I believe it is important that the plant should be gathered in the spring, shortly before the leaves appear; the stem is then to be slowly dried without artificial heat, and cut into short lengths for use. The infusion obtained from material so treated is superior to that made from plants gathered indiscriminately at any time, and also to the infusion made from the Triticum

repens which is imported by the herbalists for the purposes of French pharmacy in this country.—*Lancet, Oct.* 12, 1861, *p.* 345.

DISEASES OF THE SKIN.

71.—ON TUBERCULAR DISEASE OF THE EXTERNAL LYMPHATIC GLANDS.

By C. P. Price, Esq., Surgeon to the Great Northern Hospital, &c.

It not unfrequently happens, when ulceration follows suppuration of tubercular glands, and heals without the direct attention of the surgeon, that the implicated integument remains disfigured by induration, unseemly scars, and uneven cicatrices. Under such circumstances, the use of certain measures, to be presently described, will oftentimes lead to a favourable solution of the existing disfigurement.

Lotions. When induration of the integument and adjacent cellular tissue results from the healing of tubercular ulcerations, it may frequently be removed by absorption, especially when it is of recent origin, and correct therapeutical means are employed. Without doubt, solutions of various salts of iodine will be found among the most valuable adjuncts which the sugeon has at his command. Mr. Erichsen, in his 'Science and Art of Surgery' (3rd edit., p. 465,) strongly recommends lotions of iodide of potassium and carbonate of potass: a drachm of each of the salts, with an ounce of spirits of wine and eleven ounces of water, makes, he says, an excellent application, which should be kept constantly applied to the implicated parts by means of linen or lint covered with oil-silk.

I am in the habit of substituting iodide of ammonium for iodide of potassium, as I have reason to believe that it will be found more beneficial. Sir Astley Cooper placed great faith in a solution of chloride of ammonium; and other surgeons of large experience have written in favour of the use of simple alkaline solutions. Whatever liquid applications are employed, it is all important that sufficient time should be given for a trial of their efficacy; for it oftentimes happens that a satisfactory result is only obtained after their most persistent use.

Unguents. When the application of lotions has failed to produce the desired effect, it will be advisable to have recourse to some more definite measures to procure absorption and removal of the indurated condition. I have experienced considerable advantage from simple frictions, especially when no abnormal tenderness has been exhibited by the implicated parts. But still greater good will oftentimes be derived from the additional use of certain ointments and oils.

When the aplastic deposits are of long standing, the use of an ointment composed of iodide of ammonium, such as already described, will often lead to gradual softening and absorption of the hardened surface to which it is applied. The great advantage of this application is, that it can be used with freedom without imparting to the skin the peculiar colour which the local employment of pure iodine produces, and whilst equally effective, or nearly so, is, in many ways, less objectionable to the patient.

When it is deemed necessary to make use of mercurial preparations, the *unguentum hydrargyri* may be selected; or, under certain circumstances, iodine and mercury may be combined in one or other of the forms previously considered. I have elsewhere spoken favourably of the use of oils, when assiduously rubbed into the integument covering an indurated gland; and I can also recommend their employment in all false hypertrophies of the skin and cellular tissue. When containing iodine, mercury, or certain stimulating substances, such as ammonia, camphor, chloroform, &c., they will be found doubly advantageous. Great care and discrimination are, however, always needed before advising friction either with ointments or oils. I have known too vigorous and oft repeated applications of these substances

fraught with the greatest mischief; while it not uncommonly occurs that even a limited use of them produces an undue amount of irritation, which leads to ulceration and suppuration, and, in fact, to a reopening of the original sore. When such is the case, I have found occasional paintings with glycerine, in which is dissolved iodide of potassium or of ammonium, of great benefit.

Caustics. As already seen, an unseemly scar is one of the most frequent results which follow an indifferently treated attack of suppurative destruction of the external lymphatic glands. When a disfigurement of this nature causes not only personal but physical annoyance, the opinion of the surgeon will oftentimes be solicited as to the best method of obtaining its removal. Scrofulous scars (so called) generally present certain characteristics. They vary in colour from a nominal to a bluish-purple tint, have a raised and unhealthy appearance, and are sparingly covered with cuticle. Under certain conditions, they prove irritable and painful, and are always more or less liable, especially when the system is impoverished, to assume diseased changes by reason of their low standard of organisation.

When a scar resulting from the healing of a tuberculous ulcer, or one in connexion with a gland which has been involved with tubercle, is of such a kind as to give rise to disfigurement, and becomes the seat of irritation and pain, its destruction may be deemed advisable, so that nature, immediately assisted by art, may more efficiently repair such structures as have through disease lost their original integrity. For this purpose, the use of various caustics has from time to time been recommended. Among those most generally selected, the following may be mentioned:—Chloride of zinc, potassa fusa, nitrate of silver, and the strong mineral acids. I am most disposed towards the employment of potassa fusa, and seldom select any other caustic, as I have found it, when judiciously employed, in every way the most satisfactory. One or two applications usually suffice; and, if care be taken, the new tissue which results will bear a very close resemblance to healthy skin, although to the accustomed eye it will always, or generally so, be found to differ. It usually possesses a more shining and glistening hue, and presents a stretched appearance. Wandering over its surface will be detected irregular, tortuous, red, linear vessels; and here and there, especially if the tendency to scrofulous action be prominently marked, a proneness to ulceration. Although the patient may be well pleased to recover from a severe attack of glandular tuberculosis with no further disfigurement than one or more patches of new tissue, which if not absolutely similar, bear a close resemblance to the normal cutaneous surface, still the surgeon is bound to use every endeavour to forward the most advantageous form of cicatrisation and reparation.

It is only of late years that a correct acquaintance with the nature and nutrition of scars and cicatrices has sufficiently impressed practitioners with the importance of regulating and avoiding their unnecessary formation. There is no doubt whatever that the growth of cicatrices is accomplished by assimilative processes identical with those which lead to the development and increase of healthy tissues. Consequently, it is expedient that all available means should be used to keep them as limited as possible; for although, in the early years of childhood, a scar resulting from tuberculous destruction of a lymphatic gland may be nearly imperceptible, still, at a more advanced period of life, it will be found to have relatively increased with the other tissues of the body; or, to use the words of Mr. Paget, "the scar of the child, when once completely formed, commonly grows as the body does, at the same rate and according to the same general rule; so that a scar which the child might have said was as long as his own forefinger' will still be as long as his forefinger when he grows to be a man." ('Op. cit.,' vol. i. p. 49.)

Bearing these facts in mind, I make it a point never to resort to the use of caustics for the destruction of scars and cicatrices, unless I can be tolerably certain that improvement of an appreciable kind will result. In general, the pure caustic potassa, in stick, although immediate and sometimes severe in its effects, will be found the most advantageous form; for, with care, the

diseased tissues can with nicety be destroyed without in any way endangering those of a more healthy nature. When the cicatrices are numerous, and situated, as they usually are, on the neck, it is generally more convenient to destroy them by degrees; for, in this way, the surgeon will be better able to accomplish what always proves to be a delicate, and indeed, sometimes, no very satisfactory task. After the formation of an eschar, soothing applications will be found most grateful. In the course of a day or two, the destroyed portions will separate, and leave a healthy surface. If this surface be on a level with the surrounding skin, it will be easy to induce cicatrisation and a permanent improved condition. Should one application of the caustic prove insufficient, it may be repeated, but not without giving some slight pain. The practitioner will, however, probably encounter but little difficulty in the judicious destruction of such scars and cicatrices, which cause an amount of disfigurement unpleasant to the sufferer, and by no means creditable to surgery, provided due attention be paid to certain requirements which have been noticed in these pages.

The Knife. When the cicatrices which result from the healing of suppurative lymphatic glands are very extensive, and cause serious disfigurement, they may oftentimes be removed, or considerably lessened, by a judicious and skilful recourse to the knife. The peculiar puckered condition which these cicatrices assume has already been alluded to; but it remains to be noticed how position and various circumstances influence their formation. A cicatrix following destruction of one or more of the superficial cervical glands situated in the middle of the neck, assumes a permanent puckered and dimpled appearance, by reason of the skin being glued to the deeper structures; for it will frequently be found impossible to elevate the integument so implicated from the parts placed immediately beneath. When lymphatic glands situated near to bone are destroyed, and where there is a paucity of soft covering materials, the cicatrices resulting will be likewise found adherent, not only to the cellular sub-structure, but to the bone itself. It is most usual to observe this condition at the base and angle of the lower jaw, beneath the eye, on the forehead, and over the clavicle.

When cicatrices of this character disfigure these various parts, they may be treated by the knife in one or other of the following ways.

If the cicatrix be healthy, and its unsightliness be mainly dependent on its puckered and dimpled appearance which results from its adhesion with deeper tissues, it will oftentimes suffice to sever the bond of adhesion, and prevent, during the healing of the wound which has been made, a re-union of the divided parts.

An operation of the nature is slight, free from any great amount of pain, and oftentimes productive of much benefit.

The details of the following case illustrate how such a proceeding may be accomplished. A girl, aged 15, who had suffered for many years from various forms of scrofulous disease, experienced considerable inconvenience from an unsightly cicatrix, situated on the upper part of the right cheek, over the junction of the malar with the superior maxillary bone. The principal deformity arose from its firm adhesion to the bone. The tissue forming the cicatrix was very thin, and there appeared to be little, if any, interposed structure between it and the bone, while the patient had a fat chubby face, so that the deep depression which thus resulted was rendered the more disfiguring. A thin-bladed narrow knife (a tenotomy knife) was inserted at the distance of about a quarter of an inch from the outer margin of the cicatrix, and passed obliquely down to the point of adhesion, which was then freely divided, care being taken to keep the point of the knife close to the bone. When division had been satisfactorily accomplished, the opposing raw surfaces were separated by drawing the cicatrix away from its old position, and retaining it in its new one by means of sticking plaster. In this way, the two surfaces were allowed to heal without the possibility of their again becoming united. In four days' time, the cicatrix was allowed to slip back into its former locality, when it was found that considerable improvement had resulted, although it was impossible, as it is in all similar cases, for the deformity to be

quite removed, on account of the loss of the cellular tissue, fat, &c., which had taken place.

I have resorted to a similar operation when the cicatrix has been situated beneath the margin of the lower jaw, and with even better results. The proceeding is, however, still more satisfactory when the band of adhesion is connected with soft parts only, as where the cicatrix is situated in the neck beside the sterno-mastoid muscle. When the cicatrices are more complicated, it is often expedient to resort to a somewhat more extensive use of the knife, and entirely remove the disfigured integuments. This may often be accomplished to the credit of the surgeon and satisfaction of the patient.

The operation consists in entirely removing the cicatrix, and uniting the lips of the wound in such a way that a mere linear scar results. The advantage of such an operation is at once evident, especially if the deformity to be removed be the only one situated in a part of the body usually exposed to observation.

I may quote the following case as illustrating the advantages to be derived from such a method of treatment. A young lady consulted me, in the autumn of 1859, on account of an ugly scar, about two and a half inches in length, situated on the right shoulder, in such a position that she was unable to wear a low dress. Under the influence of chloroform, it was removed, great care being taken perfectly to adjust the margins of healthy integument. so that a mere linear seam should result. This was accomplished by means of delicate wire suture; admirable union ensued, and, in a week, she left London, well satisfied with the result obtained.

I could quote many instances in which a judicious application of the knife was followed with decided advantages, but the above case is sufficient to prove the fact.

For the adjustment of the lips of the wound, I am in the habit of using a fine metallic pin, with the figure-of-8 suture, such as is employed by continental surgeons, in preference to the sewing needle armed with either silk or wire.

It occasionally happens that the cicatrix resulting from suppuration of lymphatic glands is so extensive that neither of the above operations will prove of any service. It then becomes advisable to remove the cicatrix and transplant a portion of adjacent healthy skin—in fact, a true Tagliacotian operation is needed. It is fortunate, however, that such a severe measure is seldom required; although I have resorted to it in instances in which the lower eyelid has been extensively disfigured, and serious conjunctivitis, and other affections of the eye, have resulted from inability to close the eyelids.

When mere bridles of integument cause additional disfigurement to a cicatrix resulting from destruction of a gland, they may be removed by means of the scissors or knife, without causing any special pain or annoyance. Situated on the anterior surface of the arm are one or more lymphatic glands, which sometimes become greatly enlarged and suppurate. The destructive action not unfrequently involves the flexor muscles and tendons, and the consequence is, that a serious amount of contraction of the fingers ensues.

I have very lately seen, in the practice of my friend Mr. Fergusson, a case illustrating these various features. The patient, a young lady, had suffered from scrofulous suppuration of one or more of the lymphatic glands in front of the arm. Two or three of the fingers were considerably flexed, and could not be extended, by reason of the contraction which had ensued in the muscle supplying the tendon—superficial flexor of the fingers. Division of the contracted tendons was resorted to, and the patient regained an excellent use of the fingers.

Such is a brief notice of the advantages which may often be afforded by the surgeon in cases of disfigurement and deformity arising from tuberculous and simple destruction of the lymphatic glands; but while I have endeavoured to point them out more forcibly than, perhaps, other authors on scrofulous diseases have done, yet I cannot too urgently insist on the importance of due attention being paid, both by the patient and the practitioner, to the various destructive and reparative processes which ensue when the external

lymphatic system is invaded with tubercle; believing that, in the treatment of this disease and its results, prevention is, on all occasions, better than cure.—*British Medical Journal*, *April* 6, 1861, *p.* 358.

72.—ON THE TREATMENT, WITHOUT EXCISION, OF WENS AND OF SOME OTHER CYSTS.

By M. A. Courty, Professor in the University of Montpellier.

There is no application of our art more frequent, and in some measure more routine, than the treatment of those tumours, often so numerous, and of dimensions so varied, which are developed on the head, in the substance of the scalp, or below it, in the neck, on the face, and some other parts of the body, consisting of a cyst and its contents, whatever may be the variable thickness and density of the envelope, the solidity or fluidity of the encysted matter.

Removal is a prompt and easy method of dealing with them; but in addition to the fact that it is not always exempt from accident, it is not readily consented to by patients. Not to mention the numerous cases in which the multiplicity of the wens makes the patients dread the renewal of the pain they have once unwillingly consented to undergo, these tumours being naturally painless, often little troublesome, not interfering with any functions, not threatening life, nor causing imminent danger nor real infirmity, it is natural that they should be borne. until their great development, inducing the inconvenience of a disagreeable deformity, or the danger of approaching degeneration, overcomes the timidity of the patients, and hastens their determination to submit to operation. It still remains for us to combat the prejudice entertained, that in touching a wen we may run great danger, not only of such local accidents, as erysipelas, inflammation, or epithelial development, which are real; but still further by the possibility of supposed metastases, or of some disturbance which their removal may excite throughout the system.

Setting aside, then, removal, which must sometimes be practised, it may be said that the destruction of the cyst is the key to every method, the application of which is capable of producing a permanent result. It remains to determine the best mode of destroying the cyst, and the cases to which the method is applicable.

I shall begin by observing that the destruction of the cyst does not necessarily involve that of the skin. It is well to make this remark, because the majority of surgeons who have applied agents of destruction, especially of cauterization, in the treatment of wens have applied them on the skin; differing not much in that from quacks. who, by means of so-called dissolving plasters, or of pretended chemical pencils, determine on the skin, throughout a variable extent, the painful formation of eschars more or less extensive, which may become, as well as incision, the source of inflammatory symptoms. Whether, with M. Legrand, we employ linear cauterization with caustic potash, or with Bonnet, superficial cauterization with Vienna paste, or pâte de Canquoin, or, with M. Maisonneuve, *cautérisation en flèche*, &c., the skin is always attacked. I have had occasion to see this cauterization followed by erysipelas; and even when the bottom of the cyst was thus exposed, without being destroyed like the surface, I have witnessed an epithelial vegetating production developed at the bottom, and soon assuming the aspect of an epithelioma, or of a cancroid, little encouraging to the operator, and necessitating a fresh and extensive application of the caustic.

I, therefore, lay it down as a principle that the skin ought to be, so far as possible, preserved, and that the destructive agent ought to be carried into the very cavity of the cyst.

In what mode are we to penetrate into the cyst?

Almost always in grubs and little wens, often even in voluminous tumours, we find a black spot indicating the seat of the neck of the orifice, which is

supposed to be necessarily obliterated, and which is often only plugged. Nothing is easier than to remove the obstruction by pressure, or by the action of some very simple chemical or mechanical agent, a little soap and water, alkaline solution, the point of a probe, of a pin, or needle. We empty the cyst by pressing it; sometimes the thickened matter comes out gradually, in the form of a worm; sometimes, more fluid, it escapes in a jet, and is quickly and entirely expelled by the pressure of the fingers.

If this orifice is deficient, the point of a needle, or a little trocar, readily and painlessly substitutes for it an artificial opening.

Finally, if the cyst is very voluminous, if it is multiple, as I have seen in several instances, a double puncture, kept up by the presence of a seton during a few days, suffices to afford access to a single or multilocular cavity, and to give time and opportunity to express by degrees the entire contents, without exciting pain, or giving rise to any inflammatory symptom. To make this double puncture, and to introduce the seton, I prefer the trocar to the needle, the more rapid and less painful introduction of which forms openings larger, and less sensible to the contact of the thread. I then perforate the tumour through and through with a trocar, leaving the canula some moments *in situ*. Into this I pass the seton, which, therefore, does not touch the openings; then I withdraw the canula, and fix the seton by attaching its two extremities to each other. When the tumour is very voluminous (and I have been able thus to operate on some which were as large as a turkey's egg, or as an orange), I make use of M. Chassaignac's drainage trocar, the point of which, furnished with a notch to which the thread may be attached, allows the tumour to be perforated, the seton to be passed into the canula, and the latter to be withdrawn in less time than it takes to describe this little manœuvre.

The cyst having been emptied, how must we proceed to effect the obliteration of its cavity?

Strictly speaking, we may have recourse to one of two modes: either to modify its inner surface, to irritate it, inflame it, and determine by approximation and contact the adhesion of the opposite walls; or to produce its more or less rapid destruction, and its expulsion by the orifice, the cutaneous envelope quickly retracting, and gradually filling up, either by retraction or by deep adhesions, the void caused below it by the expulsion of the cyst. This latter mode is far preferable to the former.

At first, that is to say, about fifteen years ago, I was content to aim at simple modification of the walls of the cyst. To this end, after having expelled all the contents, I injected into the cavity various liquids. I had adopted, in preference to all others a strong solution of caustic potash (one part, and sometimes more, to fifty parts of water), thrown into the cavity by means of a small glass syringe. This solution had the advantage of first dissolving all the fatty or sebaceous matter which remained adherent to the walls, and then of irritating the latter, and disposing them to adhesion. Methodical pressure, continued or interrupted, favoured by the natural tendency to retraction of the skin and of the cyst, which were no longer distended by their contents, brought about by degrees the adhesion of the several portions of the sac, which eventually came into mutual contact, and the radical cure was finally attained after the lapse of a period varying from about fifteen to fifty days. On no occasion did pain or any other accident interfere with the treatment.

Since that time, thanks to the constant progress of cauterization applied to surgical operations, I gradually substituted for the caustic potash—of which I no longer made use, except to completely wash out the sac—agents more energetic, and more easily managed. Canquoin's paste, and particularly his various sparadraps, such as the school of Lyons has introduced into ordinary use, appeared to me perfectly to fulfil the object I proposed, and I have not ceased to employ them in the treatment of wens.

The following is the mode of proceeding:—A piece of sparadrap of chloride of zinc is cut of a length proportioned to the capacity of the cyst, narrow enough to pass through the opening; its penetration may be facilitated

by rolling it into a cylinder, so that the caustic substance occupies the external surface of the long and slender cylinder. It is always easy to push it into the interior, the natural orifice of the wen admitting of its passage with the slight aid of a director, a probe, or the head of a pin; the artificial opening, if it exists, being so much the more convenient, as it must have, as we make it, more considerable dimensions. For grubs, a piece of Canquoin as small as a grain of millet, or a lentil, is sufficient; for the more voluminous wens, a cylinder from one to three centimetres in length will be required. If the cutaneous opening appears insufficient, the end of the caustic is retained in it, instead of being forced entirely into the cavity of the cyst.

Pressure exercised on the wen at least twice a day gives exit, from this time, to a purulent matter, occasionally accompanied with small shreds of the cyst, and remains of the caustic sparadrap. But here two cases present themselves: either the cyst is small, very moveable under the skin which covers it; or it is very large, and adherent to the tissue in a great number of points.

In the first case, that is to say, when it does not exceed the size of a large nut, which is not so very small, one cauterization is often sufficient to effect a cure. Whether this takes place after one or several introductions of the caustic, it is usually accompanied with the expulsion of the modified cyst. By the use of pressure, the latter makes its appearance at the opening, whence it is easy to complete its extraction with the forceps, when its expulsion does not occur spontaneously. We then obtain the cystic sac intact, the dead fibrous tissues presenting a whitish or gelatinous appearance. It is unnecessary to say that the complete obliteration of the cavity, the retraction of the skin, and its adhesion to the deep-seated parts, soon follow upon the expulsion of the sac.

If patients were well informed as to the facility of freeing them without pain or danger from these grubs and little atheromas, which threaten to become large wens, we might with certainty prevent the development of every tumour of this kind. To introduce through the natural opening of the grub, or through an artificial opening made with a needle, after having evacuated its contents, a small portion of caustic of the size of a lentil, and thus to destroy the cyst, the expulsion of which takes place almost spontaneously, would be a matter of a few days. We should thus always prevent a small, indolent tumour from ever becoming an inconvenient or painful deformity; and we should avoid exposing the patient to the sufferings and dangers of an operation, which at last becomes indispensable.

In the second case, that is to say, when the cyst is voluminous, of long standing, multilocular, &c., we cannot hope for a termination so rapid or so complete. We must then reckon on the successive expulsion of shreds of the cyst, which gradually mortify, or on the adhesion of the points of the sac which shoot out sufficiently, in consequence of the irritating action of the caustic, to unite one to another. The successive introduction, at some days' interval, of several pieces of Canquoin's sparadrap, methodical pressure exercised on the wen either continuously or interruptedly, care to keep it always emptied of the products secreted in it, and to favour the retraction of the skin, and the contact of the opposite surfaces, finally bring about by degrees a result as advantageous as that obtained in the first case at a less expenditure of time and money.

The duration of the treatment may then vary from thirty to fifty days, but the result is always cure; and the patients attain this end without having incurred the least danger, or experienced the slightest pain. Contrary to what is true of the skin, the inner surface of the cyst has always appeared to me to be insensible to the action of caustics, all the benefit of which is consequently obtained, without our having to fear the reactions which follow the development of the pain inevitably connected with their application to the cuticle.

I have never observed, in the very great number of wens I have in this mode operated on, the production of a single one of the accidents which may follow any surgical operation.—*Bull. Gen. de Thérap., and Dublin Quarterly Journal, May* 1861, *p.* 487.

73.—ON THE PREVENTION OF PITTING IN SMALL-POX, BY THE APPLICATION OF LINIMENTUM AQUÆ CALCIS AND COTTON WOOL.

By Dr. Joseph Bell, Physician and Clinical Lecturer to the Glasgow Royal Infirmary.

[The writer of this paper has tried all the remedies or methods usually employed to prevent the pitting in cases of small-pox, and has found them each more or less objectionable. Solution of collodion certainly lessens the amount of pitting, but it often increases the irritation of the skin. Dr. Bell says:]

I have employed lard, oil, and the linimentum aquæ calcis, and found that if any of them were constantly applied, no irritation was produced, and that the pitting was very much modified. But as all these substances either run off the face soon or become dry, their benefit entirely depends on the assiduity with which their application is repeated.

Being favourably impressed with the effects of these, especially of the linimentum aquæ calcis, I was led to think that if it were applied on cotton wool, in the same manner as used in the treatment of burns, that the difficulty of keeping it applied might be obviated, and that the effect would be beneficial. Under these impressions, I applied it in several cases of confluent small-pox which were under treatment in the infirmary, immediately before the termination of my duties as physician to the fever hospital in 1854, and the result was so successful that, on my re-appointment in 1858, I employed it in every case of confluent small-pox with decided success. In some of the most severe cases of the disease, not a single pit was found to have been formed on the parts to which it was applied. At the point of the nose and around the mouth, where the patients contrived always to remove the dressing to some extent, numerous deep pits took place, and contrasted strongly with the uniform smoothness of the cheeks and other parts over which the dressing was permitted to remain undisturbed.

But, besides the effectual prevention of pitting, the dressing secured another important result—*it prevented the swelling of the face.* This effect was most marked. In the severest confluent cases, if applied early, the swelling never attained any great extent; and in cases in which great swelling had occurred prior to admission, the tumefaction of face soon subsided after the use of the dressing.

The febrile symptoms became considerably mitigated after its application; indeed this effect was so striking, that I have been induced to regard the swelling of the integuments of the face and scalp as being in a great measure the cause of the high febrile excitement which continues to exist in the confluent form of the disease; but whether or not, I state the fact that in cases of severe confluent small-pox in which the application was employed, the swelling of the face, and also the febrile excitement, were so slight as to arrest the attention in the most decided manner.

The Mode of its Application.—The linimentum aquæ calcis should be poured on a plate; then masses of cotton wool, answering in size and shape to the parts to which the dressing is to be used, should be dipped into the liniment, and applied in such a manner as to completely cover the face and neck, leaving apertures for the eyes, nostrils, and mouth. The cotton should be closely matted together, so as to allow no crevice to exist, and a large handkerchief should be tied over all, having holes cut in it so as to correspond with the apertures over eyes, nostrils and mouth. The dressing should be allowed to remain until convalescence, and if it becomes accidentally detached at any part it should be immediately renewed.

Dr. Stokes, in an excellent paper on the prevention of pitting, in the 'Dublin Journal' for February, 1859, speaks favourably of the advantages resulting from the linimentum aquæ calcis, but justly remarks that it rapidly becomes dry from the heat of the face. The use of the cotton wool, however, obviates this drawback, and at the same time completely secures the object which this distinguished physician states should be kept in view in the treatment, viz.:—

1st. The exclusion of air.

2nd. The moderation of the local irritation.

3rd. The keeping of the parts in a permanently moist state, so as to prevent the drying and hardening of the scabs.

In order to effect these objects, Dr. Stokes has employed linseed-meal poultices with decided success. I feel assured, however, that the three objects mentioned will be much better attained by the method which I have now recommended, and, I am also inclined to say, with much more comfort to the patient. The linseed-meal poultice must cause uneasiness from its weight, and no small amount of discomfort from the smell which must be produced. There can be no question but that the cotton wool will secure both the exclusion of air and light, keep the parts in a moist condition, and, as I have stated, will reduce the local irritation, as is proved by the rapid subsidence of the swelling of the face after the use of the dressing. The permanent nature of the application must be a great advantage over the frequency with which poultices require to be changed, as by this the air and light are often allowed to get access to the parts, and in this way detract from the value of any application which requires to be frequently changed.

The exclusion of air and light seems to be of the greatest moment; indeed it appears that if this can be accomplished by any plan, pitting will be prevented, or at least modified. It was on this principle that John of Gaddesden introduced the practice of blocking up the windows, and hanging scarlet cloth around the bed of the patient. In the 'American Journal of Medical Science' for 1832, Dr. Picton tells us that he had no cases of pitting from variola when the light was excluded from the room. In confirmation of these statements I may be allowed to remind the reader of the interesting experiments made by Serres. He placed a glass capsule over small-pox pustules, so as to keep out air and light; he ascertained that the pustule was rendered abortive in proportion to the exclusion of both, and that when both were completely excluded, the pustule rapidly dried up and left not the slightest scar behind. The same writer affirms that on one occasion he had to treat a number of small-pox patients in a kind of dark cellar attached to La Pitié, and that very little pitting took place in these cases.

It seems, therefore, that the exclusion of air and light will completely prevent the pitting in variola. I believe that no applications will be found more likely to secure these conditions than the one which I now recommend.—*Glasgow Med. Journal, July* 1861, *p.* 168.

74.—OBSERVATIONS ON THE TREATMENT OF DEFORMITIES RESULTING FROM SEVERE BURNS.

By Dr. JOHN K. BARTON, Surgeon to the Adelaide Hospital, &c., Dublin.

[The cicatrices left after severe burns produce, as is well known, much deformity. Dupuytren used to practice parallel incisions through these contractions, and afterwards an apparatus, which, by means of springs, would keep up a constant separation of the edges of the wounds until they had become healed. Subsequent operators have tried operations somewhat similar, but only with the result of repeated failures. Dr. Barton says:]

The causes of these unfavourable results from operations well planned and skilfully performed, will be found, I think, to arise from too much being expected from the operation, and too little attention being bestowed upon the subsequent process of extension or stretching; and that much more successful results will follow than have been hitherto obtained, by means of a careful and continued extension of the cicatrix, in some cases assisted by a cutting operation, such as has been mentioned,—in others, simply by subcutaneous section of the unyielding bands, and frequently not requiring any assistance from the knife whatever.

A consideration of the cause of these deformities favours this view of the treatment. The cause of the forcible contraction of the cicatrix which pro-

duces the deformity is the lymph which is shed in the repair of the ulcers left after the separation of the sloughs, which, following an invariable law, as soon as it becomes a part of the organized tissue, slowly but forcibly contracts. This law we may observe in many parts of the body; for instance, when the lymph is shed over the pleura, we find the walls of the thorax yielding to its contraction; and when poured out in the capsule of Glisson, we know with what a powerful grasp it compresses the liver. And in stricture of the urethra, it is the same substance which, shed upon or beneath the mucous membrane, produces such a train of evils by its tendency to close the canal; and tries the patience and skill of the surgeon, in overcoming its constant tendency to contract. Now, the treatment of stricture of the urethra has, for more than 200 years, occupied the attention of surgeons both in this country and on the continent; and while there has been great diversity of opinion upon many points, I believe I am correct in saying that all the ablest surgeons who have written on the subject agree that, in the great majority of cases, dilatation alone is the safest and the most successful treatment that can be adopted: that in some the use of the knife may be required to obtain a passage, in the first instance, which then must be maintained by dilatation; and that the cases which admit an instrument at all, and will not yield to dilatation, are comparatively very few; that, in fact, a cutting operation is only occasionally required, while careful and patient dilatation is almost invariably sufficient, either in conjunction with operation or alone, to produce a successful issue. Now, the cause of the contraction in each case being identical, we may with great advantage use the experience which we have gained in the treatment of stricture of the urethra to guide us in the choice of means for overcoming the contraction of the cicatrices of burns, especially when cutting operations have been so freely tried; and when unassisted, or only partially assisted by extension, have failed to produce the desired result.

Whatever mode of treatment we adopt, our great object must be to obtain, if possible, the absorption of the lymph, which is the contracting power: if this be removed, the case is cured. Now, no cutting operating will, of itself, produce this desirable object; on the contrary, it will cause the effusion of more lymph, which, being quite recent, will no doubt be far more amenable to extension than the old cartilaginous lymph; and so cutting may help. But the means which we may trust to, to gain the removal of the cicatrized tissue is *extension;* and this, in some cases alone, in others aided by the knife, will, when perseveringly employed, produce the absorption of the tissues of the cicatrix, and so the permanent removal of the deformities dependent upon it.

That the plan of extension which I am advocating is carried out with success at the Orthopœdic Hospital, and elsewhere in London, would appear from the following passage, which is taken from the 'Lancet' of August 13, 1859. I have not been able to find any more complete account of the treatment adopted there, nor any report of cases since this:—

"*Simple Extension in Contraction from Burns.*—We lately had the opportunity of observing the treatment of a case of deformity, arising from an old burn, in a little boy, nine years of age, under Mr. Coote's care at St. Bartholomew's Hospital. It is a plan, I believe, in use in the Orthopœdic Hospital, and consists in the proper application of simple extension perseveringly carried out. The boy was admitted on the 7th of April, with his lips and mouth drawn downwards from a burn in the neck when an infant. The cicatrix preserved the usual characters of hardness and thickening. By suitable appliances the head and chin were kept extended, with the effect of bringing back the lower lip and jaw to their natural position, and getting rid of the extreme deformity which had before existed. The mouth can now be closed. The effect of extension is to cause the absorption of the adventitious membraneous material present in the cicatrix, and thus permit the latter not only to become soft and extended, but permanently to remain so."

In the following case, it will be seen, I first performed James' operation, and then employed extension, aided by subcutaneous section. From the ex-

perience gained in the treatment of this case I would now *begin* with extension, carry it on perseveringly for some time; then, if I found it necessary, employ subcutaneous section of the most resisting bands of the cicatrix, and finally resort to the more severe operation, if I found I could not succeed without it.

Case 1.—Isabella M'Owen, a healthy girl, fourteen years of age, from the County Meath, was admitted into the Adelaide Hospital, upon the 22nd of March, 1860. When about eight years of age, her clothes having caught fire, she was severely burnt about the right side of the neck and head. Her mother had endeavoured to prevent the contraction which she was told would follow, by placing a stiff leather collar round her neck when the ulcers were healing, and for some time afterwards; the contraction, however, went on increasing in spite of this, and, as the girl began to grow up, she became very much dissatisfied with her appearance, and urgently sought that something might be done for her relief. The cicatrix occupied the whole of the right side of the neck, its densest and thickest part being close beneath the ramus of the jaw, extending from the lower part of the ear, which was involved in it, to the chin; from this central mass strong fibrous bands extended downwards below the clavicle, and as far forwards as the sternum; superiorly, the skin of the whole of that side of the face was drawn down to it; the angle of the mouth, and the external angle of the eye, being drawn downwards, the latter causing slight ectropium. The head was kept bent down to the right side, and when held straight caused increased distortion of the countenance; but by no effort could the patient turn her head to the left side.

The girl and her parents being very anxious to have something done to relieve her deformity, I determined to attempt it, my colleagues having examined the case, and agreed with me, and having also had the advantage of Dr. Hutton's advice, who kindly gave me his opinion as to the best mode of proceeding.

Upon the morning of the 28th, as soon as the patient was well under the influence of chloroform, I proceeded to operate in the following manner:—An incision was first made along the posterior edge of the cicatrix, from the mastoid process to the acromion, then a second along the anterior or inner edge, from the chin to the sternum; thirdly, another incision was carried across the cicatrix just below its central mass, connecting the two former. The two flaps thus formed were then carefully dissected from their attachments to the parts beneath, which was a matter requiring both time and care, as the skin, platisma, and fascia were all matted together and to the muscles beneath, by the dense fibrous structure of the cicatrix. As soon as this had been satisfactorily accomplished, the deformity of the face was found to have disappeared, and a gaping wound remained, extending from the ramus of the jaw to the clavicle. No vessel required ligature. Lint wet in cold water was laid on the wound, and the patient removed to bed. Considerable constitutional irritation succeeded, which, being followed by an attack of bronchitis, reduced the patient very much, and prevented me applying any instrument for keeping her head in proper position for some time. I found some difficulty, also, in getting any instrument made which would fulfil the indication, viz., to keep the head in such a position that the cicatrization of the neck could not deform the face; at last I succeeded in getting an instrument made by Read, which has answered so well, that with modifications, I think it will be found applicable to all cases of contraction about the neck or face. It consists of a shoulder-piece which, before the steel of which it is made was hardened, was fitted over the shoulder so as to sit closely and firmly; this was fixed in its place by two straps passing round the chest; from its upper side projected two steel slips, moving upon two others, which connected it with a firm cushion, which fitted against the ramus of the lower jaw, and was fixed firmly there by two straps. When the shoulder-piece and cushion were firmly strapped in their places, the head was forcibly drawn over to the left side, thus putting the parts between the jaw and the clavicle very much on the stretch, the two pairs of steel

slips sliding upon one another; the screws being then turned, the apparatus was fixed, and remained so the whole day; it was taken off at night, and reapplied, carefully, every morning. Recontraction was very rapidly taking place when this instrument was first applied; the bands of lymph in the cicatrix were extremely strong and unyielding, and the face was again being drawn into deformity; so that, from the first, the work this extending apparatus had to accomplish was to stretch the cicatrix, and thus cause its absorption. It became a matter of great interest to me to see if it could perform this; it was therefore carefully put on every day, for about three months. When about one month had elapsed, I saw some progress was slowly, but steadily, being made, and to aid the progress going on, I now divided subcutaneously, two or three of the most resisting bands; this was decidedly of service, so I repeated this about every fortnight, until at the end of three months the thickness of the cicatrix was very much decreased; it was evident that the dense fibrous tissue was being removed, and that the steady extension, aided by the subcutaneous section of the bands, was producing an absorption of the lymph of the cicatrix; a corresponding improvement had taken place in the deformity of the face; the eye was quite free, while the corner of the mouth was very slightly pulled down; the head, too, could be turned to the left side with freedom, and the face turned round to the left side completely; but when this latter motion was performed, the mouth and cheek were still a good deal dragged. The patient now went to the country,—her mother having learned in the hospital in a day or two, how to put on the instrument, and undertaking to apply it daily, and bring back the girl in November, which she did, having been absent about two months. Her neck remained very much in the same state when she returned as that in which it was when she left the hospital, but her general health was much improved. No doubt the apparatus was not kept on as regularly, nor applied as firmly, as it should have been; upon her readmission, however, it was again carefully put on, the most prominent bands in the cicatrix being again divided with the tenotomy knife, and a progressive improvement took place; the skin being soft and pliant where it had been hard and puckered, the deformity of the face disappearing at the same time. All this, however, took place very gradually. She was still under treatment in March, 1861, when, on account of the death of a sister in the country, she suddenly left the hospital. Her parents did not wish anything further to be done, expressing themselves very much pleased with the improvement which had taken place.

In this case, as in most others of a similar kind, the extending apparatus was put on at first with the object of keeping the edges of the wound separated while cicatrization went on; but very soon I found it would have to accomplish much more than this: if I was to obtain a favourable result, it would have to produce the absorption, to a great extent, of the old lymph of the burn and the new lymph of the wound, which together were rapidly reproducing the deformity. This it accomplished after being steadily and perseveringly employed for six months, aided, during a part of this time, by a subcutaneous division of the most resisting bands.

In proceeding to treat any case of this kind by extension, it will be necessary to have an instrument which will fulfil the indication, viz., to keep the cicatrix constantly and firmly extended. The difficulty of accomplishing this, particularly when the neck or face are the parts to be operated upon, is one cause why so little has been accomplished by this means. The instrument I used in this case I found to answer very well; it is very simple, and, when properly put on, it remained *in situ* without slipping, and kept up very powerful extension on the cicatrix for twelve hours daily. In putting it on, care must be taken that the lower piece is firmly fixed to the shoulder by the straps attached to it, and also that the cushion is firmly fixed against the face by the straps going round the head, before extension is made; by attending to this, an intelligent servant, or relative of the patient, will readily learn to fix it on daily. After a few days, in the case I have related, it gave the patient but little annoyance, habit and the hope of a cure quickly

reconciled her to its use. An instrument made upon the same principle, viz., a lower piece resting upon some part where it can be firmly fixed, connected to an upper movable cushion by slips sliding on one another, and fixed by screws, can be readily adapted for each particular case.

An objection which may be fairly urged against the treatment by simple extension, is the length of time it takes to produce the desired result. Now, in a case where the thickness and extent of the cicatrix is great, no doubt it will be a very tedious process; in such cases I would recommend the adoption of subcutaneous section of the most resisting bands of the cicatrix; I found it a valuable auxiliary in the cases I have related. I generally divided two or three of the most resisting bands at one time; in some cases the skin over the dense tissues divided, being very thin, and involved in it, either gave way or were cut through; but in no case was there any constitutional disturbance, and only a few drops of blood were lost each time, while undoubtedly the extension was very much facilitated by it.

While the majority of cases will be most successfully treated, I believe, by simple, persevering extension, aided, when the cicatrix is situated in the neck, by subcutaneous section, yet there are some cases, where the cicatrix is very large and the mass of organised lymph very great, which will not yield to these means, just as there are some cases of stricture of the urethra which will not yield to dilatation, but demand, on account of their dense cartilaginous character, some other measures. In such cases, after having given a fair trial to simple extension, I would then operate, and as soon as possible put on the extension-apparatus again. As to the mode of operating, the proceeding adopted in M. Owen's case, first recommended by James, will frequently answer very well; or a V-shaped incision may be made both above and below the cicatrix, *through the healthy skin, close to the cicatrix, but not through it*, which may then be dissected from its attachment sufficiently to admit of extension to the desired extent; this plan I have seen successfully practised by my colleague, Dr. Walsh.

Besides those cases of extensive and cartilaginous cicatrices which we must seek to modify by an operation before we can successfully employ extension, there is another set of cases which will be advantageously treated by a cutting operation in the first instance; these are cases where a web has been allowed to form between adjacent parts, such as between the fingers or toes, or between the arm and side, by which the motions of the part are crippled, if not destroyed. In such cases we can remove by the knife a great part of the contractile tissue, and yet, as the parts will allow of being drawn together without any deformity resulting, leave a very small surface for granulation, and by subsequent extension we can prevent the re-formation of the web. The following case, which is still under observation, exemplifies well this kind of cicatrix, and the mode of its removal.

Case 2.—Mary Kelly, ten years of age, a healthy-looking little girl, was brought up to the Adelaide Hospital, from the neighbourhood of Navan, upon the 13th of April last, for the purpose of having something done to restore, if possible, the motions of her right arm, which was connected to her side by a very strong cicatrix, which rendered the limb almost useless. Her mother stated that when the child was only four years of age her clothes caught fire, and she was very severely burnt along her right side and arm. She was attended by a doctor; but, after he had ceased to visit her, the child remained quite unable to move her arm from her side, on account of the pain which any motion of the limb gave her; the sores remained open for nearly a year; as they were gradually healing, she observed that the arm was contracted to the side, and could scarcely be moved. Soon after this she brought the child to Dublin, to see what could be done for her. She saw Dr. M'Dowel at the Whitworth Hospital, and was advised by him to return to the country for another year, at the end of which time, if she brought back the child, he would be able to do something for her relief; in the mean time the arm was to be exercised as much as possible. No doubt Dr. M'Dowel remembered the advice given by Dupuytren, not to operate upon the cicatrices of burns until they had fully contracted. Various circumstances occurred to prevent her

bringing the child at the time named, and she allowed five years to elapse, until, being urged by her friends to have something done, she came to the Adelaide Hospital.

The cicatrix was both very extensive and thick; it completely filled up the axilla, extending two inches below the bend of the elbow on the outside, and as far as the last rib on the inside; it was as thick as the fore-finger, and very dense. The motions of the arm were, as may be supposed, greatly interfered with: in the first place, it could not be separated fr :m the side for more than six inches, measured from the elbow to the chest; the backward and forward motions were very much limited; and the fore-arm could not be straightened on the arm. The muscles of the arm and fore-arm were very much atrophied, but the bones had attained the same size as the opposite limb. The artery retained its normal position.

I determined in this case before applying extension, to perform a cutting operation, for the following reasons:—1st. The density and unyielding character of the cicatrix made it doubtful whether it would yield to extension alone, and certain that it would be a very long and tedious process: 2nd. The peculiar position of the cicatrix made it quite possible to remove a great portion of the contractile tissue altogether, leaving at the same time only a small surface to heal by granulation.

Accordingly, upon the 24th of April, the child being under the influence of chloroform, I proceeded in the following manner:—An incision was first made along the outside of the cicatrix and inside of the arm, extending from the top of the axilla to about three inches below the elbow-joint; the knife was then turned round, and carried from the last-named point along the free edge of the cicatrix to the side of the thorax, when a third incision along the inside of the cicatrix was made from the point where the first began to where the second terminated; thus a triangular piece was included, which was the whole anterior surface of the cicatrix; this was now rapidly dissected from the posterior part, and removed; the posterior layer of the cicatrix was then divided perpendicularly from above downwards, its external part brought over the arm, and its internal over the chest, and the edges then drawn together by seven or eight points of twisted suture, and well-supported by broad strips of adhesive plaster; wet lint was laid over all, and the child removed to bed. Some of the needles were removed upon the third day; two were allowed to remain, where there was much strain for two days more. When these were withdrawn, the lips of the wound retracted to some extent, so that the surface to be healed by granulation, was, upon the side of the chest, two inches in width and six in length; upon the arm it extended from the axilla to three inches below the elbow, but was not more than an inch and a half broad anywhere. At the apex of the axilla there remained, however, a surface fully four inches across for granulation; the extent of this surface rendered the secondary treatment of keeping the parts extended while granulating very important.

The apparatus I employed for this purpose consisted of two parts—1st, a simple elbow-splint, which, being fixed to the arm and fore-arm when bent, could be strengthened and kept at any angle by a screw; by means of this in a day or two, the fore-arm was brought into a straight line with the arm, and kept so during the rest of the treatment: 2nd, to keep the arm from the side, and thus prevent the re-forming of a cicatrix between the parts, I adopted the following plan: a broad well padded girdle was strapped around the child's waist, resting against the pelvis, from which, directly below the axilla, and united to it by a hinge-joint, extended a steel rod, about two feet and a half in length, curved so that a rest for the arm could, as the arm was raised from the side, easily slide along it; by means of a screw, the rest could be fixed at any part of the rod, and thus the elevation of the arm easily regulated and maintained at any point. I found this instrument work remarkably well; the only change it became necessary to make in it was to have the handle of the screw, which fixed the position of the rest on the rod, filed off, and a key made to turn it, which being in the custody of the dresser who had charge of the case, put it out of the power of the little patient or the nurse

to let down the arm after it was fixed. As soon as the needles were removed this instrument was put on, and the arm held at right angles to the chest, and the fore-arm in a straight line with the arm. Both night and day the instrument was kept on, causing the patient no annoyance after being carefully fixed in the morning. As soon as granulation had fairly set in, I found it necessary to raise the arm higher than a right angle from the side, so strong was the contracting power, and also to repress the granulations of the axilla, which had a tendency to grow down rapidly, by the frequent application of nitrate of silver, and by tightly strapping the shoulder with plaster. This tendency of the granulating surface to extend downward, while at the same time contracting so as to draw the arm to the side, explained what, when I first saw the case, seemed very hard to comprehend,—how such a dense mass of tissue could have been formed between the arm and the chest. The connecting tissue grew from above downwards, uniting, as it extended, the granulating surface upon the chest to that upon the arm. This case is still under treatment (June 28); and although it is more than two months since the operation, a great part of the granulating surface is yet unhealed. This excessive slowness arises from the position in which the arm is necessarily maintained which position will I believe have to be maintained more or less for the next three months, before the danger of recontraction will be over. The motions of both elbow and shoulder are now nearly entirely regained, passive motion being employed daily.

This was, without doubt, a case in which much was gained by commencing with a cutting operation; yet the struggle only began with this, which was the easiest part of the treatment; and the final result—the restoration of the motions of the right arm—depends more upon the less brilliant subsequent treatment.

All cases of deformity produced by extensive cicatrices are serious cases; and the surgeon will do well to bear in mind, when undertaking their treatment, the obstinately contractile character of the morbid tissue against which he is about to proceed; that to obtain the removal of this tissue must be the object of his treatment; that in no case can this be obtained by a cutting operation alone; that extension, if patiently and perseveringly employed, will cause the absorption of this tissue; and that, therefore, how this can be efficiently employed in any particular case, must first occupy his attention. Then he may consider in what way this process may be facilitated; and here the knife will furnish him with the aid he requires—thus considered as an aid to extension, there will be no disappointment to surgeon or patient at the result of the operation. And when performed after and during efficient extension, it will be far more successful than it is found to be when performed first, and as the principal part of the treatment.

In these observations I have scarcely alluded to the plastic operation, because it is unsuited to the kind of cases I have been considering, viz., extensive and deep cicatrices. In cases where a flap of healthy skin could be obtained, and where the cicatrix was so small that there would be no likelihood of sloughing of the transplanted piece, such as a narrow cicatrix of the cheek, causing ectropium, I would prefer this mode of proceeding to any other, and have seen it very successfully adopted.—*Dublin Quarterly Journal, Aug.*, 1861, *p.* 1.

75—TREATMENT OF SCABIES BY BENZINE.

By J. J. Godfrey, Esq., Malvern.

That sulphur, though a certain, is by no means a satisfactory specific for scabies, is evidenced by the appearance of statements, from time to time, of the success of other applications; and, notwithstanding Professor Hebra's assertion that "all methods of treatment which do not include sulphur are more or less uncertain," it is well known that the itch-insect can be destroyed by several other agents, such as iodide of potassium, creasote, &c. That will be the best remedy, which is easiest of application and quickest in

action. Dr. Metzel employs phosphorated oil; which, however, like sulphur, requires several applications.

I wish to draw attention to a remedy which I have seen recommended; but which I believe has not been largely tried, at least in England, and which appears to be almost instantaneous in its action; I refer to benzine. M. Bonnet in 1858, noticed this as a specific for itch, the cure being effected in a few minutes. M. Rey also speaks of cutaneous insects being destroyed by benzine, and suggests its employment in scabies. During the last five months, I have used benzine in seven cases of itch, and with perfect success; one application having proved sufficient. Three cases (one very severe) occurred in the same family. Three ounces of the fluid were supplied, with directions to sponge every part of the body with it where any rash or itching was noticed. This was done in the evening, and the whole was used. The application was not repeated; and after a month there had been no return of the disease. The number of my cases is so few to draw any strong conclusion from, that I should be glad to know with what success it has been used by others. Should benzine prove constantly, or even frequently successful, its application will, to say the least, be a pleasanter mode of dislodging the acarus scabiei, than bedaubing the body with sulphur ointment.—*British Medical Journal, Sept.* 28, 1861, *p.* 343.

76.—ON THE TREATMENT OF PHTHIRIASIS.

By Dr. T. McCall Anderson, Fellow of the Faculty of Physicians and Surgeons, Glasgow.

The treatment of phthiriasis is abundantly simple. Many are the medicines and varied the forms which are used for this purpose. The preparations of sulphur, mercury, staphisagria, sabadilla, pyrethrum, the essential oils and alcohol, are principally employed.

Sulphur is used in the form of vapour-baths or fumigations, or the simple sulphur ointment of the pharmacopœia may be employed.

Mercury may be applied in the form of simple mercurial ointment, or fumigations with cinnabar, or a lotion of the bichloride in the proportion of two or three grains to an ounce of water, the solution being facilitated by a few drops of alcohol.

Staphisagria in powder is a very good preparation, or an ointment made by mixing an ounce of staphisagria with four ounces of lard and a few drops of an essential oil, as the oil of rosemary, to improve the odour. Or an infusion of staphisagria may be made with vinegar.

Sabadilla is employed either in powder or ointment, the latter consisting of a drachm of sabadilla to an ounce of lard, and scented as above.

Pyrethrum is generally applied in powder. Any of the essential oils may be used, the oil of rosemary being generally selected.

In the case of lice attacking the head, the hair should be cut short, if it is long and entangled—especially if the scalp is the seat of secreting eruptions, as we are thus enabled to remove many of the nits adhering to the hair, and to get more readily at the pediculi. A lotion of the bichloride of mercury, or the powder or ointment of staphisagria, or simple lard or oil should be applied, which causes the death of the insect. It is then necessary to remove the nits, which may be done by applying repeatedly either alcohol or dilute acetic acid, as Hebra recommends. These, after the use of the spirit, cease to adhere to the hair and may then be removed by combing. The head should also be repeatedly washed in warm water with soap. The eruptions of the scalp left after the removal of the insects must be treated according to their nature.

There is just one remark which remains to be made with regard to the treatment of pediculi capitis,—one which has been very prominently brought forward by Devergie,—and it is this,—that, in those cases where the head has *long* been attacked by *great numbers* of pediculi, especially in the case of children, it is dangerous to eradicate them suddenly, for by so doing inter-

nal diseases have been developed; and Devergie has seen two infants who died from this sudden cutting short of the accustomed itching and secretion. It is, therefore, necessary, in such instances, to cure the disease more slowly, to attack only small portions of the head at a time, and especially to avoid cutting short the hair at once. It is only in very rare instances, however, that such precautions are required.

The pediculus corporis is best attacked by sulphur vapour-baths, or mercurial fumigations, or the powder of staphisagria or pyrethrum. If one of these powders is used, warm-baths should be superadded. The clothes should be purified by exposing them to a temperature of 150° Fahrenheit, or as the common people do sometimes, and which Küchenmeister has seen successful, by burying them in hay for several weeks. Washing clothes may be put into boiling water, or cloth clothes exposed to the steam rising from a boiler. The pruriginous eruption which the lice have occasioned will disappear after they have been killed; but any constitutional eruption, such as eczema, which may have been called forth by the irritation of the scratching, or which existed previous to the occurrence of the lice, must be treated on general principles. When an eczema exists, it may sometimes be necessary to treat it first of all (on the same principle as an ordinary eczema) before attacking the pediculi; for they are very apt to lie hidden among the crusts, and soon multiply again, giving as much annoyance as before. This remark is applicable to all the forms of phthiriasis.

The pediculus pubis is easily killed by rubbing into the *roots* of the hair a lotion of bichloride of mercury, which is invariably effectual, provided it be brought into *direct contact* with *all* of them. The application should be made, not only to the hair of the pubis, but also to the hair about the scrotum, perineum, and anus, which should be repeated daily for a week. But if any excoriation exists, care must be taken not to use the lotion too freely, else salivation may be induced. In cases where there are many excoriations or abrasions, it is safer to use an ointment of sulphur or staphisagria, or an infusion of the latter, or the oil of rosemary. Mercurial ointment is the preparation most in vogue for this complaint, but it is a dirty application, and I never employ it.

There are very many other remedies which have been successfully used, but is quite unnecessary to allude to them further, for those already mentioned are amply sufficient, when properly used, to kill the pediculi, no matter how numerous they may be.—*Med. Times and Gazette, June* 8, 1861, *p.* 598.

77.—ON THE TREATMENT OF ECZEMA.

By Prof. Hebra.

One has the satisfaction, Professor Hebra observes in his clinical lecture, of knowing that we can always cure eczema, however long it may have continued, although in some inveterate cases depending upon a dyscrasis, or upon internal causes, there may be considerable difficulties in the way. As in most other diseases of the skin, the treatment should be purely local, internal measures being limited to the rare cases in which the eczema has been produced by a previously diseased condition of the economy, or when it is combined with some other affection. In the great bulk of cases internal means, such as mercury, antimony, iodine, purgatives, sarsaparilla, &c., are superfluous, and mischievous. Arsenic is the only one of such means which exerts any influence in obstinate cases. Ordinarily, however, it is of no use, and its employment should be limited to the few cases which manifest especial obstinacy, the local treatment in these also not being at the same time neglected.

Cold water, in its various modes of application, is of great importance, combined with other means, in the treatment of eczema. Employed alone it is far too tedious, and frequently not sufficing in its operation. It only aggravates the malady when applied in *eczema simplex*, arising from excess in

secretion, as *e. g.* in the axilla, between the buttocks, &c. Starch, whether alone, or mixed with oxide of zinc (starch ʒj, zinc ℥ iij) is, on the other hand, an excellent application when the eczema arises from the friction of two cutaneous surfaces, or from excessive secretion, as in the axilla, under the breasts, the scrotum, buttocks, &c. Oxide of zinc (ʒj, to ℥j of lard), sulphate or acetate of zinc, alum (ʒi to ℔j of water), red or white precipitate (6 to 12 grains to ʒij of lard), are of good service in acute eczema or in chronic when there is but slight infiltration and the disease prevails only over a limited extent. When, however, there is considerable infiltration in chronic eczema these means do not suffice, and then the almost indispensable *schmierseife*, by reason of its slightly caustic action, becomes the most preferable remedy. Its mode of employment varies according to the degree and extent of the eczema. When there is but slight infiltration, a rubbing with it once or twice a day is sufficient; but when the infiltration is more considerable, a more frequent application is necessary, and even epithems composed of it may be required. When we have thoroughly rubbed the skin with the soap, until excoriations and red points have been produced, the surface should be washed and cold applications laid on until next rubbing. This procedure must be continued as long as the moisture and itching and the infiltration of the skin continue, and until the frictions no longer give rise to heat and excoriations of the skin. The soap must then be replaced by cold applications, and the treatment terminated by the employment of tar.

In many chronic cases, with great infiltration, the soap does not suffice, and we must have recourse to a stronger form of potash, viz., caustic potash, 1 drachm dissolved in 2 drachms of water. A pencil of charpie is dipped in this and well rubbed into the diseased parts for some minutes. These are then washed, and cold applications are kept constantly employed. One or two such cauterizations usually suffice, and when more are necessary they should not be repeated oftener than once a week. Carefully applied, the caustic gives rise to no cicatrix, but the practitioner must himself always undertake its application. It is a very painful procedure, and is not often required. Cauterizing with strong acids is to be avoided, as it causes great pain and gives rise to scars. The application of the nitrate of silver is of no avail. *Tar* is in its way just as useful a remedy as the soap, the time for its application being when the moisture and itching have ceased, and exfoliation has commenced, *i. e.* when *eczema squamosum* is present. It may be used either alone or mixed with equal parts of cod-liver oil, and should be applied by means of a brush once or twice a day, carefully avoiding washing the parts or allowing water to come into contact with them. As long as any redness or desquamation continues, the tar must be repeated. Sometimes, when the application of the tar has been premature, moisture and itching are observed at certain spots, and the preliminary treatment has then to be resorted to again. Some individuals cannot bear the tar at all, it giving rise to severe inflammatory action. An ointment of acetate of lead or oxide of zinc should in such cases be substituted. *Cod-liver oil* is a valuable external application, and by its aid alone we are able to cure the eczema when this has not lasted very long and the infiltration is not very considerable. It is also an excellent adjuvatory to the treatment by *schmierseife* and cold applications, as flannels soaked in the oil may be kept bound over the diseased parts during the night. Employed alone, the treatment is very tedious, and is objectionable on account of the disagreeable smell and befouling the linen which it gives rise to. Taken internally it does not exert the slightest influence on eczema.—*Wiener Spitalszeitung, and Glasgow Med. Journal, April* 1861, *p.* 107.

SYPHILITIC AFFECTIONS.

78.—ON SYPHILIS.

By Henry Lee, Esq., Surgeon to the Lock Hospital.

If there be one pathological fact that has been more frequently reiterated of late years than another, it is that a primary syphilitic sore may be distinguished from every other affection by its secretion when inoculated, invariably producing the characteristic syphilitic pustule. Although syphilitic inoculation was practised by Hunter and other accurate observers, it remained for Ricord to draw well-digested conclusions from the facts which this mode of investigation presented for our interpretation. By patient experiment and rigorous reasoning Ricord established the truth, which has now been confirmed throughout Europe from Italy to Norway, namely, that certain forms of syphilitic sores will produce, when inoculated, their like, which in their turn will again furnish a secretion capable of being again inoculated with similar results. It would be difficult to overrate the importance of this discovery, so analogous to that of the small-pox inoculation. But practical men have, nevertheless, been unable to see in the experiments in question a conclusive representation of that which they daily observe in practice. That certain forms of primary syphilitic disease will produce the characteristic pustule, when inoculated, all must allow: but it does not follow, on the one hand, that all primary syphilitic sores are alike capable of being inoculated; nor, on the other, that when inoculated, they will necessarily yield the same result. Under these circumstances, and without proof as to these two last mentioned particulars, to say that a sore is syphilitic because it produces a characteristic pustule when inoculated, and that it produces a characteristic pustule because it is syphilitic, is manifestly to reason in a vicious circle. The propositions themselves are undoubtedly true; but they cannot, without further proof, be received as excluding other modes of syphilitic infection and other forms of primary syphilis.

We are led then to ask, in the first place, whether there is no form of primary syphilis except that which commences as a pustule. This question is important, since surgeons engaged in practice very seldom have an opportunity of observing the primary syphilitic pustule; and still less frequently have they an opportunity of tracing any secondary symptoms consequent upon it. Now, during the years 1855 and 1856 we examined at this hospital the secretion from a large number of primary syphilitic sores, and amongst these were ninety-five cases presenting all the ordinary characters of the primary infecting sores. The secretions from these ninety-five cases consisted of epithelial *débris* floating in a serous fluid, or of globules of various shapes and sizes, which did not, upon the addition of acetic acid, yield the well-defined nuclei characteristic of pus. In a very considerable number of cases the secretion appeared to consist of nothing but epithelial *débris* and serous fluid, and in some the sores yielded no fluid at all.

What reason, then, have we to believe that these primary sores commenced as pustules; or that, unless artificially irritated, they would produce pus at all? And, secondly, what evidence have we that every syphilitic sore produces a secretion capable of being inoculated in the same manner? It is to this point that I wish principally to devote this lecture.

When the secretion is taken from a chancre during its period of progress, or when stationary, and properly inoculated, the following results may, according to M. Ricord, be invariably observed. During the first twenty-four hours, the inoculated point becomes red; from the fourth to the fifth day, the secretion becomes purulent; and a pustule, with a depression in its centre resembling those of small-pox, is fully formed. ('Traité Pratique,' p. 89.) The inoculation never fails when properly performed. "*L'inoculation n'échoue jamais quand on prend le pus dans les conditions voulues et qu'on l'applique bien*" (p. 94). The appearance produced is said to be regular, characteristic, and uniform (p. 135). It is useless to multiply quotations upon this point, the

facts concerning which have been so generally received and so often republished.

In the year 1856, I published an account of some cases in which, notwithstanding the assertions so unreservedly made and so generally received, I had not been able to produce any effect by ordinary inoculation. The experiments were tried in this hospital, care being taken to select instances in which the sores had not begun to heal; and among the cases were the following:

Thomas C., aged 16, was admitted on the 27th of November, with an indurated sore extending half-way round the margin of the prepuce, causing phymosis. This had commenced a fortnight previously. The secretion from the sore was carefully inoculated on the patient's thigh in several points. The inoculations were followed by no result, and the patient was soon afterwards affected with secondary syphilis.

James G., aged 25, had a large indurated sore near the orifice of the prepuce, which had appeared as a pimple four weeks previously. The secretion was carefully inoculated in several points, but without result. Constitutional syphilis followed.

Matilda P., presented on the left external labium a well marked indurated sore, which she stated had existed one week only. The secretion, which consisted of a thin serous fluid, was carefully inoculated. No result followed the inoculation.

Julia B., aged 21, had a red glazed sore on the external labium, surrounded by distinct specific induration. The disease had commenced three weeks previously, as a pimple. The secretion from the surface of the sore was inoculated without result.

Since the period above referred to, we have inoculated the secretion from a considerable number of sores presenting the characters of the specific adhesive inflammation; and, as a rule, no result has been obtained where no artificial irritation has been applied. The secretion in these cases has consisted chiefly of epithelial *débris* floating in serum more or less turbid. The character of the secretion of these sores may readily be altered by anything that is brought in contact with them. The application of caustic, or a thick scab which confines the secretion, or a piece of linen which sticks to the sore, will produce a temporary discharge more or less puriform; but dress the part with wet lint for a day or two, and the natural character of the discharge will again be evident.

As a rule, then, I say (at least, that has been our experience at this hospital) that this kind of primary sore is not, under ordinary circumstances, capable of being inoculated with the lancet; and we therefore cannot but come to the conclusion that those who have maintained that all primary syphilitic sores can be alike inoculated have generalized too hastily. But a much more important question remains; namely, Does it follow that, because these sores are not capable of being inoculated by the lancet in their ordinary condition, therefore they cannot be communicated at all? And, if capable of being inoculated under certain circumstances, are the results produced identical with those produced by the inoculation of the secretion from suppurating sores?

In order to determine these questions, I would request your particular attention to some cases now in the hospital, and particularly to that of a boy named A. The details of this case, and the results of the inoculations performed, I will immediately mention; but, before doing so, I would draw your attention to the fact that in practice we continually meet with cases which in their earlier stages are similar to the one I am about to describe, and which, with or without treatment, become covered by epithelium; and, although the specific induration remains, yet the surface of these sores affords no fluid secretion whatever. Inoculation in the ordinary way at this period of the disease would be entirely out of the question. But, if a person in the condition I have mentioned get married, his wife will have a good chance, even before her pregnancy, of becoming infected with syphilis. Some very distinct cases where this has happened have fallen under my own

observation. How, then, is the disease communicated in these instances? We have here a rather numerous class of cases in which the sores have become covered over with cuticle, which yield no pus nor fluid secretion of any kind and yet which are capable of communicating infection from one individual to another. How, I ask, does the inoculation take place from a sore affected only with the specific adhesive inflammation either before or after that sore has apparently healed? Some light will, I think, be thrown upon these questions by the careful consideration of the facts presented to our notice in the boy A., at present in the hospital.

This lad had had gonorrhœa six months before his present attack, but otherwise he had never had any venereal symptoms. His present disease was of about a fortnight's duration. He first perceived a superficial sore behind the corona glandis, which healed in a few days. Two or three days after the first appearance of this sore, a little pimple appeared on the outer skin of the prepuce. This was squeezed, and discharged a watery fluid. A circular sore then formed, which continued to increase. He applied to me on the 25th of July, with a well marked Hunterian chancre. This was of a circular form, surrounded by a well marked and accurately defined induration, and discharged a white turbid secretion. This fluid was placed under the microscope, having previously been mixed with a little acetic acid. It contained no pus-globules. The glands in the groin were enlarged and indurated, but not inflamed.

July 27th. The secretion from the sore was inoculated in several points on the patient's thigh.

July 29th. The boy was admitted into the Lock Hospital. The inoculations had been followed by no result. The secretion from the sore was again examined, and found to contain no pus.

July 31st. The sore, which continued to increase in size, had been dressed with linen and cold water since last report. The linen appeared to have irritated the sore in some degree, and numerous globules now appeared in the secretion; but, upon the addition of acetic acid, the distinct outline of the pus-nuclei were not visible. Several fresh inoculations were made. The sore was dressed with wet lint.

Aug. 3rd. None of the inoculations had succeeded. The secretion from the surface of the sore, placed under the microscope and treated with acetic acid, did not appear to contain any pus. The sore was now ordered to be dressed with the acetum lyttæ. The glands at the back of the neck were now enlarged, and skin presented for the first time the appearance of a syphilitic eruption.

Aug. 5th. None of the former inoculations had been followed by any effect. The application of the acetum lyttæ had produced a superficial slough on the surface of the sore, and blistered the surrounding skin. Some distinct pus-globules were now visible in the secretion from beneath the slough. This secretion was inoculated upon the thigh in several points.

Aug. 7th. The sore now again secreted no pus. Fresh inoculations were performed.

Aug. 10th. The sore was dressed twice yesterday with the unguentum sabinæ, and it now yielded a copious secretion of pus. This was inoculated in several points in a fresh place upon the thigh.

Aug. 12th. The inoculations last made had succeeded. The primary sore still yielded a copious secretion of pus.

Aug. 14th. The sore had been dressed with lint, kept wet with water since last report, and now no pus could be detected in the secretion. The inoculations both of the 5th and the 10th had now succeeded. They presented the appearance of circular red patches, with some elevation and thickening of the cuticle. In one place there was the appearance of a broken vesicle, from which a serous secretion exuded. This secretion from the inoculation was again inoculated on the thigh.

Aug. 17th. The inoculation from the inoculation had succeeded. It presented the appearance of a red circular patch, from which the cuticle was abraded, with slight thickening of the skin. It had not at all assumed the

appearance of a pustule, nor was anything like pus secreted from its surface. A single pustule, surrounded with very little inflammation, had formed in one of the points first inoculated. The eruption on the surface of the skin was fading. All the inoculations in this case were made with a lancet used for no other purpose, kept carefully wiped and wrapped in paper.

Aug. 19th. The inoculations appeared as separate red patches on the skin. The skin in these situations was slightly raised and thickened, but no induration extended into its substance. The solitary pustule which had appeared had dried up. The original sore was healing; but, being dressed with the unguentum sabinæ, it afforded a purulent secretion, which was inoculated upon another patient who was apparently suffering from organic disease of the liver.

Aug. 21st. The inoculations presented the same appearance as before; they appeared covered in certain parts with thin scales. They were tender to the touch, with slight thickening on the surface, which did not extend into the substance of the cutis. The inoculations nowhere presented any appearance of ulceration. The inoculations performed on the 19th had produced a small dark brown circular patch, in which the skin was slightly elevated. The secretion from the original sore was now again inoculated on a second female already affected with constitutional syphilis.

Aug. 24th. One of the inoculations on the boy had a slight tendency to ulcerate; the others were desquamating and losing their colour.

The single inoculation on the first woman had assumed the form of a small red pimple. The redness gradually faded into the colour of the surrounding skin. The cuticle at the inoculated part was thickened and elevated.

The inoculation on the second woman, already affected with constitutional syphilis, was less marked. There was only a slight redness and elevation at the inoculated part.

The particulars of these last two cases we may follow out on a future occasion. From the facts which have already been brought under notice, the following very important points are proved:—

1. That some primary syphilitic sores cannot readily be inoculated in the ordinary way upon the patient who has them, either during the period of progress or afterwards.

2. That the sores which are not capable of being thus inoculated, as far as we have hitherto seen, are those affected with specific adhesive inflammation, and which do not, except under conditions of artificial irritation, secrete pus.

3. That these sores, although not capable of being inoculated in the ordinary way, will nevertheless, when irritated, furnish a secretion which is capable of being inoculated upon a patient already affected with syphilis.

4. That the inoculations thus produced do not give rise, as a rule, to either suppuration or ulceration, but to some adhesive form of inflammation.

The inoculations to which attention has now been directed, and the results of which may be seen in two other patients now in the hospital besides those to whom I have above referred, have all assumed a remarkably uniform character. There are many points of the greatest interest connected with this newly demonstrated form of artificial inoculation, which must be carefully distinguished from the effects of inoculation upon a patient not previously diseased. Here, after a certain period of incubation, a well marked Hunterian chancre would probably have been the result of each inoculation performed for the first time.—*British Medical Journal, April* 20, 1861, *p.* 407.

79.—TREATMENT OF GONORRHŒA BY INJECTIONS OF LIQUOR HYDRARGYRI PERNITRATIS.

By C. Borlase Childs, Esq., Surgeon to the Metropolitan Free Hospital.

The following report has been furnished us by Dr. John Warner, resident medical officer.

The effects of the above remedy, employed by Dr. G. Borlase Childs in the treatment of gonorrhœa, have been watched with much interest. In laying his experience of this remedy before the profession, Mr. Childs observes; "Gonorrhœa is a disease so common in large cities, and so well understood by medical men generally, that any farther observations on the subject may be regarded as not only unnecessary, but altogether unacceptable and tedious. There is no disease, perhaps, which affords so wide a field for fancy treatment, and there are few practitioners who are not wedded to some peculiar treatment of their own. Whilst some rigidly pursue the antiphlogistic treatment, others discard it altogether, and strive to arrest the disease by the immediate exhibition of those remedies which are generally regarded as exercising a specific control over it. In both cases the unfortunate patient is dosed *usque ad nauseam*. The functions of the stomach become impaired, the general health suffers, and, though the acute character of the symptoms is subdued, the patient is frequently left with a troublesome gleet, aggravated and prolonged by the continued use of copaiba and cubebs. Such cases are of common occurrence, and the result of my experience leads me to the opinion that chronic discharges from the urethra are oftener the result of injudicious treatment than of the disease itself. With some practitioners there appears to be a deep-rooted prejudice against any other than a constitutional treatment, and the pertinacity with which they adhere to it is something marvellous. Local treatment is the exception and not the rule; and whilst in most other local diseases local remedies are employed, in the early stages of this they are rarely entertained. Surely the facilities for applying local remedies to the urethra is not less than to any other inlet or outlet of the body. The specific inflammation, as described by Hunter, does not extend beyond four inches from the meatus, and therefore can be easily reached. It cannot be from any obstacle of this kind that local remedies are so frequently neglected: we must look for some other cause, and I think it will be found in the groundless fear entertained that injections either induce orchitis or lead the way to stricture. Now this I am propared to deny, and my experience leads me to the conclusion that a judicious and well-timed employment of injections cannot fail to lessen the risk of such complications. Orchitis in the earlier stages of gonorrhœa is rarely met with—not until a week or ten days have elapsed, and when the inflammation has crept surreptitiously along the passage—when, in fact, it has passed beyond its specific boundary. The same reasoning applies to stricture, with this exception, that the former is the result of acute, the latter of chronic inflammation. Inflammatory stricture I have never seen induced by injections. Such being granted, it is not unreasonable to infer that the sooner the specific inflammation is subdued the better, and that by so doing the chances of such occurrences are lessened."

For many years, both in hospital and private practice, Mr. Childs has acted on this principle, and his experience at the Metropolitan Free Hospital has afforded him ample opportunity of estimating the relative value of the two treatments. With some few exceptions, he has trusted *entirely* to injections, and has rarely known them to fail. Chloride of zinc, perchloride of iron, and the liquor hydrargyri pernitratis have been employed with the best results. Of this latter Mr. Childs remarks he can speak with the most unqualified praise. For the last six months he has employed it with the happiest results. In some instances six injections have been sufficient to affect a cure, whilst in other cases the treatment has rarely exceeded ten days. In no case has copaiba or cubebs been administered; and, with the exception of a saline aperient at first, reliance has been placed solely on the liquor hydrargyri pernitratis. It would be easy to support this by corroborative testimony in the shape of cases from notes made at the time, but the character of sameness about them would destroy their interest. Suffice it to say that notes have been made, and the results are so satisfactory that Mr. Childs has been induced to draw the attention of the profession to this valuable agent in the treatment of ordinary cases of gonorrhœa. It may be well to state that the strength of the injection employed is half a minim of the solution

of pernitrate of mercury to an ounce of water; to be used three times a-day. —*Lancet, Oct.* 12, 1861, *p.* 350.

80.—*Subnitrate of Bismuth as an Injection in Acute Gonorrhœa.*—Dr. MOUSLON, a French Military Surgeon, as the result of numerous trials, confirms the good account given by M. Caby of the efficacy of this treatment. He mixes twenty parts of well washed bismuth—if not well washed the acid which remains may give rise to irritation—in 200 parts of distilled water, causing as much to be taken up as possible. Some of this is thrown into and retained for ten minutes in the urethra, a local emollient bath being first employed. In only the severest cases is the patient obliged to maintain absolute rest for four or five days, after which he is enabled to return to his ordinary habits and diet. The cure, indeed, takes place more quickly under the use of this agent than that of any other. In chronic gonorrhœa success is less marked, and astringent injections may afterwards be required; while in confirmed and obstinate gleet, occurring in a broken constitution, the bismuth is of no use. M. Mouslon appends to his communication an account of a mode of treating painful chordee which he has found very successful. It consists in desiring the patient to sleep on the belly, having first fixed the penis by means of a piece of linen in the fold of the left or right groin.—*Med. Times and Gazette, June* 8, 1861, *p.* 607.

81.—*Gonorrhœa.* By Dr. T. K. CHAMBERS, St. Mary's Hospital.—Gonorrhœa is naturally, in both male and female, a most mild disease, with power to get well in about a fortnight by the simplest treatment, if only it is not made severe by the folly of the patient or his medical attendant. I consider all primary heroic treatment of urethral discharges a most unjustifiable interference with nature.—*Lancet, June* 15, 1861, *p.* 582.

82.—ON THE CURE OF GONORRHŒA BY BLISTERS.

By H. CHALMERS MILES, Esq., Assistant-Surgeon Royal Artillery, Halifax, Nova Scotia.

There is nothing novel (says Mr. Miles) in the principle of the treatment I am about to support, whatever there may be in its method and application. Nine years since, "blistering in gleet" was advocated by a gentleman who, in 1853, wrote with still further experience on the subject; but Mr. Milton appears to have "revived or introduced this practice" more with a view to the treatment of *refractory* cases of gonorrhœa or gleet than to recent and ordinary examples of the complaint, for he says, writing in 1851, "Not merely will a single blister frequently cure the most prolonged gleet, not merely will it rapidly sweep away all dregs of the disease in its ordinary course, but it will often cure those runnings which have resisted all known and usual methods." And again, in 1853 ("On the Cure of Gleet and Obstinate Gonorrhœa"), "If only pure gleet, sometimes mucous, sometimes purulent, be met with, it will in almost every case yield to a blister." And in 1854 ("On Inveterate Gonorrhœa and Gleet") he says, "My experience in these cases was, that not one in fifty would be cured by ordinary remedies; extraordinary remedies—those called for solely in inveterate cases—were blisters, &c." Whilst in 1855 he adds, "I now, on the slightest sign of persistence (of gleet), proceed to use blisters."

On my taking charge of the batteries of Artillery in this country, I determined to give the plan as ample a trial as my practice permitted, and it is because of the conviction which I entertain that the treatment by blisters is, far above all other, the best suited to soldiers—is the speediest in its result—the most effectual in its cure, and is that which is least open to tampering and deceit, that I now venture to submit some observations on the manner in which

it is applied—on the cases to which it is adapted, and on the period within which a complete cure may be anticipated.

[It must be borne in mind that the great proportion of cases of gonorrhœa which are presented for treatment amongst soldiers are of recent origin, as the men are obliged to go to the surgeon as soon as the disease appears. Supposing the running has been noticed about four days, the plan of treatment usually pursued by Mr. Miles is the following:—]

A blister, six inches by four, to be placed very high up towards the anterior and inner aspect of each thigh, is ordered. The blisters are made on adhesive plaster of the ordinary blister of the London Pharmacopœia, the plaster being spread rather thickly, and the surface afterwards sprinkled with acetate of cantharides, a wide margin being left on each side of the blister. They are safely confined in their place by a broad strip of adhesive plaster at each end. For convenience the blisters are generally directed to be applied at night, and no harm has ever come of it, and usually they are well risen by the morning. The patient is then ordered to take every four hours an ounce of the following mixture:—Sulphate of magnesia, two ounces; carbonate of magnesia, four drachms; potassio-tartrate of antimony, two grains; tincture of hyoscyamus, two drachms; peppermint water to sixteen ounces. He is placed on spoon diet, with rice pudding for dinner, and a pint of imperial drink should he be thirsty. During the day he is directed to inject now and then a syringeful of cold or lukewarm water, according to the temperature and season of the year. The blistered surfaces to be dressed with lint dipped in castor oil (a favourite dressing with the men).

The next point for notice is the *immediate* or proximate effect of the blisters on the discharge. On this head you may hear opposite statements, as in some instances the discharge is increased in quantity, and there is more scalding in passing urine, and in others the discharge is rather thicker, but diminished in quantity. Occasionally it happens (see a case narrated below) that the discharge disappears altogether. The most usual result of the application of blisters is rather to aggravate the symptoms for the ensuing twenty-four hours, the discharge becoming more copious, and the pain in micturition being slightly augmented.

The only thing required, however, is a little patience combined with perfect rest, and these symptoms rapidly subside. The saline purgative is to be continued, and the patient is ordered half an ounce of gum arabic to be mixed with water, and used as a drink whenever required, the imperial drink being continued if asked for.

On the third morning after the application of the blisters, the discharge is noticed to be much diminished in quantity, the scalding in micturition is less, and the patient feels better. On the fourth day scarcely any discharge, and there is now no scalding. The blistered surfaces look raw, but are beginning to "skin," as the men say. The saline purgative to be now given only three times a day. The next day perhaps there is a little running when the penis is squeezed, but no other symptom. His bowels are well open. To take the saline purgative (two ounces) every morning, and an injection of nitrate of silver (six grains to the ounce) to be used at night. Sixth day: Blistered places "skinned," though not firmly, and the surfaces are not sore. No sign of running is visible to-day, but there is an appearance as if of weeping at the lips of the urethra. Seventh day: No sign of discharge; the thighs have completely cicatrized, and the surface where they were blistered is not the least tender. The man is discharged for duty, with one day's convalescent leave. This, though a supposititious case, illustrates the sort of treatment adopted.

But a more favourable case than the above will happen. A lad may come the morning after the discharge appears, and in the early stage of the complaint very mild inflammatory symptoms are present. He is of temperate habits. In such a case all that may be required are, rest and low diet, the saline purgative mixture, and as a local remedy a small blister applied to the under surface of the penis. The blister should be about an inch wide, and from an inch to an inch and a half long, and be carefully fastened on by oiled

threads. It is well to fasten a suspensory bandage with some extra linen covering over the scrotum, so as to prevent the chance of vesicating it. If the blister rises properly, in a simple case of gonorrhœa taken thus early, I have had repeated instances of immediate cure by a single application, the discharge having completely ceased, with no recurrence at a subsequent period. In such a case the blistered surface has healed (it is surprising the rapidity with which it does so on this spot), and the man has been discharged for duty on the fourth morning after admission.

In the small proportion of cases in which a relapse occurs (it is generally due to the fact of soldiers who are discharged from the hospital being permitted to go "on pass" in the town, where of course they commit a debauch, and soon require re-admission) I have generally found it yield to the second application of blisters, combined, it may be, with the use of an injection or two of nitrate of silver, and aided by frequent doses of the saline purgative, which is to be given in every instance. It has rarely happened to me to find the first application of blisters inefficient; but when this has been the case, should the thighs have been blistered in the first instance, a blister to the under surface of the penis (of the size already mentioned) has completely checked the discharge; or should the penis have been first blistered, the application of the vesicating plasters to the thighs proved of unfailing efficacy.

Now in illustrating the system of treatment by blistering it will be observed that there are three regions open to choice; 1st, the upper and inner part of the thighs; 2nd, the under surface of the penis; 3rd, the perineum. I have, however, avoided reference to the latter region, as I consider the locality inadmissible in many soldiers by reason of the following objections. If the man's duties require him to be mounted, I think greater tenderness is left after blistering in this region than in the others. Should small boils appear after the application of blisters, they are unusually troublesome; and it is in this part that the "fork" of the trowsers, if badly made, is apt to cut. In addition to these reasons, as the perineum is always subjected to considerable pressure in mounted duties, should tenderness exist, the man is rendered "ineffective" for parade, and possibly remains so for a considerable time. It will be evident, therefore, that counter-irritation in the shape of blisters applied to the perineum is not suitable in such instances.

To none of these drawbacks, however, is either of the other positions open. I know of no objection to placing blisters on the thighs. Some might suppose that the tendency which often exists to the *appearance of a crop of boils* on the seat of the blister would cause annoyance; but the following treatment will prevent this:—Should some inflamed pustules appear about the roots of the hairs on the new surface, they should be pricked at once and gently squeezed, and a soft linseed-meal poultice applied; if the surface is then well rubbed over with castor oil, there will seldom be any further trouble in the matter. *Tenderness of the skin* on the new surface is removed by painting the part well over with a wash of caustic solution (ten grains to the ounce), by means of a camel's-hair brush. This application hardens the surface, and appears to prevent the cuticle from cracking, &c., on the man's returning to work and out-door occupation.

Is the cure by blisters generally effectual?—According to such experience as I have had I would answer that it is both more speedy and effectual than any I know of.

To what cases is it suited?—Nearly all the cases presented amongst soldiers are examples of recent infection, and the only complication noted is "swelled testicle," in which instance the treatment is just as suitable as in any other. While in the few cases of long-standing gonorrhœa treated, the complaint has yielded to blisters with great readiness.

What accessory treatment is required?—Saline antimonial purgatives at frequent intervals, rest, and low diet, and perhaps—as the blistered surfaces are healing—an injection or two (rarely more) of caustic wash (six grains to the ounce).

What is the period required for cure?—In the event of it being thought necessary to blister the thighs, the soldier is usually returned on the "effec-

tive list" on the seventh day from the date of his admission. If the surface of the penis is blistered, and a second application be not required, the man is discharged for duty on the fourth day. Should the case be unusually obstinate, requiring blisters to both thighs and penis, and a recurrence of the application in one or both instances, perhaps he may be in hospital for fourteen days; but this would be rare.

Have relapses often occurred?—Seldom indeed; and then only after a debauch, or the committal of some imprudence on the man's part.

Has the relapse in such cases been tedious?—In no instance has the re-appearance of the discharge failed to yield to blisters.

What objection can be taken to the plan of treatment?—I know of none in the case of either soldiers or sailors, or other persons employed in the departments attached to the services. The cure I believe to be speedy and effectual. So far as Government is concerned it is economic; and, as regards the patient, it is less painful and annoying than might be anticipated. It cannot surely be very repugnant to the patient's feelings, as several instances have occurred of men employed in the civil departments, and entitled to military medical attendance, coming to me, on getting "clap" with the request to be treated by blisters: "I would rather have *them* blisters, sir, and be cured at once."

Look at the tons of copaiba which must be swallowed by the million! Mr. Weeden Cooke says "that he found upon inquiry at the London Custom House that 118,396 lb. were admitted into the port of London during the first ten months of 1859." We have here, says he, "copaiba enough to treat 473,584, or close upon half a million persons!" Again, this author asks, "Is it not extraordinary that this disgusting medicine continues so long to hold its ground?" And let us reflect on the many other objectionable medicines which nauseate the seething mass of the population afflicted with gonorrhœa, and such like complaints. How far the treatment which I advocate may be applicable to private practice I leave others to judge. Such a question is foreign to my argument, which is limited to cases in the public service.

I beg to add a brief abstract of the foregoing remarks:—

1. That an ordinary gonorrhœa of recent date may generally be cured by the application of a single blister to the penis, in a period of less than four days. Conjointly with the blister, saline purgatives with small doses of the potassio-tartrate of antimony (*vide supra*) should be given at frequent intervals, and rest and low diet rigidly enforced.

2. That in acute gonorrhœa attended with considerable constitutional irritation, occurring in an unfavourable subject, blisters to the thighs, with the concomitants just mentioned, will rarely fail of success; and should the discharge not be checked entirely on the third day after their application, a few injections of solution of nitrate of silver (six grains to the ounce) will usually complete the cure by the seventh day.

3. That in cases of gonorrhœa of several weeks' standing, in which other remedies have been used and without avail, the application of blisters is attended with immediate success.

4. That old obstinate cases of gleet and gonorrhœa occurring in intemperate subjects, and which have resisted ordinary remedies seldom fail in being completely relieved by blisters; and in instances where the first application is insufficient, a second set of blisters will prove completely effectual.

5. That relapses are infrequent; but that on their occurrence the application of blisters has in no instance failed to effect a cure.

6. That with the exception of saline purgative medicine and the occasional injection of the caustic wash—should slight running continue after blisters have been applied—no other treatment is needed.

7. That the complication of "swelled testicle" in no degree invalidates the success of the treatment by blisters.

8. That no unpleasant symptoms have been noticed after the application of blisters. A crop of small boils occasionally appears around the roots of hairs on the blistered surface; but this is generally no source of annoyance

if treated in the manner mentioned above; and should tenderness remain on the "new skin," the surface will rapidly become "tough" if painted with a solution of caustic (ten grains to the ounce).

9. That when the above circumstances are duly considered—the brief time required for treatment; the small annoyance caused to the patient; and the entire freedom from the administration of all nauseous and offensive medicines; with the completeness of the cure generally effected,—it is thought that the treatment of gonorrhœa by blisters will be acknowledged to offer the means best adapted for use in the instance of men in our armies and fleets.—*Lancet, June* 8, *and* 22, 1861, *pp.* 558, 605.

83.—*Chlorate of Potash in Gonorrhœa.* Dr. IRVIN, United States Army, stationed at Fort Buchanan, says—

"It may not be amiss to place on record my testimony in favour of the use of chlorate of potash as a therapeutic agent, which I have constantly used during the last two years in the treatment of gonorrhœa, both in my public and private practice, with the most gratifying results. I have found it to be such an admirable remedy that I seldom resort to any other in the treatment of urethral inflammation. My method of using it is as follows:—One drachm of the salt dissolved in eight ounces of water, of which an injection is given every hour for twelve hours, at the end of which the discharge is become changed and diminished, allowing the remedy to be gradually discontinued until the second or third day, when the disease will be generally found to have ceased. So efficacious has this remedy proved in my practice, that I seldom deem it necessary to give any other medicine, save a Seidlitz draught or a dose of Epsom salts."—*Medical and Surgical Reporter, and Glasgow Medical Journal, July* 1861, *p.* 255.

84.—HOW IS SYPHILITIC TO BE DISTINGUISHED FROM NON-SYPHILITIC PSORIASIS.

By Dr. T. M'CALL ANDERSON, Physician to the Dispensary for Skin Diseases, Glasgow.

Diagnosis of Syphilitic from Non-Syphilitic Psoriasis.

SYPHILITIC PSORIASIS.	NON-SYPHILITIC PSORIASIS.
1. Eruption *not usually* very extensive.	1. Eruption *often* very extensive.
2. Patches usually very small, and in shape of spots (size of a split pea), or of *small* circles or segments of circles (seldom more than an inch in diameter).	2. Patches *often* very large and irregular. When circular, circles often two or three inches in diameter.
3. Eruption not usually on the elbows and knees; more on the inner than the outer aspect of limbs. When limited to soles or palms, usually syphilitic.	3. Eruption on any part of the body, but *almost* invariably on the *elbows and knees also.*
4. Eruption usually of a distinctly coppery tint, after it has become chronic; sometimes very dark, even nearly black *(psoriasis nigricans.)*	4. Patches of a dusky-red or light coppery colour.
5. Scales thin; not so imbricated; often greyish.	5. Scales thick, imbricated, white, and silvery.
6. May last months, or even one or two years, when no treatment employed.	6. Often of five, ten, fifteen, twenty, or thirty years' duration, or even almost a whole lifetime.
7. Almost always commences after puberty, and usually after 20.	7. Most cases (not all) commence before the age of 20.

8. Rarely if ever itchy.	8. Sometimes not itchy; sometimes intolerably so; generally *slightly* itchy *now and then*.
9. A relapse not usual after *all trace* of the first eruption has *completely* disappeared.	9. Relapses are the rule, and are often very numerous.
10. Can often be traced to a hard chancre.	10. Can often be traced back to hereditary transmission.
11. Patient *may* be cachetic, and concomitant symptoms detected; *e.g.* roseola syphilitica, lichen syphiliticus, condylomata, sore throat, alopecia, &c.	11. Patient in robust general health; bad health incompatible with the eruption, and the eruption is antagonistic to scrofula.
12. Removed almost invariably by mercury.	12. In the majority of cases, removed partially or entirely by arsenic.

[Dr. Anderson gives the two following cases illustrating the effects of remedies in these forms of disease.]

The Effects of Remedies.—It must be known to all that mercury is a never-failing remedy for the removal of syphilitic psoriasis, an example of which is furnished by the case of Mrs. Davidson, aged 39, who consulted me on April 3, 1861, (the minutiæ of the case I am unable to give, as I write from memory, not having taken notes of it at the time), with syphilitic psoriasis in the shape of very distinct little coppery circles, covered by white scales. None of these circles were more than an inch in diameter, and were situated principally on the extremities. After twelve calomel vapour baths, no trace of the eruption was left, except in the shape of very faint red circles—all the scales, the coppery tint, and the elevation above the surface having disappeared. She was told to continue the baths, and as she never returned there can be little doubt that she was completely cured. The vapour bath was extemporized according to the suggestion of Mr. Henry Lee. She was told to heat to redness half of a common brick, to place this in a common pan, the bottom of which contained a little boiling water. She was then to place the calomel powder in a thin layer (20 grains) upon the surface of the brick which was not in the water, to put the pan beneath a cane-bottomed chair, and to sit on the chair, enveloped in a blanket, which was drawn *tight* about the neck—to prevent the mercurial vapour getting into the mouth, causing sickness and salivation—but covering the body *loosely*, so as to allow the vapour to have free access to and to act upon the skin. The hot brick has the effect of keeping the water boiling, at the same time as it volatilizes the calomel.

Arsenic, on the other hand, is the remedy *par excellence* in the treatment of the non-specific form of the disease, and to which a large proportion of the cases yield. As an illustration of this may be taken the case of William Martin, which occurred at the Dispensary for Skin Diseases, under the care of Dr. Buchanan. This patient, a blacksmith, aged 25, was admitted March 5, 1861, with psoriasis covering almost the whole body, though leaving intervals of sound skin between the patches. It was the most extensive eruption of the disease I ever saw, and had existed more or less for five months. The joints were uneasy and painful, owing to the deep fissures which existed, and the hands were quite covered with the eruption, and so stiff and painful that the patient was unable to open the dispensary door on the day of his admission. He was ordered five drops of Fowler's solution thrice daily, and the dose was gradually increased up to fifteen drops. The exhibition of this dose soon occasioned slight pain in the epigastrium, and the conjunctivæ became suffused, so that it was diminished to six drops three times daily. On the 19th April there was merely redness of the skin left in the sites of the previous patches, and on the 26th almost all trace of the disease had disappeared.—*Glasgow Medical Journal, July* 1861, *p.* 160.

DISEASES OF THE EYE AND EAR.

85.—ON THE RELIEF OF SYMBLEPHARON BY TRANSPLANTATION OF CONJUNCTIVA.

By T. Pridgin Teale, Jun. Esq., M. A. Oxon., &c.

[Mr. Pridgin Teale gives us the following mode of operating in cases where adhesions of the eyelids to some contiguous parts interfere considerably with vision. It has generally satisfied surgeons to divide the bands or adhesions, as they would the cicatrices from burns, and keep the parts asunder as much as possible, till healed; but the subsequent cicatrix often makes the case as bad as before. Mr. Teale, therefore, very cleverly adopts the same principle with respect to the eyelid as is applied to cicatrices of the skin, in the following way:]

Mode of operating.—Having first made an incision through the adherent lid, in a line corresponding to the *margin* of the concealed cornea, I dissected the lid from the eye-ball until the globe moved as freely as if there had been no unnatural adhesions. Thus the apex of the symblepharon being part of the skin of the lid, was left adherent to the cornea.

In the next place two flaps of conjunctiva were formed, one from the surface of the globe near the inner extremity of the raw surface, the other from the surface of the globe near its outer extremity. I first marked out with a Beer's knife a flap of conjunctiva, nearly a quarter of an inch in breadth, and two-thirds of an inch in length, with its base at the sound conjunctiva, bounding the inner extremity of the exposed raw surface, and its apex passing towards the upper surface of the eye-ball. The flap was then carefully dissected from the globe until it was so far at liberty as to stretch across the chasm without great tension, care being taken to leave a sufficient thickness of tissue near its base. A second flap was then made on the outside of the eye-ball in the same manner. In making the flaps, conjunctiva alone was taken, the subconjunctival fascia not being included. The two flaps thus made were then adjusted in their new situation. The inner flap was made to stretch across the raw surface of the eyelid, being fixed by its apex to the healthy conjunctiva at the outer edge of the wound. The outer flap was fixed across the raw surface of the eye-ball, its apex being stitched to the conjunctiva, near the base of the inner flap. Thus the two flaps were dovetailed into the wound. The flaps having been adjusted in their new position, the vitality was further provided for by incising the conjunctiva near their base, in any direction in which there seemed to be an undue tension, and by stitching together the margins of the gap whence the transplanted conjunctiva had been taken. One or two other sutures were inserted, with a view to prevent doubling in of the edges of the transplanted conjunctiva.

[Four cases are then related in which the operation was performed, and from which Mr. Teale draws the following conclusions:]

1. That conjunctiva may be transplanted without losing its vitality or properties.

2. That a symblepharon may be replaced by loose moveable conjunctiva, at least equal in breadth to the flaps originally transplanted.

3. That a comparatively small breadth of conjunctiva introduced into the situation of a symblepharon is sufficient to afford greatly increased, if not perfect, freedom of motion to the eye-ball, a freedom which continues to increase for many months after the operation.

4. That where the conjunctiva of the lateral and upper parts of the eye-ball has not been damaged, flaps of a quarter of an inch in breadth may be taken away without giving rise to any deterioration of the parts whence they have been taken.

5. That in separating the adherent eyelid from the globe, it is not necessary to dissect off from the cornea any portion of skin that may be adherent to it, but that it is better to commence the separation of the lid at the margin

of the cornea, leaving the opaque apex of the symblepharon adherent to the cornea.

My object in commencing the separation of the lid at the margin of the cornea, thus leaving an opaque island of skin on the cornea, was two-fold. In the first place, it seemed more than probable that, if the corneal surface had been denuded of its adherent skin, any conjunctiva transplanted into the gap would be drawn by cicatrization over the newly-made raw surface of cornea, as the skin of the eyelid had been in the first instance. The conjunctival flap would thus have been rendered useless, and the opaque skin would have been replaced by not very transparent conjunctiva. In the second place, I had a faint hope that the isolated portion of skin, being cut off from all continuity with skin, and depending for its nutrition on the cornea supporting it, and a merely *cicatricial* union with conjunctiva, might become atrophied and less opaque. The result has far exceeded my expectations. In the case of Jessop, the trace of this skin can only be detected in a good light. In two other cases the situation of former frœna can only be seen on minute inspection. In the case of Spence, operated on last March, the edges of the opacity have become transparent, and the remaining portion is atrophied, and but little raised above the level of the surrounding cornea.—*Ophthalmic Hospital Reports, Oct.* 1861, *p.* 254.

86.—*Presbyopia.*—By Mr. J. Soelberg Wells.—A very rapid increase of presbyopia is one of the premonitory symptoms of glaucoma. If, therefore, a patient tells us that his far-sightedness has rapidly increased within a few months, so that he has had repeatedly to change his spectacles during that time for stronger and stronger ones, our suspicions should be aroused, and we should without fail examine him as to the presence of other premonitory symptoms of glaucoma—*e. g.*, rainbows around the candle, periodical obscurations, &c. Von Graefe thinks that this rapid increase of presbyopia is most likely due to an increase of intraocular pressure and flattening of the cornea.—*Med. Times and Gazette, June* 1, 1861, *p.* 575.

87.—*Black Eye.*—A writer in the 'Medical Times and Gazette' says:—"A correspondent" asks the communication of an effective remedy for ecchymosis of the eye in a lady patient who had met with an accident. One which I have used for a great many years, never disappointing me, usually getting rid of the blackness in a few hours, is the root of the "Convallaria," "Solomon's Seal." It should be washed, the dark-coloured cuticle carefully cut off, then scraped like horse-radish, applied direct to the eye in the way of a poultice, cold. A tingling sensation is the consequence; when this sensation ceases another fresh application should be made, and repeated until the whole ecchymosis is absorbed. I have often found it sufficient to apply the scraped root at bedtime to the closed eye, when the blackness has disappeared by the morning. The convallaria is readily found, and is equally effectual at this season of the year.—*Dublin Hospital Gazette, June* 1, 1861, *p.* 175.

88.—ON CATHETERISM OF THE EUSTACHIAN TUBE, AND ON THE PHARYNGOSCOPE.

By Dr. Voltolini, District Physician in Breslau.

Dr. Semeleder, of Vienna, has published in the Austrian Journal of Practical Medicine an interesting essay on the above subject; but he therein attributes to special aurists a peculiar dexterity in catheterising, so that it might be supposed that the employment of the pharyngoscope, at least for the purpose of controlling the situation of the catheter, might not be so necessary to such practitioners. It will therefore not be superfluous for me, as a practical aurist, to write something on the catheterism of the Eus-

tachian tube, especially as I do not only daily practise this operation, but also almost daily employ the pharyngoscope; indeed, I think it even necessary to write something upon catheterism, because, in consequence of the introduction of the pharyngoscope, that operation, and with it aural surgery, have assumed a new phase. Pharyngoscopy first reveals to us all the mistakes hitherto made, and still daily made in catheterising, and of which, to a certain extent, the operator could have no suspicion. Even our anatomical knowledge of that region of the body is enlarged, as we can now every moment by inspection study the parts in the living state, whereas we cannot every moment have a dead body to examine. The previous want of anatomical knowledge is evidenced in the description of catheterism. Thus, for example, the beginning of the description of this operation by Kramer is incorrect, although undoubtedly Kramer contributed largely to the improvement of the operation. He says: "If the catheter is introduced into the nose so far that its point touches the posterior wall of the pharynx (whereby the outer visible ring, consequently also the concavity of the beak, is still directed downwards) we raise the posterior end of the catheter, the beak sinks and glides, while we carefully draw the catheter to ourselves, over the posterior roundish projection of the mouth of the Eustachian tube, &c." Now this is incorrect: if I guide the catheter, with the beak directed downwards, straight through the nose to the posterior wall of the pharynx, and then draw it back in the same direction, I cannot pass the projection of the opening of the tube, and it is quite indifferent whether I depress or raise the beak of the catheter; if the catheter remains in the same direction, that is, with the beak downwards, it passes freely through the naso-pharyngeal cavity to the posterior wall of the pharynx, and as freely again back to the velum palati. But if the catheter in this course gets into a depression, as perhaps into the opening of the tube, we have to do with a pathological condition. As I write this, I repeat the experiment on myself, and with the pharyngoscope upon a patient; I now feel and see the catheter passing quite freely through the naso-pharyngeal cavity; it is not until the beak of the instrument is turned outwards, that it touches the orifice of the Eustachian tube—and sometimes not even then, if a catheter with a long beak be not employed. This relation we could certainly without the pharyngoscope demonstrate in the dead body, but there are other circumstances respecting which we could not in every case without this instrument obtain information. Incidentally to the term "pharyngoscopy," I may observe that I shall enter more fully into this subject in a paper which is about to appear in Virchow's *Archiv;* here I shall confine myself to it so far as it relates to aural surgery, having nothing to do with the inspection of the nose (Rhinoscopy).

As to the catheterism itself, I speak only of the method through the inferior nasal passage—other methods depend in part on incorrect anatomical views, or are quite unsuitable and much more complicated than this. In catheterising through the middle nasal passage, we arrive over the mouth of the tube, as unfortunately more frequently happens in attempting the operation through the inferior passage; but neither does it then succeed, and the catheter must be again taken out, and introduced into the inferior passage. I have not yet had occasion to catheterise through the mouth, and this method would be applicable only to those cases where the nose is rendered impermeable by abnormal formations.

In catheterising through the inferior nasal passage, there is only one fixed rule, namely, to keep with the catheter on the floor of the nasal cavity; all other rules are liable to numerous exceptions. In general the operator passes through the nasal passage with the beak of the catheter directed downwards, and towards the septum. But cases occur where it must be directed upwards both in entering and passing through the nose; an instance of which is at present under my care. In general, if any resistance is met with, the catheter is made to rotate gently between the fingers, and, so to speak, to find the way for itself—only no force should be employed; this is the first immutable condition, just as in catheterism of the urinary bladder

(unless one has a special passion for forcible catheterism.) In like manner, there are cases where the floor of the nasal cavity passes obliquely outwards and downwards, and is, as it were, hollowed out; in this case the catheter must be kept far from the septum. If in difficult cases we cannot, by twisting and turning the catheter in various directions, pass through the nose, a very slight catheter must be taken, and after having ascertained with it how the obstruction is to be avoided, we must then try a stronger one.

Having passed the catheter through the nose, the rule must be now to turn the beak of the instrument downwards, if it has not already in its transit been held in this direction; this usually easily succeeds, because the operator has sufficient room in the naso-pharyngeal cavity, that is, if the beak be first in the cavity turned to the other nostril, and then turned downwards; generally all this is not necessary, but the catheter is passed immediately with the beak directed downwards, straight through the nose to the posterior wall of the pharynx. If the latter has in either mode been reached, two ways are open of bringing the catheter into the mouth of the Eustachian tube. Either its beak is turned immediately outwards on the posterior wall of the pharynx, whereby the instrument is brought into Rosenmüller's cavity, therefore behind the projection of the tube, and is now drawn gently feeling our way, straight forwards over the prominence of the orifice; and so soon as it is felt that it has glided over the latter, it is gently pressed into the mouth of the tube; or the beak of the catheter is drawn, directed downwards, from the posterior wall of the pharynx directly forwards back to the velum palati (in so doing it is well to press the beak with somewhat greater force still more downwards, in order to be sure of hitting on the velum, and not by more gentle manipulation to run the risk of returning into the nose); having arrived there, the beak is turned outwards and somewhat upwards, and pushed a *little* forwards, whereby it passes into the mouth of the Eustachian tube. The latter operation is often still further facilitated by causing the patient to make the movement of swallowing in the moment when the quarter-turn is made, and the catheter is thus usually involuntarily thrown into the mouth of the tube.

What I have hitherto said respecting catheterism is calculated to be of use only to those who are little practised in the operation; for the aurist in full practice, these are matters of daily experience. But I shall now state some circumstances first discovered by the pharyngoscope, and which are of importance also to the aurist. Thus, if we inquire what proof we have that the catheter is actually in the tube the one aurist will say, "I feel the catheter gliding over the prominence of the tube;" the other will answer, "I hear the air, when I blow into the catheter, penetrating through the tube into the cavity of the tympanum." It would be all right if I feel that and hear this; but how if I neither feel that nor hear this? then I have no certain guide but the pharyngoscope; and these cases are not so rare. For example, if we have to do with a case of obstruction of the tube, we cannot hear the air penetrate, and if the projection of the tube be at the same time not perceptible, in consequence of its being too small or lying too much externally, this ground of support also fails. But now there occurs, even frequently, a still more remarkable case, namely, on blowing through the catheter, we hear the air very distinctly penetrating through the tube into the cavity of the tympanum, and still the catheter is not in the tube. In such instances we see, through the pharyngoscope, the beak of the catheter not in the mouth of the tube; but at a greater or less distance from it, in a direct line inwards, the mouth of the catheter looks, as it were, from afar into the orifice of the tube. If I now blow strongly into the catheter, the current of air passes of course directly out into the mouth of the tube, through this into the cavity of the tympanum; and on auscultation, I shall hear the air as well as if the catheter were actually in the tube. I may thus attain even great therapeutic results by removing mucus, &c., and so making the tube permeable, I may be still further confirmed in my opinion that the catheter is really in the tube. But the matter is quite different, if in such a case I attempt to introduce vapours or a catgut into the tympanum. The vapours then extend around the mouth of

the tube without reaching the cavity of the tympanum, or at most they arrive there in very small quantity, because they cannot be constantly driven in with such force as a powerful lung exerts in driving the air in jerks through the catheter. Matters are still more unfavourable to the introduction of a catgut; if the catheter be not well fixed in the tube, the catgut in general bends when it comes out of the mouth of the catheter, and passes down into the pharynx—I shall not venture to say where a probe or pointed instrument introduced through the catheter would reach to. But in those cases where neither the prominence of the mouth of the tube is felt, nor the air is heard to press through the tube, no particular mischief will be done if merely air or a drop of medicine be blown in—they will simply not penetrate the tube. But if I suppose that the catheter is actually in the tube, because I have perhaps made the regular turns with it, and if I wish forcibly to remove the supposed obstruction of the tube, I might cause great mischief, nay, even the death of the patient. It is well known that the dreadful misfortune happened to a practitioner in London, that not one, but two patients fell dead, in consequence of the incautious use of the compression pump, so that he was of course brought into court about it. But the pharyngoscope reveals still other positions of the catheter; we may more frequently observe that its mouth is placed at one side of the orifice of the tube, that is, it will be either partly within the latter, or may be so placed upon a margin of the prominence as to look partly, perhaps half, into the orifice; in these cases, too, on strong insufflation, the air is heard to pass through the tube, while for other operations the above described obstructions exist.

Dr. von Troeltsch, in his valuable work on the anatomy of the ear, has shown, that in children the pharyngeal mouth of the tube projects less into the pharynx; and that the slender lips of the simply split-like opening usually lie so near one another, that in the dead body it is often difficult to discover them in the swollen mucous membrane of the throat. This is a very important circumstance; for I have repeatedly in patients of from twelve to sixteen years of age found the parts still in the condition they present in infancy (and probably the same state sometimes occurs also at a later period of life), that is, an arrest of development, where the mouth of the tube still presented that lip-like split. In such cases it is a chance if the catheter will ever be brought into the tube, for the soft lip-like fissure cannot be felt. In this instance it is not enough to inspect the parts with the pharyngoscope, but the catheter must be introduced with the assistance of the latter, that is, it must be introduced during examination with the pharyngoscope. In this I am already so practised, that with one hand I can manipulate my spatula-speculum, while with the other I introduce the catheter. In another paper I shall shortly describe my mode of proceeding, and also communicate some cases where this arrest of development of the tubular opening occurred, and which were successfully operated on. Meantime I shall here mention only that raising the uvula in pharyngoscopy is a point I have long since got over, that is, I no longer trouble myself about raising the uvula in any way; but with my little spatula-specula, as prepared according to my directions by Hauck, of Vienna, I almost daily inspect the tube, without at all displacing the uvula. The new venturesome instruments recently brought out for raising the uvula are not only unpractical, but superfluous. It is not the uvula, but the contraction of the soft palate, which renders pharyngoscopy or rhinoscopy difficult; therefore it is of no use to raise the uvula, all above remains as dark as before; and, on the other hand, if the contraction of the palate has been overcome, if the pharyngo-nasal cavity be free, it is again not necessary to raise the uvula, the speculum can be very well used without that. There are very rare cases in which, even when the soft palate is relaxed, it is necessary to raise the uvula, in order to see very freely into the cavity. This is the case where the uvula is very broad and long, so that we cannot even see through the two arches of the palate, or if we should wish to look very deeply into the posterior nares. There are noses where, if the clear sunlight be allowed to fall into them, a very thick catheter having been at the same time introduced, we can see di-

rectly through them to the posterior nasal wall, and follow with the eye the movements of the catheter in the naso-pharyngeal cavity. In such instances, therefore, no speculum nor instrument for raising the uvula is necessary to enable us to inspect the nose.

There is still another mode of catheterising, in which the pharyngoscope is of essential use, that, namely, where from one nostril we pass the instrument into the tube of the opposite side, if the corresponding nasal cavity is in any way malformed. In such a case we make use of a catheter with a longer beak. Dr. Erhard, of Berlin, doubts the practicability of this operation, and Kramer describes it imperfectly. He says when the posterior wall of the pharynx has been touched by the point of the catheter, turning, the latter on its axis towards the affected ear will, on account of the longer beak, in general succeed only with greater difficulty, and best at the moment when the patient draws in his breath, or makes the motion of swallowing. Now this is, properly speaking, no description at all; for it would be a mere accident, if in such manipulation the catheter entered the tube. I have already, in March, 1860, in the hospital here, and on the second of July in the Association of Medical Science of Berlin, exhibited this operation on myself; the gentlemen who were present at these demonstrations saw very plainly, in the pharyngoscope, the catheter pass from the one nostril into the opposite tube. The operation must be so performed, that the beak of the catheter may pass directly into Rosenmüller's cavity of the opposite side, that is, in the diagonal of the naso-pharyngeal cavity; then, in drawing back the catheter, it must be guided gently over the posterior projection of the tube, and so into the latter, as in ordinary catheterism.

I believe that I have in this paper shown what the defects of the art of catheterism have hitherto been, and that the pharyngoscope can no longer be dispensed with in aural surgery. Pharyngoscopy and rhinoscopy are certainly more difficult than laryngoscopy; they will, however, undoubtedly be more cultivated, and the instruments will be improved. Semeleder's illuminating spectacles are an excellent instrument for all investigations of this kind, particularly for near-sighted people, who can have glasses adapted to them inserted in the frame; these spectacles do not incommode the operator in speaking, as the specula with a mouth-stalk do; which is not unimportant, as it will often be necessary during the operation to give directions to the patient how to hold himself. I regret not having sooner possessed them.

In conclusion, I may mention an anatomical peculiarity of the nose, which I met with in the greater number of skulls in the museum here. The nasal septum is mostly bent to the left side, and the left nostril is consequently narrowed; this observation first explained to me why in most men catheterism is more difficult on the left side than on the right, and why we can scarcely ever introduce so thick a catheter on the left side as on the right.—*Medizinische Jahrbücher; and Dublin Quarterly Journal, August* 1861, *p.* 223.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

89.—ON THE INDUCTION OF PREMATURE LABOUR.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

Professor Simpson said that he had brought on labour prematurely, about four years ago, in a case where he believed that he had met with a new indication for the performance of the operation. Mrs. L., the patient who was the subject of the operation, had fallen in labour of her first child in 1851. Her labour had lasted two days, and was terminated by the head of the child, which was in a hydrocephalic condition, being perforated by Dr. Jamieson of Peterhead. Her second labour, in 1853, was also terminated by means of the same operation, by Dr. Johnston of Stirling, because of the same morbid condition of the fœtal head. In 1856 he (Dr. S.) first saw the patient, in consultation with Dr. Johnston, at a time when she had arrived at the eighth month of her third pregnancy. There was every probability that in this instance again the child would become hydrocephalic, and be destroyed at birth; and the probability was increased by the circumstance that the only sister of the lady in question had given birth to but one child, which had likewise been hydrocephalic, and had been delivered by means of craniotomy. The likelihood of the recurrence of the disease in the third fœtus was still further impressed on his (Dr. Simpson's) mind, by a letter which he received about that time from Dr. Embleton of Embleton, in which that gentleman related how he had just delivered a woman of a child that presented by the breech, and was very difficult of extraction, because of the head being distended to twice the natural size from hydrocephalus. And Dr. Embleton had stated further, that the first and third children of the same patient had been born hydrocephalic, and had been delivered by means of the forceps. Regarding it, therefore, as in the highest degree probable, in the case of Mrs. L., that if her third pregnancy were allowed to run its full course her third child would become the subject of intra-uterine hydrocephalus like its predecessors, he (Dr. S.) had two reasons to lead him to induce premature labour at once; viz., 1st, it was a matter of great importance for the lady to have a *living* child, and there was little prospect of her hopes being fulfilled, if her progeny were all born hydrocephalic; and, 2ndly, it was fairly to be expected, that if the child could be brought into the world at once, and the conditions of its existence changed, it would have a better chance of escaping the dangers of the disease which had proved fatal to the first two children. The result justified his (Dr. S.'s) expectations. Labour was induced, and a living child was born, which he had seen a few weeks ago in the enjoyment of the most perfect health. The method by which labour was induced in that instance, was one which he (Dr. S.) hoped would never again be employed. At that time, the means most commonly adopted for the purpose was the injection of tepid water into the maternal passages, and the manner in which this process acted in bringing on labour was then a frequent subject of discussion. He had become convinced that its *modus operandi* depended on the separation, which it effected, of the membranes from the interior of the uterus, and which he had come to regard as the exciting cause of parturition at the ordinary termination of a normal pregnancy. As the injection of water was attended with this drawback, that the bed and body-clothes of the patient became disagreeably moistened by the fluid that escaped, he determined to try whether the separation of the membranes might not be as effect-

ually accomplished by some less discomforting means; and, accordingly, he attempted to effect his purpose by the injection of atmospheric air, in the case of a patient whom he had seen before in consultation with Dr. Smith of Lasswade, and in whom he had induced premature labour on a former occasion. He introduced the air by means of a catheter passed into the interior of the uterus, between its walls and its contained membranes, on two successive days; and labour supervened within a few hours after the second inflation. In the case of Mrs. L., however, he had used neither water nor atmospheric air, but carbonic acid gas, which had just at that time been proposed as an oxytoxic by Scanzoni. The gas was introduced for the first time by means of a catheter on a Wednesday, and its injection was repeated once each day till the following Sunday. Labour pains supervened on the Saturday, and continued feeble and intermitting till the Monday evening. The os uteri was firm and cartilaginous, and a number of small incisions were made into it, to favour the dilatation. The child was not born till Tuesday morning, or for six days after the first injection of the gas. The case, however, was so far successful. But he (Dr. S.) believed that a similar operation *ought never again to be attempted.* For, having happened soon afterwards to speak of these cases to some of his confreres in another medical school, one of them, who was induced to adopt the operation, had the misfortune to lose his patient. The gentleman in whose hands the accident happened was a distinguished and experienced practitioner, and performed the operation with every care and precaution; yet the patient died in the course of a few minutes. The fatal effect was probably due to the separation of a portion of the placenta, and the entrance of air through the uterine sinuses into the circulation. The case was not solitary, however; for Scanzoni had recorded two fatal cases of the injection of carbonic acid gas into the uterus; and in a case in which the artificial and criminal induction of abortion, at Philadelphia, was attempted by blowing in air into the uterus, the patient suddenly expired. He (Dr. S.) therefore maintained that the injection of air or gas of any kind into the interior of the uterus, for the purpose of inducing premature labour, was an operation which we were not entitled to repeat. He (Dr. S.) had many times had recourse to the injection of tepid water for the induction of labour,—sometimes by means of Kiwisch's douche, which was a clumsy and inconvenient contrivance; more frequently by means simply of Higginson's syringe, the nozzle of which could be introduced within the os uteri. But, besides being attended with discomfort from the wetting of the bed, this operative procedure was open to the further objection, that it sometimes brought about a change in the presentation of the child. Moreover, he (Dr. S.) had been greatly alarmed by seeing a patient on one occasion faint under its use, probably from some of the fluid getting into the circulation.

And in consultation practice he had seen two more alarming cases still, where both the patients died. In both, only a few ounces of water were injected; and yet rupture of the uterus took place. The occurrence of the rupture was to be explained as he (Dr. S.) believed, by the fact, that the uterus being already fully distended, could not admit the few ounces of fluid necessary for effecting the separation of the membranes from its interior without being stretched and fissured to some extent; and during labour these slight fissures might easily be converted into dangerous and fatal ruptures. In one of the cases he referred to, the patient died before labour was completed; the other in twelve hours after its termination. They both occurred in the hands of most accomplished and able accoucheurs; and he (Dr. S.) thought the procedure ought to be entirely set aside, or should be had recourse to only in some exceptional circumstances. Fortunately, we had other and safer means of effecting our object; and of these, perhaps, the best was that now in very common use in Germany, viz., the introduction of a flexible male catheter into the interior of the uterus, between the membranes and the uterine walls, withdrawing the stilet and leaving the catheter there until labour set in. The history of that operative procedure was, shortly, this: Dr. Hamilton used to bring on premature labour by separating the membranes

from the interior of the cervix, to a distance of two or three inches, with his finger or a catheter. The plan was not very effectual, and required always to be repeated very frequently before labour supervened. About eight years ago, he (Dr. S.) had supposed that it might be produced more effectually by leaving a foreign body in the cavity; and chose as the subject of experiment a patient on whom labour had on two previous occasions been successfully induced by means of the water douche, and who had had several disappointments from habitual death of the fœtus in the two last months of utero-gestation. On the occasion referred to, the membranes were separated to a distance of two and a half inches from the interior of the cervix by means of an intra-uterine pessary, which was left permanently in the cavity, in the expectation that labour would speedily supervene. It was moved about from time to time, but no signs of approaching labour appeared. The fœtal heart continued to beat vigorously; and as it seemed quite probable that it might be carried to the full time with perfect safety, no supplementary measures were taken to insure the premature occurrence of parturition. The pessary was thus worn for thirty-one days in the cervix, and was frequently moved all round its interior, without the slightest effect; for labour only supervened at length on the completion of the full term of pregnancy. Believing that labour could be more certainly induced by the separation of the membranes from the interior of the *body* of the uterus, he (Dr. S.) had afterwards attempted to do this by passing the uterine sound to some distance into the interior of the uterus, and moving it slightly from side to side, taking care to avoid the site of the placenta. The success attendant on this form of operation was so great, that for some years past he (Dr. S.) had used the sound almost exclusively for inducing premature labour. It had one disadvantage, however, inasmuch as in some cases it was found necessary to introduce it several times before labour was finally brought on; and in one case that had occurred in his practice some months ago, six days elapsed from the time of the first introduction of the second ere labour finally supervened. The retention of the flexible portion of the male catheter in utero overcame this difficulty. If placed between the uterus and membranes, labour seemed almost invariably to supervene within twelve hours. If its extremity chanced to perforate the membranes during its introduction, labour did not supervene in some instances for twenty or thirty hours longer. In Germany they were in the habit of introducing a gum elastic male catheter into the uterus, and leaving it there with the stilet till labour supervened. Fearing lest the uterus might be injured by the presence of a firm foreign body, he (Dr. S.) had withdrawn the stilet, and left only the flexible catheter, which remained until labour supervened. Statistics of fifty cases that had been operated on by this means in Germany had been collected by Dr. Amann, who showed that in all, labour had been induced within twelve hours. He (Dr. S.) believed, therefore, that it would be found to be an improvement in the operation to leave a foreign body in the cavity of the uterus; but perhaps an india-rubber tube or some soft body would be found to be as safe an instrument, and quite as successful a one, as the elastic catheter. Dr. Simpson added, that in some special cases other modes of treatment might require to be adopted, and stated, in particular, that where it was desirable to have the os uteri dilated to some extent before labour came on, the sponge tent might be employed with advantage.—*Edinburgh Medical Journal, Sept.* 1861, *p.* 289.

90.—INDICATIONS AND OPERATIONS FOR THE INDUCTION OF PREMATURE LABOUR, AND FOR THE ACCELERATION F LABOUR.

By Dr. Robert Barnes, Physician to the Royal Maternity Charity, London.

[Much attention has been devoted to this subject lately, and its use revived. It was practised many years ago, but had become obsolete and forgotten.]

The only method used for a long time was the puncture of the amniotic sac directly over the os uteri. Experience showing that the premature escape of the liquor amnii led to the frequent peril of the child from undue compression in being forced through an imperfectly dilated cervix. Hamilton, to obviate this objection, substituted the separation of the membranes for a given space from the lower segment of the uterus; and Hopkins tapped the sac at a point remote from the os. Others gave ergot of rye. Merrem and Crause, in Germany, placed a flexible catheter in the uterus, leaving it there to excite pains. This plan was now adopted by Braun, of Vienna, who spoke highly of it. In 1820, Bünninghusen led the way in a series of proceedings for dilating the cervix by plugs and instruments of various kinds. In 1842, Hunter proposed to expand the vagina by inflating bladders introduced in a flaccid state. Braun then introduced his caoutchouc dilator. These contrivances have been followed by inflammation of the genitals and death. The application of galvanism, of the irritation of the breasts, of the carbonic acid gas douche, were adverted to. The carbonic acid douche to the vagina had caused sudden death in the hands of Scanzoni. In 1846, Kiwisch introduced the vaginal warm water douche; soon after Cohen, of Hamburg, revived a proposition of Schweighaüser, in 1825, to inject water into the cavity of the uterus. This, the intra-uterine douche, was now extensively used. The action and merits of the douche were discussed. Although easy and convenient, it was not free from objections. Dr. Guillier had related a case in which sudden death followed a vaginal douche, it being conjectured that air had passed with water through the Fallopian tubes into the peritoneum. Other fatal cases are known. The free injection of water into the uterus might detach the placenta; it might stretch the imperfectly developed uterine fibres beyond their power of reaction, causing paralysis. The author then adverted to the recent application of the caoutchouc dilator inside the os and cervix uteri for the purpose of dilating these structures. He quoted the case of Mr. Murray, and referred to the claims of Dr. Keiler and Dr. Storer. He then related two cases of placenta prævia, in which he had himself accelerated labour, successfully opening the cervix for delivery by turning by aid of water-power applied to the interior of the cervix. He also related the histories of three cases in which labour was induced and accelerated by the same method; one being a case of distortion of the pelvis simply, one of distortion complicated with cicatrised os, and one of almost complete obliteration of the cervix by cicatricial tissue. He described the instruments employed, and the advantages attending their use. The caoutchouc dilator acted gradually and gently; unlike the douche, it admitted of being applied to the exact spot where a dilating and exciting force was required; as a means of overcoming rigidity, it was more certain and efficient than any other, superseding bleeding, incisions, tartar emetic, and the stretching by the hand. As an accelerator of labour, it was so perfect as to supersede altogether the *accouchement forcé*, an operation which might now be exploded. It enabled the Practitioner to deliver almost at will, not only on a fixed day, but at a predetermined hour, a power that gave us control over cases of convulsions, obstinate vomiting, exhaustion from disease or hemorrhage, much needed and not hitherto possessed. In connexion with this subject, Dr. Barnes adverted to the importance of the method described by Dr. Braxton Hicks, of turning without passing more than two fingers into the uterus, by combined internal and external manipulation. Whenever expulsive pains could not be excited, this operation enabled the Practitioner to complete the delivery on the spot. The author concluded by classifying the methods of inducing labour according to their therapeutical application. They should be divided into—A. Preparatory measures. B.—Labour-provocative measures. C.—Labour-accelerating measures. He insisted much upon this division, believing that in many cases no one method could be relied upon, but that it was desirable to proceed in regular gradation through the stages of preparation of the soft passages, the evocation of contractile energy, and the acceleration of delivery, selecting in each stage such means as were most suitable. By the judicious consecutive use of these means we had it in our power to terminate a labour with certainty

within a given time, and that with a greater amount of ease and security to the mother and child than had been hitherto attained. This increased perfection of the operation justified the hope that it might be applied to the relief of emergencies and dangers, which had hitherto baffled the skill of the physician.

[The above is an abstract of a paper read before the Obstetrical Society of London. It induced some interesting remarks from other members of the society. Mr. BAKER BROWN considers the most successful method of inducing premature labour is to puncture the membranes, and he thinks this proved by the tables laid before the society by Dr. Barnes. The injection of liquids is too tedious and uncertain. He approves of the practice of incising the os in some cases, and considers it may be done when necessary "with safety and advantage."]

Dr. PRIESTLEY observed that in determining the safety to mother and child of the several methods of provoking labour, statistics were particularly valuable; and nothing appeared more conclusive of the advantage of one method over another than the 88 per cent. of children saved by Dr. Hamilton, who separated the membranes, but did not puncture them, as contrasted with the 45 per cent. of Dr. Merriman, and the still smaller per-centage of Dr. Lee, both of whom adopted the plan of puncturing the membranes. Of the various modes which had been suggested, Dr. Priestley had personally found the injection of tepid water into the cervix uteri the simplest and the best. It needed nothing more than a gum-elastic catheter, adapted to a Higginson's syringe, and the operation was almost painless, in most cases easily performed, and usually brought on labour in less than twelve hours. The water so injected had been said to be liable to separate the placenta, and one case had been published in which air had entered the uterine veins during the operation and caused the death of the patient; but it must be recollected that such alarming results were extremely rare, and probably might be entirely avoided by proper precautions. One disadvantage of injecting a large quantity of water into the uterus, according to Cohen's method, until the patient complained of a feeling of distension, had not been alluded to. By distending the lower segment of the uterus and altering the ovoid form of the organ the child was very likely to change its position and present preternaturally. Twice had Dr. Priestley witnessed this. In a case where he induced labour some time ago for Mr. Fergusson, of Notting-hill, the head distinctly presented when the water was injected, but when labour came on the child presented by the feet. Latterly he had taken the precaution to inject not more than four or six ounces of water, which was usually effective, and less likely to alter the presentation. The dilatation by caoutchouc bags might prove serviceable in certain cases, but under ordinary circumstances he was well satisfied with the injection of warm water.

Dr. HALL DAVIS remarked that in connexion with the Royal Maternity Charity and other institutions during many years, he had frequently been called upon, on account of various indications, to resort to the induction of premature labour, and had employed several of the methods which had been proposed. In the early part of his practice he adopted the original mode—that by the stilet, first performed in 1756 by Dr. Macauley, the colleague of Dr. William Hunter. He (Dr. Davis) made a small aperture, so as to ensure a gradual escape of the liquor amnii. In several cases consecutively this plan was perfectly successful as to the mother, and the children also; but subsequently two cases came in succession, where the children were still-born, which it seemed to him was due to the pressure exerted on the body of the child in the absence of the liquor amnii to defend it. Since the occurrence of those cases he had never, except where profuse hemorrhage had been the indication, resorted to the discharge of the waters for the induction of labour—as a first measure. It should be, he thought, a great object with Obstetricians to save the bag of liquor amnii for the labour, as being Nature's providential arrangement for safely dilating the genital passages, and at the same time for defending the child from dangerous pressure. His practice now, therefore, was to preserve the waters intact, if possible. He first, if

practicable, proceeded by introducing the index finger gently through the os uteri, and upwards outside the membranes, as far as he could reach; he then swept around it, thus dislodging the mucous plug, and separating the membranes to a corresponding extent. He subsequently introduced a piece of soft sponge, previously oiled, as large as could be passed without much difficulty. Where the os uteri would not admit the finger, he commenced with a sponge-tent, supporting it with a piece of sponge at the top of the vaginal tube. He had found these to answer in most cases; in other instances he had resorted to the use of an elastic bougie, passed high up, about half way into the uterus, outside the membranes, previously drawing off the urine, and leaving it in for five or six hours, secured *in situ* by tapes, so far as to prevent its slipping out again. This he found to answer only in one recent case; labour came on in four hours. He had also employed, and with success, the method usually accredited to Dr. Cohen, of Hamburg, consisting of the injection of water into the uterine cavity. The elastic tube used for the purpose of conveying the fluid to its destination should be passed up cautiously; care must be taken not to rupture the membranes by too forcible injection of the water, to allow it free egress, and also to guard against the injection of air with the water, especially by keeping the end of the suction-tube well immersed in the water to be injected. As regards the induction of labour by the ergot of rye, he (Dr. Davis) could testify that it had the effect of inducing it, but, unfortunately, the children appeared to be destroyed too frequently under its influence. He must say that he was much pleased with Dr. Barnes's paper, and with his proposal of an elastic dilator, water being the medium, as a means of dilating the os uteri, and so of inducing labour. Any addition to the previous resources for the induction of labour, having for its object the saving of the liquor amnii for the useful offices it serves to perform in the act of parturition, must, he apprehended, be considered a great improvement in careful hands.

Dr. TYLER SMITH was sure that great good would result from the paper and the discussion which had followed it. The subject itself was most important; and the intrinsic excellence of the paper, and the original proposals which had been made by the author, would do much to attract the attention of the profession to the induction of premature labour. In proper cases it was of the highest value, and its application in practice was still only in its infancy. What had taken place would no doubt lead to its more extended adoption. He did not think that he should have found any one holding a higher opinion of this operation than himself, but he could not quite agree with Dr. Barnes in placing it before the forceps. After the use of the forceps, he held the induction of premature delivery to be the most important and life-saving of all obstetric operations. He had tried most of the methods of proceeding which had been proposed, and for some years he looked upon the vaginal injections of Kiwisch (which he was the first to use in this country) as far better than puncture of the membranes or any other plan. For the last three or four years he had, however, used uterine injections, and had found them an improvement upon the method of Kiwisch. He had not known of any accident from the vaginal douche, except in a patient whose case had been communicated to him, and who was placed in a cold bath during the douche. In this instance death was caused by internal congestion and hemorrhage. The weak point of the vaginal douche was, that sometimes labour was several days or even a week before it came on. The only death which he had known to follow the uterine douche or injection occurred from puerperal fever, and was not caused by the mode of producing labour. His plan had been to inject about half-a-pint of warm water, of the temperature of the blood, into the uterus, through a catheter or tube passed four or five inches into the uterus. The injection of the fluid should be gentle, care being taken not to inject air. He had never known the coming on of labour to be deferred as much as twelve hours from the time of using the injection, and generally it had commenced immediately. So far there seemed little to be desired. He had never known hemorrhage to be produced by its use. He could not join in the light estimate which had been taken of

statistics. These were, in his opinion, of high value, and their collection was one of the most useful objects of the society. He regretted also to hear the light manner in which bold incisions into the cervix uteri, to facilitate labour and ruptures of this organ, had been spoken of. He thought the cervix uteri entitled to greater respect; and he should certainly feel it a grave accident, if, at any time during labour, the cervix were lacerated so as to allow the head to pass through it suddenly. Any new method of inducing labour with safety and certainty would, he had no doubt, receive due attention. He should certainly try the ingenious instruments devised by Dr. Barnes, and should be very glad to find them an improvement upon the injection of the uterus. It might be necessary to caution the profession, who would no doubt try the method of Dr. Barnes, against dilating the os and cervix too suddenly, so as to incur the risk of producing contusion or rupture of the lower part of the uterus, as the amount of fluid pressure which could be brought to bear was enormous. He considered that Dr. Barnes had earned the gratitude of the society by the manner in which he had brought this most important subject before it.

Dr. BARNES, in reply, said, that if it was found that out of 1,000 labours in England five women died, while fifteen or twenty died abroad, that might justify the conclusion that English midwifery was more successful than foreign. But the moment we sought to extract pathological and therapeutical laws from the compilation of statistical tables, we were surrounded by so many complications and disturbing clauses as to render any conclusions unsafe. If in his bed-side observation he saw that by puncturing the membranes too early the child's life was imperilled by the head being driven against or through an imperfectly prepared cervix uteri, he was convinced of the error of the practice, and was in a position from which he could not be dislodged by the abstracted facts of statistical tables, which could not be subjected to cross-examination and verification. In reference to the method of inducing labour which he had brought forward, he disclaimed the idea of advocating it to the exclusion of others; he rather insisted upon the necessity of observing the three stages he had defined—preparation, provocation, acceleration; and pointed out that, by due observance of this principle and the proper application of the caoutchouc dilator to the cervix, and the improved method of turning, the induction of labour was no longer a matter of uncertainty as to time, but one completely within our control.—*Med. Times and Gazette, April* 27, 1861, *p.* 456.

91.—INDUCTION OF PREMATURE LABOUR.

Case under the care of Dr. ARTHUR FARRE, at King's College Hospital.

The following case of induction of premature labour, for which we are indebted to Mr. John Barr Brown, the resident physician-accoucheur, has lately occurred at this hospital in the practice of Dr. Arthur Farre. The result was attended with complete success.

J. B., a mulatto, aged 38, of very diminutive stature, applied here, during the month of June, for the purpose of having labour prematurely induced. Her previous history runs thus:—She has been married eight years. In 1854, her first labour was terminated at the full time by craniotomy, performed by Dr. Farre. In 1856, labour was brought on, at this hospital, by means of the injection of hot and cold water, the woman being then fully seven months advanced in pregnancy; the child lived only a few hours. In July, 1860, being then about thirty-two weeks gone, she again applied at the hospital, and labour was induced a second time by means of the douche. The operation was commenced on the 28th of July, at four p.m. Uterine pains occurred slowly and irregularly until eleven p.m. on the 29th, when the membranes were forcibly ruptured, which greatly increased the force and frequency of the pains. The child was born at a quarter-past eleven on the 30th, and lived about sixteen hours, during which time it swallowed some

milk from a bottle. The weight, when dead was 3lb 2 o.z.; it measured in length sixteen inches. The mother had no milk in her breasts. She left the hospital perfectly well on the 12th of August.

In June, 1861, she applied for the third time. Dr. Farre saw her on the 26th. She had then, by her own calculation finished the thirty-third week of pregnancy. The point of the finger could be easily passed within the lips of the os uteri. Dr. Farre commenced the operation, at three p.m. on the 26th, by introducing an elastic catheter (No. 10 size) for about three inches within the external os uteri. He then swept it round, so as to separate the membranes from the walls of the uterus. This was easily accomplished, except where the head was found to be lying—viz., over the symphysis pubis, a little to the left side. The woman had some slight pains in the back after the operation, but these soon subsided. Uterine pains commenced at six p.m. on the 27th, but were very slight and at long intervals. The membranes ruptured spontaneously at two a.m. on the 28th. The os uteri was now a little larger than a sixpence, soft, cool, and moist. The pains continued slowly and irregularly till five p. m. on the 29th, when they became more effective. At nine p. m. the os was dilated to the size of a half-crown; the presentation was easily made out. The child was born at two a.m. on the 30th, the head presenting in the first position. The umbilical cord, which was looped round the neck of the child, was pulsating strongly. The placenta came away easily about ten minutes after the birth of the child. The child, a female, seemed well formed, and cried lustily. Its head was well covered with hair; the nails were perfect; the pupils of the eyes quite regular; no membrana pupillaris existed: it weighed the next day 3 lb. 4oz., and measured in length eighteen inches.

From this time, both mother and child progressed most favourably, the latter being fed on milk-and-water from a bottle, as the mother had no milk in her breasts. They left the hospital, on July 13th, both quite well.

This last method of inducing premature labour—viz., by separating the membranes from the uterus by a catheter—is the one specially advocated by Dr. Farre. In the two previous labours the douche was employed, in consequence of the occlusion of the os and the difficulty of passing a catheter.—*Lancet, Aug.* 17, 1861, *p.* 158.

92.—ON THE TREATMENT OF CASES OF ABORTION, IN WHICH THE MEMBRANES AND PLACENTA ARE RETAINED.

By Dr. W. O. Priestley, Physician-Accoucheur to the Middlesex Hospital.

[As is well known, during the first two months the ovum is frequently expelled without rupture of the membranes. But in the later months it sometimes occurs, that, the embryo having escaped, the secundines are not extended, and after the uterus has made repeated ineffectual attempts to expel them, the os uteri closes and action ceases. Authorities differ widely as to the treatment to be adopted. Denman, Davis, Ramsbotham, and Dewees, deprecate any attempts at extraction by the hand. Burns and Churchill say "only in exceptional cases"; and Tyler Smith recommends the removal of the secundines in *all* cases. The author has met with a considerable number of patients who have seriously suffered from placental retention].

The dangers and morbid conditions arising from membranous and placental retentions were stated to be:—

1. Flooding; this being sometimes of a very serious character, and the patient being always liable to hemorrhage so long as a fragment remains.

2. Decomposition of the uterine contents, leading to local inflammation of the uterus and surrounding tissues, to phlebitis and phlegmasia dolens, and lastly to general poisoning of the system, as evinced by irritative fever, peritonitis, rheumatic pains, the formation of abscesses, and even the occurrence of death.

3. Sub-involution of the uterus.

4. The generation of some of the forms of mole out of the tissue left in the uterine cavity, and the hemorrhage attending the presence and expulsion of these morbid growths.

The entire absorption of the placenta was regarded as uncertain and not to be counted upon. Cases illustrative of the several sources of danger were detailed.

In considering the various methods of treatment which have been recommended, the author's experience led him to believe that the effect of the administration of ergot was very uncertain, and that galvanism and injection of water were not to be depended upon. Any form of abortion-forceps which must needs be pushed beyond the reach of the finger he regarded as useless and dangerous. He was thus driven to the conclusion that the introduction of one or two fingers into the uterus was the safest and best way to remove the retained uterine contents. In his hands such manual interference had never been followed by any evil results, and a considerable number of cases had fallen under his observation. He begged particularly to be understood as not advocating rash and violent attempts to empty the uterus. Rough and careless manipulation might be fatal to a patient; but the consequences of placental retention were so serious that, under ordinary circumstances less risk was incurred by the operation. It was important to effect removal, not only before putrescence began, but before the os uteri became much contracted; and if in six hours after the escape of the embryo the placenta did not follow, it might be removed. The extraction might be effected sooner than this if much hemorrhage were present. He had found it convenient in operations to place the patient on the back, with the thighs flexed on the abdomen; and while one hand steadied and depressed the uterus externally, the other was with all possible gentleness passed into the vagina. The os uteri was then dilated with the index-finger, and the second finger followed if required. These two fingers formed the best and most sensitive forceps, and although time and care might be necessary to their introduction, their employment was most satisfactory. The administration of chloroform previous to the manipulation, afforded great assistance by relaxing the passages and saving the patient from pain. The author had succeeded in removing the placenta with the hand in some cases days and weeks after the escape of the fœtus; but he had never ventured on any such attempt if inflammatory symptoms and irritative fever had already set in. Where the os uteri was too contracted to permit the introduction of the finger for the removal of a placental mass, its dilatation could be effected by sponge tents, which had the double advantage of staying hemorrhage and facilitating the passage of the finger.

[The evil results of these kinds of retained placenta are considerably exaggerated. We almost invariably let nature do the work herself; she will almost invariably throw off the placenta safely sooner or later. Flooding is easily checked by the plug, which, in fact, answers two purposes: it plugs the passage and stimulates the womb to act. We may say all these cases do well if not meddled with too much.]—*Lancet, May* 11, 1861, *p.* 461.

93.—ACCIDENTAL SEPARATION OF THE PLACENTA AT THE SEVENTH MONTH OF PREGNANCY.

By G. E. Stanger, Esq., Nottingham.

Mrs. H., aged 36, mother of three children, healthy, and moderately robust, had arrived at the end of the seventh month of pregnancy, when she took a long walk on the 20th of March, 1861. On her return home, and even before she arrived there, she felt extremely faint, and was scarcely able to walk. She soon became so alarmingly exhausted, that a messenger was sent for me in great haste, as her husband thought she was dying. On my arrival at 3.30 P.M., I found my friend, Mr. Higginbottom, in attendance. The patient was lying prostrate on the bed, blanched, and in a state of syncope.

She could only speak in a whisper; but she complained that her body felt very large, hard, and distended. The pulse was very indistinct and fluttering, and her whole appearance indicated extreme danger. She was not aware that she had used any undue exertion, or that she had slipped or injured herself in any way; but I felt convinced that hemorrhage had taken place into the womb, and was in all probability still going on. The abdomen was as large as might be expected at the expiration of the full term of pregnancy. No fœtal sounds could be heard; and the patient remarked that she had not perceived any movement of the child after leaving home in the morning. At this time there was not the slightest hemorrhage externally; neither was there any indication of the approach of labour; and, on examination *per vaginam*, the os uteri was in its normal condition.

Under these circumstances, both Mr. Higginbottom and myself felt some difficulty in deciding what course we should pursue; for, on the one hand, we feared that if we did not interfere, and the hemorrhage continued, she must inevitably sink; and, on the other hand, if we attempted to induce premature labour, the blood might flow more rapidly as soon as it was allowed to escape from the womb, and death might ensue before we had accomplished the delivery.

We determined, therefore, to wait for a time, especially as there seemed to be now and then an attempt to rally; and we hoped that the os uteri might begin to dilate.

She continued in much the same state during the evening. Mr. Higginbottom remained with her the whole time, as I was only able to call occasionally, in consequence of another engagement.

At 10 P.M., the symptoms not being in any way improved, we decided on the propriety of bringing on labour; but, before doing so, we requested the opinion of Mr. Wright, the senior surgeon to the Nottingham General Hospital. Mr. Wright at once recommended this practice, and considered that the situation of the patient was so critical that it was doubtful whether she would survive the operation. Soon after this advice had been given, hemorrhage began to take place from the vagina in considerable quantity; and the syncope became still more alarming. I, therefore, immediately introduced a straight silver catheter into the uterus, and let off the liquor amnii. By the conjoint and persevering efforts of Mr. Higginbottom and myself, the os was sufficiently dilated to allow of a foot being brought down; but so rigid and unyielding was it that it was a long time before we could succeed in laying hold of the second foot, and even then the exertion required to extract the body was so great that the head was separated from the trunk, which, of course, greatly retarded the operation. We succeeded, however, at length, in removing the head; and we were rejoiced to find that during the two hours that we were engaged in effecting the delivery, very little hemorrhage occurred. The placenta was removed without any difficulty, being already partially, if not altogether, separated. Slight hemorrhage occurred about half an hour after, and great faintness and exhaustion continued for many hours; but the flooding was checked by the application of firm pressure. The patient recovered in a few days without any drawback.

Remarks.—I think it can scarcely be questioned that the plan which was pursued in this case was the only practicable one. Whether the operation ought to have been performed earlier may, perhaps, admit of some difference of opinion. If I were to meet with another case of the kind, I should, as soon as I was satisfied that a separation of the placenta had taken place, puncture the membranes, and then wait for the appearance of urgent symptoms before I forcibly dilated the os uteri.

The following case will show the danger of delay in such instances.

Several years ago I was summoned to a patient at the same period of pregnancy as the above. She was a person of remarkably lax habit, and had had copious floodings in several of her confinements. On this occasion she had been exerting herself at the garden in pulling up vegetables, and went home very much fatigued. When I saw her, at six A. M., she was very much depressed, tossing about in bed, and bathed in cold perspiration. She said that

she felt as if she should burst; and the abdomen was very hard and distended. There was no appearance of hemorrhage externally, but I felt satisfied that blood was being effused into the uterus, and I was convinced that she could not continue long, unless something was done for her. I sent a messenger immediately for Mr. Wright, but she died before he arrived.

I made a *post mortem* examination of the body the same day, in conjunction with my friend, Mr. Thomas Wright. We found the placenta separated throughout its whole extent, and the uterus distended by an enormous coagulum.

In this case, doubtless, the hemorrhage had been going on slowly during the whole night, and there seems very little question that if the patient had been seen sufficiently early, and prompt measures had been adopted, her life might have been saved. I apprehend that some sudden exertion or strain in these cases produces a slight separation of the placenta; that the blood which is then effused acts as a wedge to detach more and more of it, until in time the whole mass may be separated without any blood escaping from the womb. I do not remember to have met with any account of such cases in obstetric works, and I should be glad if some of the readers of the 'Journal,' would favour us with their experience in relation to this subject.

I think it worthy of remark that no stimulants were administered in the case first narrated. I am satisfied, from my observation of the effects of brandy in cases of flooding, that it tends rather to favour the continuance of the hemorrhage than otherwise, and, therefore, I have for some years entirely discontinued its use in such cases, and I believe that my patients have had not only less hemorrhage, but a more rapid recovery in consequence.—*Brit. Med. Journal, April* 27, 1861, *p.* 440.

94.—THE TREATMENT OF PLACENTA PRÆVIA.

By Dr. Robert Barnes, Physician to the Royal Maternity Charity, London.

[When Dr. Barnes revived the practice of separating the placenta from the lower zone of the uterus, he was careful to point out, that the separation of the placenta, either partially or wholly, from its uterine attachments, cannot by itself arrest hemorrhage. Contraction of the womb is the condition upon which the arrest of flooding depends. The great object of all interference in the management of cases of placenta prævia is, to secure this saving contraction of the uterine muscles. Dr. Barnes' views have not been thoroughly understood on these points by many. The report of the first case which follows was drawn up by Mr. Woodman, the resident obstetric assistant.]

"Mrs. F., menstruated last from Sept. 25th to Sept. 30th, 1860. Since that time she has had, especially during January and February, several sharp attacks of flooding, with labour-like pains, especially when she has overworked. On March 29th labour-pains set in strongly about five p. m., and there was flooding, but not enough to alarm her. As this kept on, however, and the pain increased in severity, they sent for Mr. Holman, the gentleman appointed to the case. On his arrival, he found a pool of blood on the floor, and the woman almost fainting. He sent for me immediately. When I came I found she had lost a great deal of blood, at least thirty or forty ounces, as there was a pool on the floor, and the linen of the patient, bedclothes, &c., were all soaking with it. On making examination, I found a portion of the placenta presenting externally, and nearly separated from the rest. No one had pulled it, for the women were afraid, and Mr. Holman thought it best to send for me, as it was so near the hospital. I removed this loose piece, and on making a digital examination, found that the placenta was mostly in the vagina, except a portion which was firmly adherent to the uterus, just within and above the anterior lip. I could also feel a small fœtal head in the usual position. The os was dilated to about the size of half-a-crown, but was easily dilatable by the finger. Pains had ceased, and flooding was still going on. I loosened the attached portion of the placenta, and removed it entirely,

which checked the hemorrhage completely. I then gave about a drachm of ergot powder in a little warm tea, which brought on pains again, and the fœtus was expelled in about fifteen minutes, with about three pains. No hemorrhage followed. The funis broke off in removing the placenta; it was very soft. The placenta was easily lacerable, and appeared to have been broken up by the natural efforts at expulsion. The child was œdematous. I need not further describe this or the placenta, as both have been seen by Dr. Barnes."

If we analyse this case, we shall find several points of interest. In the first place, we observe that there was profuse flooding in connexion with a dead fœtus, for there is no doubt that the fœtus in this case had been dead some time before the labour set in. This disposes of one theory that has been advanced—namely, that the occasional spontaneous arrest of hemorrhage is due to the circumstance that in such cases the child has been dead some time, and the ultero-placental circulation arrested previously to the supervention of parturition. This theory accords indeed with the doctrine of the placental source of hemorrhage, but it does not accord, as the case before us proves, with clinical observation. The simple fact is, that the hemorrhage did not stop until the womb contracted, although nearly the whole of the placenta was detached. When the portion adhering to the cervix was removed, contraction was excited, the flooding ceased, and the child was *expelled* in about fifteen minutes. The expulsion of the child is an unmistakable sign of active uterine contraction. This contraction, just as it does in post-partum hemorrhage, arrests the bleeding by constricting the mouths of the vessels.

I will relate another case in illustration of this fundamental principle.

Case 2.—On the 26th of April last I was called by a midwife of the Royal Maternity Charity to a woman who had been flooding profusely. She had borne four children at term. She had now arrived at the end of the eighth month of pregnancy. Flooding set in for the first time at ten p.m. on the 25th, and continued more or less during the night. The midwife saw her at three a.m. on the 26th, and I arrived at half-past four a.m. I was shown a large chamber vessel half full of clotted blood, and this was by no means all she had lost. The pains were very slight. The patient was weak, but not greatly exhausted. The os uteri was high in the pelvis, and was reached with difficulty; it was expanded to the size of a half-crown. The placenta was felt extending over the os internum, but the membranes could be felt anteriorly. I swept the forefinger all round the lower zone of the uterus, separating the placenta. Then I punctured the membranes with a steel pen, and holding the os open, whilst firm pressure was exerted upon the uterus externally, liquor amnii freely escaped. A bandage was then applied to the abdomen, to maintain firm compression on the uterus. By these means the capacity of the womb was much diminished; the muscular structure contracted in adaptation with the lesser bulk of the contents, and no more bleeding ensued. At this time it would have been indeed a concession to authority, but a violence to Nature, to have proceeded to forcible delivery. Things appeared to be now favourable for the completion of labour without the further intervention of art. The woman was delivered normally of a living child at one p.m. without further hemorrhage.

What do we learn from this history? Or let me first ask, what might we have learned, if, following ancient precepts, I had, during the impetus of the flooding, delivered forcibly by turning when first called in the morning? One thing we should not have learned: we should have excluded the possibility of observing the process by which Nature checks hemorrhage in these cases; we should have raised another barrier against the admission of true knowledge, receiving only confirmation in the prejudice that Nature is here utterly at fault, and cries aloud for the instant help of the physician. But something positive we might have learned too. In the first place, with a cervix uteri only partially dilated, firm, and high up in the pelvis, turning would not have been an easy operation; to drag the child through such a cervix is to afford it but a slender chance of life. In all probability, instead of a living child, we should have a dead one. In the next place, great vio-

lence would have been done to the cervix uteri, and this violence would perhaps have been revealed by the occurrence of secondary hemorrhage, the result of laceration of the cervix, and, at a later period by the development of uterine inflammation and pyæmia. Let me recall to your attention the following considerations. The normal slow and gradual opening of the cervix uteri under the natural influences of contraction of the uterine muscles and hydrostatic pressure is not seldom a painful operation. Whosoever has had the opportunity of dissecting the genital organs of puerperal women who have died within a few days after even an easy labour, will have witnessed an amount of contusion, of ecchymosis, perhaps of laceration, of the lower part of the cervix uteri which will satisfy him as to the hazard of forcibly stretching open that structure with the hand. But in a case of placenta prævia this danger of injury to the cervix uteri is enormously increased. Instead of structures in a normal condition expressly adapted to undergo with safety the distension of labour, we have before us structures preternaturally developed under a misplaced physiological stimulus; the cervix uteri is now full of large vessels, which when the placenta is detached, expose open lacerated mouths on the surface. Through this highly developed tissue, and over these torn, gaping vessels, the child must pass. Nor can it always pass with impunity, even when the delivery is accomplished by the regulated efforts of Nature. Occasionally, and under apparently the most favourable conditions, the bruising of the hypertrophied cervix and the injury to the vessels are followed by fatal hemorrhages or pyæmia. It must not be forgotten that the lochia, and all the discharges, healthy and unhealthy, from the cavity of the uterus, must flow over these bruised open vessels. It demands all the care and all the skill of the obstetrist not to add to this terrible risk. Yet surely the irregular bony fingers of the surgeon cannot, however tenderly he may use them, effect the distension of the cervix uteri without adding to this risk. This is what the *accouchement forcé* might have taught us. But you will say, there was another resource. Instead of turning, the placenta might have been wholly detached. No doubt it might; and I will even admit that the hemorrhage might have stopped. It would have stopped, however, not because the placenta was separated from the uterus, but because the operation would necessarily have caused the discharge of the liquor amnii. But this object was accomplished quite as effectually by simply puncturing the membranes with a steel pen. And I will ask you if you believe it possible to separate the entire placenta when it adheres, as it so commonly does in these cases, to a very large proportion of the area of the uterus, extending six, seven, or eight inches from the os internum, without passing the whole hand into the cavity? And if you had resorted to this proceeding, you would have actually used more violence than if you had turned; for turning can be effected without passing more than two fingers through the cervix.

Passing from the dangers we might have experienced had the woman been forcibly delivered, or the entire placenta separated, let us now see what the case, treated on true physiological principles, teaches us. First, we see the flooding persisting so long as there are no effective pains, and no contraction; secondly, we see the flooding arrested so soon as the placenta is detached from the lower zone of the uterus, and the liquor amnii discharged; thirdly, we then see the labour progressing under the unaided powers of Nature, and ending in the birth of a living child, and the safety of the mother. Not interposing, *nimia diligentia*, a screen of our own construction, we are enabled to detect another fallacy that has long hindered the recognition of the true physiology, and consequently fostered an erroneous treatment, of placenta prævia. This case tells us plainly, that the dogma which affirms that the hemorrhage increases with every successive detachment of placenta, and in proportion to the extent of the detachment, is not true. The flooding ceased when all that portion of the placenta which grew to the lower zone of the uterus was designedly detached by the finger, contraction, be it borne in mind, concurring; for without contraction the flooding might still have gone on. The remaining part of the placenta,

being connected with the lateral and fundal zones of the uterus, the region of normal attachment, was safe from the risk of detachment. There it remained to maintain the life of the child until committed to aërial respiration.

But all cases will not terminate so well unless further aid be given. The cervical detachment of the placenta and the evacuation of the liquor amnii may not be sufficient to induce full hemostatic contraction of the uterus. Great loss of blood not uncommonly tends to paralyse the uterus, so that it will respond neither to pressure, to ergot, to brandy, or any other oxytocic agent, so long as its walls are distended by the presence of the fœtus. Or the enfeebled powers of the uterus may be spasmodically exerted and perverted, owing to the abnormal condition of the cervix. Under these circumstances, the full and uniform contraction of the uterus, the true hemostatic process, cannot be ensured until the organ is emptied. And even although no further hemorrhage may occur, the exhausted patient rarely completely rallies whilst she remains undelivered. But from the moment that labour completed, the uterus is fully contracted, the systemic and mental reaction is surprisingly rapid. Depressing anxiety gives place to confidence; and confidence is the parent of security. Having done what you can to persuade Nature to help herself, you may find it necessary to complete delivery by artificial means. But still, even under this emergency, there is no need of precipitation or of violence. It is not necessary to force the hand through the cervix, either to tear away the whole placenta, or to extract the child. We possess other means of accelerating and completing the delivery, which possess this great recommendation, that they themselves bring no element of danger to add to the inherent perils of the case.

I will relate another history to illustrate the treatment to be pursued in these more formidable cases.

Case 3.—On Nov. 19th, 1860, I was called to a woman who had borne several children. She was in her ninth month, and had been losing blood freely for a week, with slight intermissions. Flooding came on with increased copiousness the previous night. I saw her at four A.M.; she was much prostrated; had yawned, and tossed her hands, and was very faint and anemic. The placenta was quite over the centre of the os. The cervix was more than an inch long. The os externum uteri was sharp, the size of a half-crown. There was little bleeding at the time of examination, and no pains. But as I feared the bleeding might return, and saw that the patient was too exhausted to bear further loss, I considered it necessary to expedite delivery. I therefore detached the placenta from the lower zone of the uterus, and then inserted a caoutchouc dilator into the cervix. By injecting water into this bag, the cervix was freely expanded in about five minutes; but pains were not excited. I was, however, enabled to pass my hand by the placenta, and reach the membranes, which I ruptured with a skewer, there not being tension enough to break the sac with the fingers. The head was presenting. A foot was easily seized. I ascertained that neither cord nor heart was pulsating. The child was extracted in a few minutes: it was premature and still-born. The uterus being now made to contract by pressure, the placenta was speedily cast and withdrawn. No hemorrhage followed the labour. The patient recovered well.

In this case had I not possessed the caoutchouc dilator, I should have been compelled to stretch open the cervix uteri with my fingers at the risk of injury, or to encounter the alternative risk of renewed flooding and a lingering labour, with exhausted uterine power. By accelerating labour, the patient was quickly placed in security.

To sum up: we have, you will perceive, an ascending series of remedial measures finding their application successively with the rising obstinacy and dangers of the case.

1st. Detach, with one finger passed through the cervix, all that portion of the placenta which adheres to the cervical zone of the uterus.

2nd. Tap the amniotic sac, and drain off the liquor amnii. To effect this, some change of posture on the part of the patient—holding the cervix a little free from the presenting head, which sometimes acts like a ball-valve, com-

pletely closing the os—and firm pressure on the uterus externally, will sometimes be necessary.

3. If the uterus still remain flaccid, and if hemorrhage continue, or, generally, where the patient is much exhausted, so that effective muscular contraction cannot be roused, artificial aid is called for in order to accelerate delivery.

This necessity may arise under two conditions. First, the cervix may be freely expanded. If this be the case, you may immediately turn the child by passing two fingers of the left hand through the cervix, gently yet steadily tilting the head forwards over the pubes, whilst simultaneously pressing the breech backwards and downwards, until a knee or foot comes over the os within reach, and then hooking this down into the vagina. When the breech is engaged in the cervix you must extract slowly and with care. The breech will thus fully prepare the cervix for the quicker and safer passage of the trunk and head, increasing the chance of life to the child. All this time steady pressure must be kept up on the uterus externally.

But, secondly, you may have an undilated cervix, with more or less rigidity. This is the condition in which the coautchouc dilator offers such invaluable service. A bag of suitable size and shape is rolled up and slipped into the cervix either by the fingers or by aid of a stem; it is then to be distended with air or water, so as to put the cervix gently on the stretch. This will act in three ways: it will mechanically stretch the cervix just as the natural bag of membranes will, when driven down strongly by vigorous contractions of the uterus; it will press upon the mouths of the vessels and tend to arrest the hermorrhage; it will, again imitating the distended amniotic sac, excite the diastaltic function of the uterus by stimulating the incident nerves distributed over the os internum.

In this manner the energies of the uterus may be roused sufficiently to terminate the labour without further help. If this should not be the case, then you may expect, in a short time, to find the cervix so dilated as to admit of turning in the manner already described. I assume that, throughout, all care has been taken to support the patient by nourishment and stimulants.

Sometimes the removal of the placenta will be tedious. There are three conditions which may impede the expulsion of the placenta: first, there is the atonic condition of the uterus, so frequently present in these cases, giving rise to actual paralysis, or to spasmodic and irregular contractions: secondly, the uterus is less able to cast off placenta which adheres to its lower zones: and, thirdly, there is a great liability to morbid adhesions. I have, I think, accumulated sufficient evidence to prove that a diseased state of the uterine mucous membrane is a predisposing cause of placenta prævia; and out of this diseased mucous membrane the decidua is developed which is to form a constituent part of the placenta, and to bind this organ to the uterus. If no morbid adhesions exist, you will rarely fail to induce the uterus to throw off the placenta, if you grasp it firmly for some minutes through the walls of the abdomen before you begin to pull upon the cord. If adhesions exist, it may be necessary to separate them with the hand. A firm binder is of especial service in these cases.

If you bear the leading principle well in mind, that contraction is the end to be secured, and if you bring into successive use the measures I have described, you will, I am confident, experience more success in the treatment of this, one of the most dangerous complications in midwifery, than has been hitherto arrived at.

The majority of the deaths in placenta prævia arise either from hemorrhage or from pyæmia. Hemorrhage you may greatly avert by accelerating the labour by the judicious resort to the separation of the placenta from the lower zone of the uterus, by rupturing the membranes, by pressure externally, and, above all, by the intra-uterine application of the caoutchouc dilator. Almost all the violence which is the most frequent cause of pyæmia, you may avoid by these accelerative measures, and by turning when the parts are duly prepared by combined internal and external manipulation. *Lancet, June* 1, 1861, *p.* 527.

95.—OBSERVATIONS ON PLACENTA PRÆVIA.

By Dr. CHARLES CLAY, late Senior Medical Officer to St. Mary's Hospital, Manchester.

[No form of uterine hemorrhage more especially calls for the energies and resources of the practitioner than that arising from placental attachment to the os uteri, and consequently any observations on the subject must be received with attention by the profession.]

In 1822, Kinder Wood, Esq., of the Manchester Lying-in Hospital, with whom I commenced my own professional career, had observed cases in which the placenta was attached wholly or partially to the os uteri, and in which, when the placenta chanced spontaneously to separate, immediate cessation of the hemorrhage followed, both child and placenta being subsequently expelled by the simple efforts of nature unaided by the accoucheur, and without any bad consequences, the patients generally doing very well, and making reasonably rapid recoveries. From these facts he inferred, that by simply detaching the adhered placenta from the os uteri by the forefinger, the case might safely be left to nature for completion in almost all cases; and that the result would be far less dangerous than the usually accepted plan of version, which is not always applicable without a great amount of violence, and even in its most desirable view is attended by a large amount of fatality, even when unaccompanied with the extensive hemorrhage that usually characterizes cases of placenta prævia. One of Mr. Wood's strongest points of argument rests on the *now well-ascertained fact* that in all cases, immediately after the detachment is effected, the hemorrhage *as certainly ceases*. Another point equally forcible is that detachment can be accomplished at much earlier stages of the dilatation of the os uteri—when only the finger can be admitted—and before any great prostration can have taken place, and this at a time when it would be the height of impudence, (which is the mildest possible term that could be applied to such a proceeding) to forcibly dilate an unwilling and almost undilatable os to the extent of introducing the hand into the uterus with the view of version. The amount of violence done to both mother and child is excessive. A point abundantly proved and confirmed over and over again by the high rate of mortality displayed in the statistical returns of various authors. Indeed the per centage of mortality shown on version *in all cases is so very high*, that, without the necessary increase applied to those cases where extensive hemorrhages accompany it, it cannot be justifiable to increase the mortality if any possible means can be suggested for its reduction, and which I trust will be clearly shown in these remarks.

Mr. Wood's plan of detachment was not only proposed, but put into practice for a considerable period before 1822, and with invariable success; an example that was not lost on his pupils, myself amongst the number. I have, for nearly forty years, continued the same practice with almost entire success, except in one or two solitary instances, where the distance travelled was great, and consequently the loss of blood, and time, had produced an unconquerable amount of prostration. In 1845 Professor Simpson of Edinburgh, whose indefatigable exertions for the improvement of the profession are beyond all praise, again opened the general question, and a lengthy controversy arose between him and Dr. Radford of Manchester as to priority of views. Mr. Wood's previous claim was almost lost sight of during this controversy; and this was the more remarkable as Dr. Radford had been the colleague of Kinder Wood for many years, and was perfectly well acquainted with his mode of practice.

In cases of placenta prævia, repeated examinations cannot be too severely condemned. The one examination to ascertain the facts of the case should, if possible, be followed by prompt and energetic means to check the hemorrhage by detachment, and thus facilitate subsequent delivery by the efforts of nature only. If the hemorrhage ceases on the detachment of the placenta, the delivery may with great propriety be waited for, and without danger. I have scarcely known an instance to the contrary. In reference to the old

plan of version and immediate delivery, for which the detachment of the placenta is proposed as a substitute, there is in the first place the heavy rate of mortality of 1 in 3 to the mother, and 1 in 2 to the child, increased considerably if those cases are treated separately. Where the turning and delivery are effected when the dilatation of the os uteri has only just begun, that is, only to admit the point of one finger, or to within the size of a shilling at most—a rate of mortality would be shown which I do not hesitate to place at near 50 per cent. to the mother, and almost universally fatal to the child. These figures have to be compared with 1 in 44 to the mother, and 1 in 5 to the child, where the placenta is detached. Secondly, there is the probability of means having been previously tried to check the hemorrhage by rupturing the membranes, or giving ergot, *or both;* either of which will immensely increase the difficulties and dangers attendant on subsequent version. Thirdly, if the os uteri is but very slightly dilated and not disposed to do so, the violence done by attempting to turn is very serious and the difficulties of version increased. Fourthly, if the hemorrhage occur before the period of utero-gestation is completed (the seventh month for instance), the os is certainly not prepared for *extensive* dilatation, and I do not believe it ought to be subjected to such extreme treatment. Fifthly, the danger of waiting for a sufficient state of dilatation to effect version judiciously (say to the size of half-a-crown, as generally admitted by the best authorities), and the great loss of blood in the interim, when the hemorrhage can be at once checked and the waiting for natural expulsion justified, by the simple process of detachment. Sixthly, in almost all cases the prostration is often so great that even the necessary efforts of version will in very many instances hasten death.

I am convinced from long experience, that the dangers of version and hemorrhage may in a great measure be done away with by the simple detachment of the placenta. I have never known it to fail, nor do I believe it will ever fail if the detachment is completely and properly effected. We have also the highest authorities for stating that the arrest of hemorrhage is complete in 19 out of 20. If even then for the sake of argument we admit one failure in twenty cases, the results of detachment would stand immeasurably superior to the old (and I trust soon to add obsolete) system of version and immediate delivery.

It has been objected to detach at the seventh month, the os not being dilatable; but I have found it as easy dilatable at the seventh as at the ninth month, I mean *so far* as is necessary for detachment only, and therefore believe the objection to be more ideal than real. Then again, if it be an objection, how much more objectionable must it be to dilate more extensively, as in version and delivery, where the violence must of necessity be greater. Others argue that the fœtus may be retained for days after the detachment and fever may arise, but as no facts have been advanced to confirm such an opinion, it can only be entertained as a mere supposition. I have never seen the fœtus retained, and consequently no case of fever from such a cause has ever came under my notice, and certainly does not apply to cases of fœtus dead *in utero* from other causes. After all, there is such a remarkable difference in the statistics on these important questions, that I should consider any person reckless indeed of human life, who would advocate turning and delivery in cases of placenta prævia in preference to detaching the placenta, if at all aware of these results. But I think few could be found to go further than either of these doctrines, and advocate forcible dilatation at early periods, or attempt turning with the os only capable of receiving the point of the forefinger, or at most dilated to the size of a shilling; increasing the already too great mortality, and adding greatly to the future difficulties to be contended with. One other point has been advanced —that detaching the placenta encourages indolence of practice, and screens inability. I would only observe in answer, that to watch such a case *as it ought to be watched*, is any thing but indolence; and to detach a placenta neatly and effectually, so as to realize our full expectations, requires as skillful and as careful manipulation as version. And if the accoucheur can

accomplish the former, there is no fear of his being able to manage the latter.

In conclusion, I have never witnessed any bad consequences from detaching the placenta; there is infinitely less violence done, the danger is much reduced, future difficulties are of less importance, and the results far more favourable. And with the accumulated facts of the past forty years, from individuals of the highest standing in the profession, we may safely hope never so far to retrograde as to adopt the old and barbarous system of boring through the placenta, turning, and delivering the child. But even if we should be so far led astray as to accept this old barbarism, let us at least escape the opprobrium of attempting such practices in the earlier stages of dilatation of the os uteri, and *knowingly* increasing all the dangers attendant on such cases.—*Glasgow Medical Journal, July* 1861, *p.* 129.

96.—ON A PECULIAR (SYPHILITIC?) DEGENERATION OF THE DECIDUA.

By Professor Virchow.

Professor Virchow recently examined the membranes of a fœtus from two to three months old, which had come away from a woman who had been healthy before her marriage, but had soon after it become hoarse, and suffered from syphilitic ulcers of the throat. These were cured by the protioduretum mercurii; she then conceived and had a miscarriage soon afterwards. The fœtal part of the membranes showed an unusually strong development of the cells of the chorion at the placental part, which looked like cylinders and were covered with long and fine epithelial cells. The maternal part of the membranes was even more changed; it had come away quite separately from the ovum itself. The whole inner part of the mucous membrane of the uterus (the decidua vera) had become loose, and only that part which covered the fundus was wanting. The change was especially striking at the posterior and anterior part of the inner surface of the decidua, where it was very thick, from one to four lines, and was covered with large, polypous growths, part of which were half an inch long, a quarter of an inch and even more broad, and three-quarters of an inch high. Their insertion was broad, and they became narrower towards the top, which was bluntly rounded. Between these growths the decidua was smooth, a little wrinkled, and of a pale whitish colour. The orifices of the utricular glands were only with difficulty recognised. The whole of the external surface was rough, almost villous. The large tuberosities, which were similar to papulæ and tubercles, had a smooth and dense surface of a red colour. A hyper-plastic state of the mucous membrane evidently existed, which was also confirmed by the microscopic examination; for M. Virchow found a foundation layer in which fibres were recognised, and large cells of lenticular form, which on a perpendicular cut looked like thick spindles. This hyper-plastic state was no doubt due to irritation; it was an endometritis papulosa and tuberosa, which had probably caused abortion. It is difficult to say whether the endometritis was of a syphilitic nature, and whether the papulæ and tubercles were to be considered as condylomata. But this is very probable, as M. Virchow has found in the mucous membrane of the uterus of syphilitic women sometimes symmetrical, sometimes truly papulous swellings.—*Medical Times and Gazette, June* 8, 1861, *p.* 610.

97.—WHAT INFLUENCE HAS CONTRACTION IN STOPPING UTERINE HEMORRHAGE?

By Dr. James Adams, Great Wakering, Essex.

[In Dr. Barnes's lecture on placenta prævia (see page 188 of this volume,) the opinion generally held by medical men is expressed, viz.—that the hemor-

rhage which follows the separation of the placenta is stopped by the contraction of the uterus, causing compression of the vessels, and thus preventing the escape of blood.]

The contraction has without doubt great influence, but I do not think that it entirely accounts for the phenomena. I have found in more than one case that on adopting Dr. Barnes' plan of partially separating the placenta the hemorrhage has ceased, although the bulk of the uterus has not materially diminished. For the contraction to be so perfect as to close the vessels, the circulation would be so arrested in the attached portion—a part of the supply being already cut off—as to render the birth of a living child utterly impossible. In Dr. Barnes' second case the child was born alive several hours after the partial separation of the placenta, and consequent arrest of flooding. In this case the liquor amnii was discharged, and the uterus contracted on the child; but still the size of the uterus was very great, and the contraction was not permanent, but followed by relaxation. What is to prevent the flow of blood during the relaxation? The vessels would still be very large, and the pressure of the child's head could not act altogether as a compress, as a portion of the placenta was interposed between the head and the surface of the uterus.

A more reasonable explanation of the arrest of hemorrhage appears to me to be that there exists in the uterine vessels, as in arteries generally, a power of contraction and retraction; for we find that the flooding ceases at once on the violent separation of the placenta, but not when it is gradually and gently separated, either by the enlargement of the uterus and disappearance of the neck, or by uterine contraction, as in cases where labour has begun and the os is somewhat dilated. Two cases of this nature have fallen under my notice, the arrest of hemorrhage having been effected in one by the manual separation of the placenta, and in the other by turning and delivery. That the power of contraction and retraction does exist in the vessels of the uterus is certain, or how would they become lessened with the diminished size of the uterus as labour becomes completed? They would in such a case become folded upon themselves. If they possess the power of contraction and retraction, why should it not be exerted for the purpose of stopping hemorrhage? That Nature should have endowed them with inherent power for effecting so important a purpose is only what we should expect, considering how beautifully perfect she is in all her operations.—*Lancet, July* 6, 1861, *p.* 23.

98.—ON THREE CASES OF PUERPERAL CONVULSIONS.

By Dr. Robert Harper, Holbeach.

Case 1.—A. M., aged twenty-three, a stout, healthy-looking young woman, at full period of gestation; first pregnancy.

Feb. 6, 1858. Eleven A.M. I am summoned hastily to see her in consequence of her having had several fits. When I arrived (a distance of two miles), I found that the convulsions followed up more quickly, and that stertor and deep insensibility existed between the attacks, whereas, at first, she was perfectly conscious during the intervals. The convulsions were so powerful that it took from six to eight persons to hold her. Her tongue was much lacerated in consequence of the attendants not having put anything between her teeth during the fit. The os uteri was not dilated; pulse full, hard, and bounding. I bled her to about sixty ounces; and although the pulse was not sensibly altered, I was afraid to go on. Croton oil and a large dose of calomel were given; castor oil and turpentine enemata administered; the hair was cut off, and cold lotions applied to the head. About three hours afterwards the bleeding was repeated to about twenty ounces, and a large number of leeches applied to the head. The croton oil and calomel were also repeated.

7th. Convulsions, stertor, and comatose condition the same; the bowels freely and repeatedly acted upon; the os uteri dilated, and head down in the

pelvis. She was delivered of a dead child by the forceps; the placenta easily extracted. A blister was applied to the neck, and mustard plasters to the calves of the legs.

8th. Convulsions gradually became less frequent until they ceased altogether about noon. Complete consciousness did not return till after their total cessation. From this time she gradually improved, and got quite well. She has had two living children since.

Case 2.—A. B., aged twenty-one, a stout girl; first pregnancy; full period.

May 7th, 1859. Labour commenced last night at eleven A.M. She was progressing favourably, and I left her in charge of my pupil, Mr. J. Morton, for a short time. At two P.M. she was seized with a fit, soon followed by others; stertor and insensibility between them. Pulse firm and bounding. The head being in the pelvis, I delivered her with forceps of a living child. The placenta was quickly thrown off; soon after which flooding, to a most alarming extent, occurred, and which was restrained with difficulty by the introduction of the hand into the uterus, and pressure, combined with cold applications, to the abdomen. The convulsions and deep insensibility continued for more than forty hours, and although considerable abdominal tenderness followed, she made a good recovery.

The same treatment was adopted as in the preceding case, except that venesection and leeching were not used.

Case 3.—H. F. M., aged twenty-four. About ten months since she was rather delicate-looking, but has become much more robust the last few months. Primipara, full period.

April 29th, 1861. Was delivered of a living child at three A.M., after only three hours' labour. Placenta thrown off naturally; bowels acted freely during labour from a dose of castor oil taken the previous evening. Two P.M.: Progressed most favourably up to this time, when I visited her. She was at first asleep, and upon awaking she appeared alarmed; did not answer questions put to her; seemed to have a choking sensation in her throat; then a violent winking of the left eyelids came on, and directly afterwards a convulsive fit. Stertor and total insensibility immediately followed. The convulsions recurred every twenty minutes throughout the remainder of the day. Calomel and croton oil were administered by the mouth; the hair cut off, and cold applied to the head; bleeding from two veins at once to between thirty and forty ounces.

30th. Three A.M.: Much the same. Several leeches were applied to the temples, and mustard plasters to the calves of the legs; bowels acted freely. Eight A.M.: Has only had one more fit; stertor less, but insensibility the same; bowels acted several times. One P.M.: The same, except that the pulse is 140, and not so full. I examined the abdomen carefully, but could not ascertain that there was any tenderness or tumefaction. Six P.M.: Patient quite conscious; pulse 140, small and moderately firm; complains of pain and tenderness over the lower part of the abdomen. Ordered calomel in small repeated doses; leeches to painful part of abdomen, followed by poultices.

May 1st. Much the same, except that there is tumefaction of abdomen; no more fits; sensibility complete. About two hours after this she became much altered, and gradually sank at twenty minutes to four P.M.

Remarks.—I have sent the foregoing very briefly written cases for publication, because it is unusual for three such cases to happen in one medical man's general practice in so short a time; secondly, they each differ as to the period at which the convulsions commenced—namely, before labour began, during labour, and eleven hours after its completion; and lastly, it will be interesting to hear the opinions of other practitioners as to the most desirable and efficacious treatment of one of the most formidable and frightful complaints which a medical man ever has to treat or witness, entailing upon him much labour, thought, and anxiety, and, in the case of a fatal termination, often much opprobrium. In the first case which I have related, I was afterwards told by the patient's friends that if she had died, they should *not* have attributed her death to the disease, *but to my excessive bleeding.* In

the case of A. M. and A. B. there was evidently a plethoric condition previously; but as I knew nothing of them till called in at the last moment, this did not come under my observation. In the case of H. F. M., she was at all times highly nervous, having a *great dread* of seeing anybody in a fit (which from the office she held she was liable to), and the convulsions were immediately preceded by an appearance of alarm, the patient, as it is termed, "awaking in a fright."

In conversation with a very intelligent medical friend, I was told by him that an eminent obstetric lecturer to one of the leading London medical schools repudiates the bleeding plan of treatment now for some years almost universally adopted in this complaint. If such be the case, I shall be glad to know if other teachers promulgate the same views, and the amount of success which has followed the new system.—*Lancet, June* 22, 1861, *p.* 607.

99.—USE OF CHLOROFORM IN PUERPERAL CONVULSIONS.

By Dr. Fred. Page, Landport.

On Feb. 14, Mrs. S., a woman about 30 years of age, in every respect healthy, and naturally of a cheerful disposition, at the full period of pregnancy with her sixth child, was suddenly seized with convulsions about midday. When I saw her, she had had seven or eight severe attacks in an hour. She was lying in a state of coma, with stertorous breathing, contracted pupils, pulse full, strong, about 80. She had a severe paroxysm soon after I arrived, with violent struggling, foaming at the mouth, &c., which left her, as before, in a state of lethargic sleep.

The os uteri was intact; there was not the slightest disposition to dilate: nor was there any symptom of approaching labour. She was bled to sixteen ounces. A violent fit ensued. I then kept her under the influence of chloroform for half an hour, and waited another half an hour; no fit occurred. A turpentine enema was given; and a blister was applied to the neck.

At six P.M., she had had convulsions at intervals. At the moment of my entering the room, the child was expelled suddenly during a fit. The placenta followed at once, and the labour was complete. The patient was still insensible, with rapid convulsions. The enema operated well. I again used chloroform for half an hour.

10 P.M., four hours after labour. The patient was very quiet, sleeping: there had been no return of the convulsions.

Feb. 15, 8 A.M., sixteen hours after delivery. She had been in convulsions during the night. I found her at 9 P.M. much as I left her the night before, in profound sleep. She had convulsions whilst I was there. Chloroform was again given for half an hour; after which she had no attack. Sensibility gradually returned, but it was some days before she quite recovered. She and her child are now quite well. I may remark that she has not the slightest recollection of anything that happened during her illness.

Remarks.—This was a case which assumed what is called the apoplectic form, in contradistinction to the hysteric, and in which much doubt has been expressed as to the propriety of exhibiting chloroform. In reviewing this case, it is evident that chloroform checked the attacks. It was used at three distinct periods: 1. Before delivery, preventing them for two hours; 2. Directly after delivery, for four or five hours; 3. Sixteen hours after delivery, when they ceased.

The first anxiety in puerperal convulsions is to empty the uterus and bowels as early as possible; but convulsions continued for twelve hours after the former had taken place. I must conclude that bleeding, the use of chloroform, and early emptying the uterus, are the grand remedies in this disease—especially chloroform. I shall be glad to hear more of its use in these cases.—*British Med. Journal, April* 13, 1861, *p.* 386.

100.—ON A CASE OF EPILEPTIC PUERPERAL CONVULSIONS.

By Dr. F. W. Wilson, Medical Officer to the East Grinstead Union and Workhouse.

[This case is interesting from the fact of chloroform having exerted a marked influence in arresting the fits, both during and after labour. The patient was eighteen; primipara in the eighth month of pregnancy. The fits commenced early in the morning, and continued with increasing severity during the day. In the evening her condition is reported as apparently desperate.]

The fits were occurring every quarter of an hour, and were extremely violent. The respiration in the intervals was effected with the greatest difficulty; indeed, in the state she then was it seemed both to Mr. Whyte and myself impossible for her to exist more than two or three hours. As labour had somewhat advanced, the os uteri being nearly the size of a half-crown and very dilatable, it was determined to deliver artificially; but on account of the violence and frequency of the fits we also determined to use chloroform cautiously ere doing anything. I placed half a drachm in the inhaler and held it to the face; the air was drawn in with so much violence that in half a minute considerable effect was produced, the pulse being scarcely perceptible, and catching of the breath occurring, which caused us some alarm. We were neither of us prepared for the most favourable effect which afterwards, however, ensued, as in ten minutes from the application of the chloroform the stertor was much diminished, the pupils became somewhat under the influence of light, and our patient was lying as placid as when I saw her in the morning; she had not breathed so easily, indeed, since the commencement of the fits. In addition to these good effects, labour advanced with regularity, and by applying the forceps it was completed in the space of about half an hour, only one fit occurring during the time, for which the chloroform was again applied with good effect. Hour-glass contraction occurred, which rendered it necessary to introduce the hand to remove the placenta.

Having remained an hour afterwards with my patient, and no fit occurring, I left, desiring to be sent for in the event of an unfavourable change. Shortly after my leaving a severe fit again came on, and in an hour's time another. When I reached her (about three hours after delivery) I found the stertorous breathing as bad as ever. Another severe fit shortly supervening, I again administered chloroform, after which the breathing was tranquillized and no fit occurred for some time. Once afterwards, chloroform was again given with apparently equally good effect, and after this only one slight fit occurred. *Lancet, May* 4, 1861, *p.* 432.

101.—ON SUPPORTING THE PERINEUM IN LABOUR.

By Dr. Graily Hewitt, Physician to the British Lying-In Hospital; Lecturer on Midwifery and Diseases of Women and Children at St. Mary's Hospital Medical School.

[Dr. Hewitt thinks that the principal effect actually produced by supporting the perineum is retarding the advance of the head, and even this effect cannot be obtained unless by the exercise of a considerable degree of force.]

It appears to me that the only conclusion we can come to is, that the operation must be rejected. Its object, if that object be limited to retarding the advance of the head, may be in itself good; but this particular method of attempting to gain that object is objectionable. In ordinary cases of labour, as a device for preserving the perineum from laceration, I believe it to be practically worthless, and there are grave reasons for surmising that in many cases it has led to the very evil it was intended to prevent.

The arguments derived from a careful consideration of the mechanism of the process of dilatation of the perineum and expulsion of head, all, in fact, lead us to the conclusion that the proper treatment of the perineum in labour is, under all ordinary circumstances, to abstain from active interference of any kind.

Is the conclusion arrived at by reasoning, countenanced by the results of practice?

In the works of Denman, we find the following:—"When women were delivered without assistance, I have not, in any case, observed any considerable laceration." (Vol. I, page 60.) That the accident does occur, however, in cases where the delivery occurs unassisted is now well known. The explanation I have already given of this occurrence—that the laceration is due in these cases, not to the absence of assistance, but to the rapidity of the labour—I would now repeat.

Drs. McClintock and Hardy consider medical assistance so necessary, that they endeavour to explain why laceration does *not* occur in cases of unassisted labour, on the theory, "that inasmuch as these females are almost always involuntarily subjected to the deprivation we have mentioned, they habitually use their utmost endeavour to retard the birth of the child when they feel the head in vagina, in the hope of aid reaching them before the critical moment of delivery; and another reason is that such patients have been spared the ill effects arising from vaginal examinations." (Report, &c., p. 9.) It may, I think, be doubted whether patients do, as a rule, under such circumstances, exercise the kind of control mentioned. The exemption from laceration in such cases appears to be explicable in a very different manner. It is very certain that many deliveries, and rapid ones, do take place in the entire absence of all assistance, and with perfect safety to the mother, so far as the perineum is concerned. The only conclusion to be drawn from the fact is, that assistance must be of little service in ordinary cases, seeing that no bad result usually ensues in extraordinary ones when it is absent.

We cannot, at this moment, procure any considerable numerical testimony as to the results of the non-interfering system; the practice of supporting the perineum has been too universally adopted to allow of this being the case; but so far as the evidence procurable goes, it is calculated to give us every confidence in the resources of nature. Dr. West of Alford, and Dr. Swayne of Bristol, both assure us, from the results of a large number of cases, that the plan of "letting the perineum alone" has with them given very satisfactory results.

Is there, then, nothing to be done to preserve the perineum from laceration?

There are certain precautions which it is necessary to take, and to which I should be disposed to attach very considerable importance. When the head is passing through the vaginal outlet care must be taken that there be no impediment to its passage forwards. If the thighs of the patient be closed, and if the legs be in a straight line with the body, there may be an impediment of this kind, and the natural result is, that the fourchette is called upon to bear a much greater strain than is necessary. The practice adopted in France, of delivering the woman lying on the back with the legs separated, is the one best calculated to remove this source of danger to the perineum. So, again, in the delivery of the shoulders, care must be taken to direct the delivery as much forwards as possible. It is very easy to see how infinite mischief may be produced by want of attention to this rule, and it is, in fact, extremely probable that laceration is not seldom due to this cause.

By adopting these simple precautions, I believe that the practitioner will do all that is possible to *prevent laceration* in ordinary cases.—*British Med. Journal, May* 11, 1861, *p.* 487.

102.—ON LACERATION OF THE PERINEUM.

By Dr. James Gray, Glasgow.

[Dr. Gray considers that Dr. Graily Hewitt has overlooked the main point in question; namely,—why laceration in natural labours takes place in one case and not in another. He takes it for granted that in every female the perineum is alike in its anatomical relations.]

I hold there is great diversity in its position, its length, and its relation to the pubis and coccyx. The pubic margin of the perineum or fourchette in one female is nearer the symphysis pubis than in another, while in a second it is nearer the coccyx; in a third, it holds an intermediate place, and so on; but I will class these diversities into three orders, which I shall designate—1. Coccygeal; 2. Axial; 3. Pubic.

In the first, or coccygeal, the labour progresses and terminates without rupture of the perineum, as the perineum is behind the axis of the outlet, and is not carried forward by the presenting part of the child's head as it emerges under the arch of the pubes.

In the second, or axial, the perineum holds a mid position, and, consequently, in such cases a slight rupture is apt to take place, as from the intermediate position of the fourchette, the presenting part of the head stretches and carries a portion of it forward.

In the third, or pubical, laceration can hardly be prevented; in this order the fourchette is anterior to the axis of the outlet, and as the head emerges under the pubic arch, it pushes the distended perineum forward funnel shaped, stretching it, in many cases, from three to five inches; as the bones do not give way, the head is more inclined to pass in the axis of the outlet.

I think this view of this subject sufficiently explains why rupture does not follow, in all cases, the birth of the child.

Dr. Hewitt says that supporting the perineum "must be rejected." I hold that by gently supporting the perineum in the second and third order, and guiding the head of the child forward in the direction of the abdomen of the mother, in the great majority of cases, we may prevent laceration; at all events, it greatly lessens the rent.

Of 2896 cases attended personally, in only one did laceration occur and extend into the rectum. When this happened, the perineum was not supported; the labour was not rapid; neither was the head of the child very large; nor was there any deformity in the parts of the mother. I believe this accident might have been prevented.

As an auxiliary, chloroform ought not to be overlooked; for it is well known that under its influence the perineum becomes relaxed, and danger to that part is thereby very much lessened.

Conflict of opinion suggests an opposite line of practice, and a certain amount of truth may be on both sides. It would seem, therefore, to be practical to adopt a middle course without entirely rejecting either recommendation.—*British Med. Journal, June* 22, 1861, *p.* 654.

103.—ON SUPPORTING THE PERINEUM IN LABOUR.

By Dr. Edward W. Murphy, Professor of Midwifery in University College.

[Dr. Murphy gives us his views as follows:]

The whole action which takes place by which the womb is opened, the child forced through the pelvis, the perineum expanded and the head expelled, may be considered as a struggle of opposing forces. The tissues of the cervix uteri, the bones of the pelvis, the tissues of the perineum, are the resistance: the muscular power of the uterus, the overcoming force.

But such a struggle must not be considered merely as a question of dynamics; the action is vital, and every provision is made so to regulate it as to prevent injury. Thus, when the tissues of the cervix are to be dilated, the power of the uterus is kept under control by the liquor amnii; and if it escape by the suspension of its action, if the force of the pains excite irritation, every means are used to prevent the expulsive force of the uterus from suddenly expanding, or rather tearing through the tissue; nevertheless the cervix is frequently torn by the violence of the pains. So with regard to the perineum, if the membranes be entire, the most gradual and least irritating force expands it; if not, and the head descends, its pressure is only continued during the pain; when that ceases, the head retreats, and the irritation is removed.

Thus the expansion proceeds, the tissues becoming thinner and thinner; and although they are sometimes so attenuated that the head seems to press down almost into the world, yet it will again retreat until the next pain, the parts not being yet prepared to yield.

The practitioner is often disappointed by this delay and uses every effort to prevent a repetition of it; he therefore endeavours to direct the head forwards towards the pubes, and thus to force it over the perineum. I am not quite sure that this practice does not frequently cause slight laceration. But why does the head retreat when the perineum offers such little resistance? Because *the act of dilatation* has not taken place.

The tissues of the cervix uteri and the perineum both possess a power of expansion independent of the force employed against them. Both may be dilated suddenly and unexpectedly, and either may offer an obstinate resistance.

When this act of dilatation takes place, the head passes safely over the perineum; if it should not, there is always the risk, if not the certainty, that if the head be forced into the world, the perineum must be torn, and this whether it be thick and resisting, or thin and apparently yielding. Cases have occurred in which the tissues have been so expanded, that the head has been forced through the perineum, without the margin yielding in the least.

Hence in the management of the perineum, so as to save it from injury, the question arises—What prevents this act of dilatation taking place? The leading causes which interfere with it seem to me to be congestion and inflammation. If irritation be excited, congestion takes place, and the action is arrested; if inflammation take place, the perineum becomes still more unyielding. It remains, then, to determine whether these unfavourable results are more likely to occur when the perineum is supported by the hand of the practitioner, or if it be left to itself. There are very few cases in practice where the perineum is completely unsupported during the expulsion of the head, unless indeed in those instances of sudden expansion where the head is expelled by a single pain. It is impossible, therefore, to draw a comparison by quoting cases, but some conclusions can be formed by observing the symptoms which present themselves in an ordinary labour, when the perineum is being dilated.

No pains are so agonizing as these; the support of the hand relieves the patient; she is conscious that her labour is assisted; at no period are the services of the practitioner more earnestly acknowledged. It is fair, therefore, to infer that this support relieves the irritation; so also it diminishes congestion, as pressure on distended vessels always does.

If such, then, be the case, the support of the perineum is essential, in order to prevent those causes coming into operation which interfere with the act of dilatation.

But besides this, the action of the uterus and that of the perineum do not always correspond; the one may resist the other, and the pains are sometimes so powerful as to tear through the perineum, even when most carefully supported.

What the result would be, when not assisted, may be decided by those cases where from some accidental cause the support is withdrawn. One of the worst lacerations that ever came under my notice arose from the patient, an extremely excitable person, suddenly withdrawing herself during a violent pain; the perineum was deprived of its support, and was torn quite through. Every practitioner of experience can readily bring to his mind such cases.

Hence the support of the perineum is not alone for the purpose of allaying irritation and diminishing congestion, but also to counteract too violent distention from the action of the uterus. It is true that pressure so employed can never absolutely resist such powerful force, but it can control it, can moderate the power, and alter its direction.

Thus the pressure of the head forwards directs the force of the uterus towards the vulva and from the perineum; but in doing so, caution is necessary, lest the head be forced over the perineum before it is prepared. The rule

to direct the head forwards is of value when the principle is understood, but most mischievous when it is not. It is one thing to assist in directing the force of the uterus from the perineum towards the vulva, but a perfectly different procedure to endeavour to force the head over the perineum not prepared to yield. This latter practice is, I fear too frequently adopted, and causes many lacerations. Hence in my lectures I have stated that the object of supporting the perineum was to prevent its too violent distention by the action of the uterus, not for the purpose of forcing the head forwards.

The object of supporting the perineum seems to me to be twofold. First, to prevent or allay irritation and to diminish congestion, so that the act of dilatation may not be interfered with. Secondly, to counteract too violent action of the uterus.

The latter cause I have only referred to in my lectures, and the subject was left thus far incomplete; I have ventured, therefore, to trespass on your indulgence to allow me to supply the deficiency.

The only point on which I cannot agree with some of my colleagues on this practice, is the rule to press the head forwards over the perineum; I have omitted this because it is so constantly misunderstood and adopted as a means of concluding more rapidly a labour that may have been tedious. Forcing the head over a perineum unprepared is just as likely to cause its laceration as drawing it back, or leaving it to itself unsupported.—*British Med. Journal, April* 20, 1861, *p.* 426.

104.—ON RUPTURE OF THE PERINEUM DURING LABOUR.

By Dr. Thomas Skinner, Obstetric Physician to the Liverpool Dispensaries.

[In a case of severe rupture of the perineum, Dr. Skinner proceeded to remedy the laceration as follows:]

Within a few hours of the occurrence of the accident, and with the kind assistance of Mr. Patterson, I placed the ruptured parts together again with two deep "needle sutures," and two superficial interrupted ones, all of which were removed on the morning of the fifth day, when Mr. Patterson and I had the satisfaction of seeing the rupture healed in its entirety; and so perfect was the line of union, that it was scarcely possible on the tenth or twelfth day to trace the *locale* of the lesion by the cicatrix.

I have little hesitation in stating that the "needle suture," as invented or reintroduced by my friend Dr. Aveling of Sheffield, is much less irritating, better borne by the patient, and is in every respect much to be preferred to the silk quilled suture. The thickness of the suture which I used was sixteen on the Parisian wire gauge; they are manufactured by Messrs. Cocker Brothers of Sheffield, and sold by Mr. Henry Aitken, cutler, of York, who supplied me. I think it proper to add that I bent the wire before introducing the needle point, that I passed each end through a smooth flat splint of wood, that I fixed them by means of pierced duck shot, and then removed the extremities. I would further add, that the sutures ought to be applied only sufficiently tight to bring the deep parts of the laceration into apposition, greater tightness will only do harm, by cutting tissue and causing congestion and irritation; it is quite simple, if necessary, to retighten the sutures as the subsidence of the congested state of the perineal structures takes place.

Remarks.—Denman has said, that when women were *delivered without assistance*, he had not in any case observed considerable laceration (5th edit. p. 79.) If this case teaches anything, it teaches most clearly that the whole tissues of the perineum may be ruptured by the unaided efforts of nature, without even *voluntary* attempts at bearing down on the part of the mother. I am told by the patient that she tried rather to restrain herself than otherwise. The mechanism of the rupture in this case seems to have been too great rapidity of the second stage; the perineum having had no time to be safely dilated; and the fact of the fœtal head being rather within the average

size, twelve inches in circumference, the average circumference being fourteen inches. This case also shows that very severe ruptures of the perineum are much better repaired at the time than left to themselves or to the alternative of rawing the edges on a future occasion, not to speak of the chances and evils of rectocele and prolapsus uteri in the interval.

I would further observe, that so far as the *minor* lacerations of the perineum are concerned, I am not disposed to believe in the unaided restorative powers of nature. I have frequently looked for it, but in vain. Contraction of the swollen and dilated parts, and of the edges of the wound, undoubtedly do take place; but coaptation and adhesion of the lacerated structures I have rarely or never seen to my satisfaction. So thoroughly am I convinced of this, that, for several years back, I have never trusted to nature alone, but have always introduced a twisted or quilled suture, or two, as the case may have required. I can safely say that I have never regretted doing so; on the contrary, I have always been extremely satisfied with the result; and I am strongly of opinion that, if an examination of the condition of the perineum was instituted, *post partum*, as frequently as the same structure is vainly supported during labour, and treated as I have stated, my friends Dr. Keiller of Edinburgh and Mr. Baker Brown of London would have fewer operations to record for rectocele and prolapsus uteri. When we consider that an incalculable amount of the future physical comfort of the patient, and no small portion of the pleasure and happiness of matrimonial life, depend upon the strict integrity of the structures of the perineum, I really think that we are guilty of culpable neglect, or of ignorance, to say the least, in leaving the rupture to nature, or, what is very much the same thing, *leaving it unobserved.*—*Brit. Med. Journal*, *Aug.* 17, 1861, *p.* 170.

105.—IS THE ERGOT OF RYE, WHEN ADMINISTERED TO THE MOTHER DURING LABOUR, DANGEROUS OR NOT TO THE LIFE OF THE CHILD?

By Dr. Uvedale West, Vice President of the Obstetrical Society of London.

In Dec. 1855, the author published the particulars and results of an inquiry similar to the present—to the effect that, in a series of 69 cases in which he had administered the ergot of rye on a gross total of 278 labours, 9 children were still-born—viz., 2 putrid at birth; 2 born after labours which were preceded and accompanied by hemorrhage; 1 born footling, with hydrocephalic head, and consequent fatal compression of funis; 1 in which there was evidence of latent compression of funis; 1 a difficult primiparous forceps delivery; 1 a difficult vectis delivery, the mother seriously ill from excessive œdema; and 1 born dead without any assignable cause.

In consequence of certain criticisms on this paper which were published in France, accounting variously for the results, and challenging the author to continue the inquiry on the same plan, one of the critics, Dr. Danyau, on the part of the Imperial Academy of Medicine, maintaining that, unfavourable though the results appeared to be to him, yet a similar report on a like number of cases would probably be more unfavourable still, Dr. West continued to tabulate his ergot cases as he had begun, with the results which he now begged to lay before the Society:—

Between Dec. 23rd, 1855, and June 22nd in the present year, on a gross total of 734 labours attended by the author, the ergot was given in 172 cases, including one case of twins, so that there were 173 children born under the effects of ergot of rye. Of these, only 5 were still-born from all causes—viz., 3 putrid at birth; 1 with placenta prævia and profuse hemorrhage—premature; and 1 with prolapsed funis, detected an hour after the ergot was given, the operation of turning, which was then immediately performed, having been too late to save the child's life. The author urged that, in reply to Dr. Danyau's precise challenge, he might fairly have rested contented with the results of the first 69 cases of the second series, in which there was not a single still-

birth from any cause; but he considered that that fact, when compared with the unfavourable result of 9 still-births in the preceding series of 69 ergot cases, proved only that 69 was too small a number to form a correct conclusion from, and therefore he went on with the inquiry until it was spread over a gross total of 1013 labours, on which number the ergot was given in 241 cases. Of that number of 241 ergot cases, including 242 children born, there were, adding the 9 of the first series to the 5 of the second, 14 still-births from all causes—viz., 5 putrid at birth; 1 footling; 1 prolapsed funis; 3 hemorrhage during labour from placenta prævia; 2 difficult instrumental deliveries; 1 suspected latent compression of funis; 1, cause not manifest. On the whole number of births—1013 labours, and 1029 children born, including the series without ergot as well as that with ergot—there were 50 still-births; of which 5 were born dead without any manifest cause. So that, inasmuch as only 1 of those 5 was born under the influence of ergot, that medium having been given in the greater proportion of 1 in 4¼ of the whole number of labours, the author thought there was no sufficient evidence in the facts he had accumulated to justify the doctrine that ergot of rye was dangerous to the life of the child.

As to the mother, the author found the following results on a retrospect of the gross number of 1013 labours—viz., 7 deaths within the lying-in month, of which 1 occurred with a patient who had had ergot of rye; 18 cases of incarcerated placenta, of which 5 were after ergot of rye; 25 instances of post-partum hemorrhage, of which 5 were after ergot of rye; 30 cases of puerperal disease or disorder, of which 9 were after ergot of rye. So that he was led to the conclusion that ergot has little or no influence in either causing or preventing parturient or puerperal accidents or diseases. At the same time he wished to remark, that probably in proportion as ergot might improve uterine action, certain accidents which depended on deficient uterine action, such as post-partum hemorrhage and after-pains, might be controlled by ergot of rye.

The author concluded his paper with the observation that the tables before the society appeared to prove that it was immaterial in what stage of the labour the medicine was given; whether the os uteri was rigid or supple; whether the liquor amnii was or was not evacuated; or whether the mother was multiparous or primiparous; but that it was essential that actual labour should be present, as well as that the accoucheur should be competent to meet any emergency that might arise, just the same as when ergot has not been given; that ergot was useful wherever it was desirable to improve uterine action; and that it could be dangerous only where uterine action would be dangerous, as, for example, in a case of arm presentation after the liquor amnii was evacuated, as then it would make turning more difficult.—*Lancet*, *July* 20, 1861, *p.* 62.

106.—ON THE USE OF THE FORCEPS IN TEDIOUS LABOUR.

By Dr. G. Hamilton, Falkirk.

[The use of the forceps in labour is on the increase. If skilfully used, and at the proper time, there is no doubt a great deal of time and suffering are saved, with comparative safety to both mother and child. In the early stage of labour, before the dilation of the os uteri, we can do nothing but wait patiently.]

When, however, the os uteri has become dilated, and the head has entered the pelvis, so that an ear can be easily felt, I hold that the danger to the child usually becomes imminent if it be allowed to remain undelivered much more than two hours, especially where the pains are smart, or the compression is considerable. When mother and child have been much exhausted by a protracted or very severe labour, my rule is, when an ear comes within reach, not to wait more than a quarter or half an hour, or even, if the case is urgent, to deliver immediately. I always feel anxious in such instances as I have last mentioned to get quickly over the latter half of labour; for

though no serious harm may yet have happened to mother or child, there can be no question that, to the latter at least, much additional delay would be fatal. If, therefore, nature is getting over this portion of the labour quickly, I am content; if not, I use the forceps, and deliver.

It is always an essential preliminary in applying the forceps to ascertain exactly the position of the head, but practitioners, I think, will allow that it is sometimes not easy at once to settle this satisfactorily. If the tip of an ear can be felt, and I have still any doubt on the point, I fix the forceps over the ears and exert only traction, until the head has somewhat advanced, when I remove the instruments and satisfy myself of the exact position. I have said in my former papers that when this has been done it is most important, in a large proportion of the cases, gently to turn the head round at the same time that traction is being applied, so as to bring the face into the hollow of the sacrum. Sometimes this can be best done by making it turn towards the right side, while in other instances the left will be found the easier. Nature, if left to herself, usually, I imagine, takes the shortest course. When the practitioner interferes, perhaps the invariable rule ought to be at first to endeavour to turn to the right side, when the face is towards that side, opposite the symphisis pubis, or even slightly to its left, as the rectum on the left side diminishes the capacity of the pelvis, and offers an obstacle to the free version of the head. When the face is opposite the left acetabulum, or the left sacro-iliac synchondrosis, I usually try first to turn by the left into the hollow of the sacrum; but, should this fail, I think that the accoucheur has not exhausted the resources of his art until he has endeavoured to turn the face round towards the *right* side, for delivery will sometimes be found practicable by this circuitous course when the direct one by the left is impassible. I suspect that this position, with the face to the left, from the obstruction referred to, is one that accoucheurs have often found among the most formidable they have had to encounter. Mere traction is of little avail, for nature, as I have remarked, will take the short route, and the obstacles there may be insurmountable. It is only within these few years, I confess, that I have become fully impressed with the importance of this line of practice, and the extent to which version of the head can be carried. As the circumstances connected with this are very instructive, they will be, I hope, a good apology for my giving a few details.

I was requested, at 8 p.m., in July 1857, by a brother practitioner, to see, along with him, a primipara in labour. I found that the woman had been in labour about eighteen hours, and that, five hours previously, my friend had attempted to deliver with the forceps, but had been unable to do so. He therefore wished me to apply them. Upon examination, I found that the vertex presented, and that an ear could with difficulty be felt near the right acetabulum, with the face towards the left side. I had no difficulty in applying the forceps over the ears, and succeeded after a while in advancing the head a little, at the same time that it was easily made to rotate so as to move the ear felt from the right side to opposite the left acetabulum. Here the opposition of the rectum was encountered, so that though, while the forceps were being used, the face felt constantly on the point of passing into the hollow of the sacrum, whenever these were removed, the ear had a tendency to come again nearly opposite the symphisis, the head of course assuming a nearly transverse position. As the most persevering efforts with the forceps, as well as excellent pains, had failed at 6 a.m. next morning materially to advance the head, it became imperative, for the sake of the mother, that delivery should be effected. Accordingly, the head was opened, and delivery accomplished without any delay, the mother afterwards making a rather lingering recovery.

Two circumstances struck me in this case,—first, the ease with which the head could be made to rotate, so as to bring the face opposite the left sacro-iliac synchondrosis, and the yet impossibility of carrying it into the hollow of the sacrum, or of bringing it nearer the outlet in any material degree; and, secondly, the very small quantity of brain which required to be abstracted in order to make the passage of the head quite easy. Reflecting upon

these circumstances, I had a strong impression that, had the face in this instance been towards the right instead of the left side, or could it have been turned to that side, the child might have been saved.

On Sept. 20th following, a case very similar in position and circumstances to the one mentioned occurred to me. The woman had previously had four children, all of whom had been delivered with the forceps—the two last by myself, without any great difficulty. The vertex presented, with an ear opposite the symphisis pubis and the face to the left side. The head having continued in this position, without making a sufficient advance, as long as I thought it prudent to allow it, I applied the forceps over the ears, and again easily caused the head to advance slightly, and the face to come opposite the left synchondrosis, but again also I found that no further progress could be effected. Instead, therefore, of persisting in attempts to force the face to pass over the rectum, I resolved to try whether I could make it sweep round the pelvis, so as to bring it opposite the *right* synchondrosis. With the aid of the forceps first, and afterwards of my hand, I found no great difficulty in pushing up and rotating the head, so as in a short time to effect this. Having thus got rid of the opposing rectum, the forceps were again applied, and the child safely delivered without much difficulty.

This operation will of course generally be easier when the face is towards the symphisis pubis, but that this is not invariably the following case will show. It was my 732nd case, and broke the long continuity of successful deliveries, and I therefore give its details the more willingly, in order to show the formidable difficulties which had to be encountered. The patient was a farmer's wife, with a remarkably hard muscular development, who on two former occasions had had abortions about the fifth month. I saw her about six a.m., and for some hours the labour went on slowly but favourably. In the course of the forenoon, however, she was suddenly seized with a convulsive fit, not very severe, but which still somewhat affected her consciousness when she came out of it. The convulsions continued to recur every hour or so until she was delivered about 3 p.m. As she was tolerably easy during the intervals, I allowed the labour at first to proceed without attempting to interfere, the advance then made being such as would have rendered the application of the forceps very difficult. Afterwards, when the os uteri had become perfectly dilated, and an ear could be just felt, I applied them, and succeeded, after considerable exertion, in bringing the head so far within reach as to enable me to satisfy myself that the face was nearly opposite the symphisis pubis. Simple traction I now found did no good. I therefore endeavoured to turn the face to the right; but, after persevering efforts both with hand and forceps, I could produce no good result. As a last resource with the forceps, I then tried version to the left, using traction along with it, and, to my surprise and delight found that the face slipped without much difficulty into the hollow of the sacrum. The delivery was then easily effected. The child, however, was still-born. The mother's recovery was remarkably favourable, considering the severity of the labour, but a purulent vaginal discharge continued for some months after the labour. This, I may remark, is the only instance in all the series of cases I have referred to in which I am aware of such an occurrence having taken place, or where, indeed, any local injury of the slightest consequence was sustained.

It is always desirable that the head should have emerged from the uterus before the forceps are applied, and determined efforts should be made with the finger or hand to effect this. Nevertheless, imminent danger to child or mother, or to both, sometimes requires that the instruments should be used before the head has passed the os uteri. The last case was an example of this kind, when I first applied the forceps. My impression is, after frequently having had to use them under these circumstances, that where caution is employed, the risk is not great. The only case, indeed, I have ever had which could make me doubt of its safety, was one which occurred to me very lately. In a primipara, when called, I found the os uteri well dilated, and about a foot of the funis prolapsed, the arteries, however, beating strongly. I tried to replace it, but not being able to do so, I immediately applied the

forceps, and delivered with great ease, and so quickly as to enable the child to survive. Mother and child apparently did well for several days, and I had ceased seeing them, when I was sent for to the mother, who had been seized with diarrhœa. Though this was controlled, a remarkable degree of prostration accompanied it, some abdominal tenderness came on, and she sank on the tenth day after delivery. Although I have thought it right to mention this case, as puerperal fever was occurring about the same time in the district, it seems to me impossible to say whether, or how far, the instrumental interference was connected with the fatal result.—*Edinburgh Medical Journal, Oct.* 1861, *p.* 315.

107.—ON A MODIFIED FORM OF OBSTETRIC FORCEPS,

TO BE USED IN CASES WHERE THE ORDINARY ONES ARE INAPPLICABLE, OWING TO THE UNUSUAL ELONGATION OF THE FŒTAL HEAD.

By Dr. Graily Hewitt, Physician to the British Lying-In Hospital, &c.

[When the fœtal head is large, and the passages not over wide and dilatable, the labour is much prolonged, and the head of the fœtus becomes excessively elongated. The ordinary obstetric forceps are not applicable to such a form of head, or only with great difficulty, the blades being too short and too sharply curved to embrace the head.]

The occipito-mental diameter of the head, with which we are chiefly concerned in reference to the applicability of the forceps, measures, according to various obstetric authorities, from 4½ in. to 5¼ in. Van Pelt, who has recently published the results of his measurements in 700 cases, found the average to be 5 1/16 in. These measurements were made, not immediately after the escape of the head, but when sufficient opportunity had been given to the head to recover from the effects of compression during the parturitive act. No observations had hitherto been made for the purpose of ascertaining the difference between the measurement of the occipito-mental diameter during parturition and at a period a few hours after the head had been born. The difference in question was known, however, practically to be considerable.

The relations subsisting between the curve and length of the blades in the ordinary forceps and the shape and curve of the head in ordinary cases and in cases of undue elongation were next alluded to, and illustrated by means of drawings. Thus it was made evident—1. That the ovoid presented by the head (as it is in ordinary cases) is such that the ordinary forceps, with a length of blade of from 6½ in. to 7 in., and a curve the arc of a circle from 10 in. to 11 in., is admirably adapted to it. 2. That when the ovoid exceeds 5¼ in. in its long diameter, the forceps in question is likely to be inapplicable, and, as in the case alluded to, difficulty might arise in endeavouring to apply it. How often would such exceptional elongation be observed in practice? In Van Pelt's cases there were 28 out of 646 in which, some little time *after* the end of the labour, the long diameter measured 5¼ in. Hence in about 3 per cent. of all cases we may expect to find the head so long as to give rise to the difficulty in question, and this not taking into consideration cases in which there may be undue elongation, and a less degree of difficulty in cases where the head is of medium size, but yet unusually elongated. The author believed that the difficulty alluded to had been (how frequently it was impossible to say) encountered in practice by others, though not recognised. To those who would object that, admitting the possibility of the difficulty occurring, it must yet occur very rarely, he would reply that, admitting the rarity of the occurrence in question, it was extremely desirable that it should be recognised, and that this or any other misconception likely to arise in connexion with the use of so important an instrument as the forceps should be removed. The only modification in the forceps hitherto admitted or insisted upon had reference to the *position* of the head in the parturient passage. No modification having reference to the *shape* of the head had as yet been insisted on. The latter principle was the one he wished to see adopted. The author exhibited to the society an instrument constructed under his directions, the

blade of which was 8 in. long (instead of 6½ in. or 7 in.), the curve that of a circle of 14 in. (instead of 10 in. or 11 in.), and identical in other respects with the ordinary short forceps used in this country. The curve more nearly approached that of the French instrument. The instrument thus modified would, he believed, be found applicable in the class of cases now under consideration—those in which the fœtal head is unduly lengthened. The cases requiring it would be easily recognised; the blade of the forceps used as a probe would inform the operator whether the shape of the head required one or the other instrument. The ordinary instrument used in this country was perfectly adapted to all ordinary cases, and it was only a further development of the principle on which that instrument was constructed to insist on the necessity for occasionally employing a forceps of which the blades were by their length and shape suited to the altered length and shape of the fœtal head. In the long, tedious labours of primiparæ the new instrument would be most likely to be called into requisition.

[In the discussion which followed the reading of Dr. Hewitt's paper before the Obstetrical Society of London]

Dr. BARNES said he had encountered the practical difficulty signalized by Dr. Graily Hewitt, but had not met it in the same way. At one time he was in the habit of using the Dublin straight forceps, which is somewhat longer than the one described by Dr. Hewitt as being in ordinary use in England. This instrument he had now abandoned; it was apt to split the perineum. This objection he thought applied to all straight short forceps. To introduce this instrument it was necessary to press the handles far back, stretching the perineum backwards, and then during extraction, as the bow of the blades sprang directly from the handles, a great strain upon the perineum was unavoidable. For some years past he had used exclusively the forceps of Mr. Roberton of Manchester. Being called upon frequently to apply the forceps, he was able to say that this instrument was admirably adapted to all the ordinary emergencies, whether the head lay at the brim, in the cavity, or near the outlet. The pelvic curve, the length of the blades, and the length of the shank, kept the perineum free from any strain whatever beyond what was incidental to the passage of the head itself. The instrument added nothing to the danger to the soft parts. He wished to add that it was not a fair representation of the application of the forceps to show it as embracing the head with perfect symmetry. The grasp was oblique, one blade lying on the right brow or temple, the other on the occiput or behind the line of the transverse diameter. This was the necessary consequence of the circumstance that the long forceps was introduced in the transverse diameter of the pelvis, and not with reference to the child's head. This he thought an important rule to follow. In some cases, with the head low in the pelvis, it might be possible to apply the short forceps with a blade on either parietal bone; but it was a bad practice. The pelvis should be the guide for the position of the forceps, not the head.

[Dr. BRAXTON HICKS observed that the short straight forceps could not be passed forward enough to lock, and by employing the long curved forceps this difficulty was overcome.—*Lancet, June* 15, 1861, *p.* 590.

108.—SOME PRACTICAL OBSERVATIONS ON THE TIMELY USE OF THE OBSTETRIC FORCEPS.

By Dr. EDWARD B. SINCLAIR, A.M., Fellow and Censor of the King and Queen's College of Physicians in Ireland; Ex-Assistant Physician to the Dublin Lying-in Hospital, &c.

It has been said by a high authority, that "in every case of midwifery, the chief object to be attained by the practitioner should be the preservation of the *lives* of both mother and child committed to his care;" and this has formed the basis of treatment in all cases of labour. Now though I thoroughly concur in the above rule, yet I do not consider it without fault; it does not go far enough in my opinion. I conceive its great author would have been

more judicious, had he defined the chief object of the obstetric practitioner to be, the preservation of *the structures of both mother and child, and the prevention of fever and inflammation* during and after parturition. Acting on this dictum it would be found, that the lives committed to his charge, especially that of the mother, would seldom be placed in jeopardy. The former dictum has given rise to what may be called the expectant line of practice in obstetrics, which, however judiciously employed, has generally speaking, been carried too far. Too great a reliance on the powers of nature, albeit the lives of mother and child may have been preserved *at the time*, has often given rise to occurrences which have rendered the post-partum state not only dangerous and protracted, but fatal; or what is as bad, has caused the life of the woman, so saved, to be a burden to her. Therefore let us look, not to the preservation of life merely during parturition, when conducting a labour, but to the conservation of the tissues and the prevention of inflammation. Then life will follow as a matter of course, and that unclouded by any permanent defect.

For many years a portion of the Dublin school of midwifery, in conjunction with some members of that of England, and also of Scotland, have manfully struggled against what I fear I must designate, the narrow prejudice of some of the heads of the profession, in endeavouring to establish a more rational practice with respect to the application of the forceps. In our school I could mention a goodly list of gladiators, and amongst them would stand conspicuous those of the elder Beatty, his son, the present eminent obstetric practitioner, Evory, Kennedy, Shekleton, and Churchill; several equally eminent gentlemen from the sister countries I could also enumerate, did space permit. But it is no matter of surprise to me that practitioners should have been guilty of errors of omission with regard to the application of this instrument, when I review the rules laid down for their guidance. Some of these I shall take the liberty of quoting; premising, that so highly did their authors stand in the estimation of the profession, that their word was considered almost a law. And it must be also observed that their dicta, as will appear, were uttered with the greatest solemnity and precision; yet, notwithstanding, they are frequently found with equal solemnity contradicting themselves.

Dr. Collins, one of the former Masters of the Dublin Lying-in Hospital, thus writes:—"Let it be carefully recollected that, so long as the head advances, *ever so slowly*, the patient's pulse continues good, the abdomen free from pain on pressure, and there is no obstruction to the removal of the urine, interference" (that is, of any kind)"should not be attempted, unless the child be dead." Now, from this we must infer that the state which is the converse of the above, viz., where, amongst other matters, the symptoms of inflammation, indicated by quick pulse and tenderness on pressure, *are* present, we should use the forceps. But, in all likelihood, the life of the child shall then, indeed, be found extinct; so that this rule would ignore the forceps altogether, and turn cases which might have terminated happily into craniotomies. Again, prior to interference with the forceps, "the head of the child shall have rested for six hours as low as the perineum, though the pains have already ceased." Therefore, if the pains continue so that the presentation advances ever so slowly, we should not use the forceps; and though the pains or uterine action may have ceased for more than five hours, the head making no advance whatever, the practitioner is not to interfere with this instrument.

Dr. Murphy, late Assistant-Physician to the Dublin Lying-in Hospital, and at present Professor of Obstetric Medicine in the University of London, gives very nearly the same advice. "It appears to me," he says, "that four hours would be quite sufficient to allow the head to remain in the same position to authorize your interference." These two last recommendations show that the eminent gentlemen who put them forward considered the question merely one of time, apparently forgetting that the powers of endurance are not equal with all women—that some could bear with impunity, for many hours, what would kill others to endure for one. How uncertain, how unsafe, then, are such precepts!

The rules of practice above quoted evidently originated from the axiom of Denman (who may justly be called the father of British obstetric medicine), as follows:—"It is meant, when the forceps are used, to supply with them the insufficiency or want of labour pains; but *so long as the pains continue*, we have *reason to hope* they will produce their effect, *and shall be justified in waiting*."

Again, it has been taught that "when the pulse becomes quick, the tongue dry, the discharges foul, and the mother's parts hot and swollen," the time has arrived for the application of the forceps. In other words, this teaching directs that the forceps, which I assert should be used for the purpose of *preventing* the occurrence of inflammation, ought not to be had recourse to till *that inflammation has set in*. Dr. Rambotham's teaching goes far beyond this, and tends to ignore altogether the effectual application of the forceps. He tells us, "if the pains are subsiding gradually, or have entirely disappeared; if the strength is failing, the spirit sinking, the countenance becoming anxious; if the pulse be 120, 130, 140, in the minute; the tongue coated with white slime, or dry, brown, and ropy; if there have been two or three rigors; if, on pressing the abdomen, there is *great* tenderness of the uterus; if there be green discharge; if there be preternatural soreness of the vulva, with heat and tumefaction of the vagina; if the *head have been locked for four hours, and made no progress for six or eight hours; if the patient be vomiting a dark, coffee-ground-like matter; if there be hurried breathing, delirium, or coldness of the extremities, then* we are warranted in having recourse to the forceps, although the labour has not lasted the limited period of twenty-four hours, or even twelve:" he adds, however, "and we should be acting injudiciously (!) to allow the case to proceed until the last four symptoms appear without relief being afforded."

Omitting, then, the last four symptoms, it seems to me, as I dare say it has appeared to many a junior practitioner, that, according to Dr. Ramsbotham, not until the parturient woman exhibits to her attendant impending dissolution, should he use the forceps; but that he is not to wait for the occurrence of the last four symptoms, indicating that she is absolutely in *articulo mortis*. Still more adding to the perplexity of those who may consult this author for information as to the period when the forceps is applicable, we find him, in another place, telling us emphatically to use it without loss of time; for, says he, "*bear in mind* that more injury may accrue from too long delay than arises from premature assistance!" Strange doctrine after his prior advice! I could multiply quotations, did space permit, tending to show the uncertainty of the teaching of authors on this question; and the perplexity and fear that must, consequently, have arisen amongst the mass of practitioners, concerning the operation of delivery with the forceps. Suffice it to add one more.

Dr. William Osborne, in his 'Essays,' informs us—"All the powers of life are exhausted, all capacity for further exertion at an end, and the mind as much depressed as the body; they would at length sink together under the influence of such continued but unavailing struggles, unless rescued from it by means of art:"—that means being the forceps. How truly does Dr. Burns say "that the disciples of the school of patience carry their fears of the mischief resulting from the use of the forceps to an extravagant length, and place a mistaken confidence in the efficacy and safety of a continued action of the expulsive powers;" and how correct is the observation of our latest authority on this operation, Dr. Churchill, who writes as follows:—"It must be admitted that, until lately, we have been too much afraid of the forceps, and have allowed cases to become subjects of craniotomy, which, at an earlier period, might have been safely delivered with the forceps."

It would, indeed, appear that until very lately the general tendency of obstetric teachers, especially in this city, was not to teach the use of the forceps, but its abuse. With few exceptions, they shrouded this instrument with such suspicion and horror, that practitioners were deterred from availing themselves of it. The difficulties and dangers attending the use of the instrument were ever brought prominently forward, to an exagger-

ated degree; while the facility of its application and the safety of its action were kept completely in the back-ground. I have no doubt, entertaining as I do the highest respect for the eminent physicians, that those who thus taught in this city were actuated by the purest motives—that they thus taught with a laudable desire of preventing as much as possible the origin of a system of "meddlesome midwifery." But it is much to be feared, that, running to extremes with their views, a great loss of life and comfort was the result; not so evident, however, in private practice, as in hospitals and charitable lying-in institutions, where the physician meets a greater amount of ill-health and general constitutional delicacy. The rule till recently was, to postpone the operation as long as possible—not to act till those symptoms occurred, the advent of which should have been rendered impossible. Life, certainly, was often preserved; but the operation was performed too late to hinder subsequent inflammatory action of the mother's tissues, and all the dangers attendant thereon. I well remember listening to Dr. Churchill's eminent predecessor, picturing in his usual powerful language the dangers and difficulties of forceps delivery. So terrible a picture did he pourtray, that just after having heard him lecture on the subject, I would rather have attempted to tie the abdominal aorta than use the midwifery forceps. But a short time since all lacerations, fistulæ, and cicatrices were laid at the door of instruments; while the fact of the head having continued to press for a long time in one fixed situation was never taken into account. It was thus that, when a vesico-vaginal fistula arrived in this city from the country, we have so often heard the following story:—The patient was a long time in labour; a dispensary doctor was called in, and he used the forceps. Well, here was a forceps delivery, and the woman has got a vesico-vaginal fistula; so the forceps caused the inflammation, which resulted in sloughing and fistula. But the long labour was not taken into consideration. That the results of inflammation from prolonged pressure of the head in one position is sometimes unjustly attributed to the use of instruments, I shall mention the following circumstance to illustrate; and though this anecdote relates to the perforator, still, on consideration of the facts, it will be found more applicable than if it referred to the forceps.

A little better than two years since, I was requested by the medical attendant of a large public institution to visit a woman there in labour. Upon inquiry, I found she had suffered strong uterine action for more than thirty hours. When I saw her, the tongue was dry and brown, the pulse quick: there was much thirst: the parts were hot, swollen, and tender; a fetid olive-coloured discharge flowed from the vulva; the perineum was partially distended by a large scalp tumour, and was absolutely at the moment in a state of inflammation. The head of the child was completely impacted —not even a flat catheter could be introduced into the bladder (which fortunately was not very full), neither was there room to have insinuated its beak at any point between the fœtal head and the pelvis. The fœtal heart could not be heard. Of course I perforated at once, and as soon as I had evacuated the contents of the cranium as completely as possible with the crotchet, I removed that instrument, and by means of my fingers hooked through the cranial opening, with the utmost possible ease I completed the delivery, which did not take twenty minutes from first to last. I left directions, amongst others, for treating the inflamed parts, and put the nurse on the *qui vive* for sloughing. The gentleman for whom I acted took up the case, and the next day I heard the poor woman was going on as well as could be expected. I may here remark, she ultimately recovered, without any serious inconvenience. Some days, however, after this operation, I met a gentleman who occupies an important post in one of our large medical hospitals; he laughingly informed me, in a manner not to be mistaken, that he had seen a patient of mine with "*sloughing after instrumental delivery.*" This gentleman had been asked, by the one for whom I had previously acted, to take his duty for a day or two, soon after I had operated; saw this midwifery case amongst others; heard that I had used instruments—and at once

connected the sloughing with the instruments; never considering that even *though instruments had been used, the sloughing might have been caused by long pressure of the head.* I do not think I ever convinced him that I was not to blame for the results. Yet here *no instrument had touched the mother's parts;* and the head after it had been lessened, passed through them so readily as to require the hooked finger only to extract it. I could relate many such anecdotes as this. Now, if persons would only consider the parts of the mother which generally come in contact with the forceps during delivery—if they recollected that the blades were adjusted in an obliquely lateral position; that they were more or less transverse, as regards the pelvic space of the mother; that they are very seldom put on antero-posteriorly, the only position in which they could exercise direct pressure on the urethra; could they be convinced that, when adjusted in this latter position, the site of the urethra would correspond to the fenestrum of the public blade; that the only portion of that blade which could ever come in contact with the urethral portion of the vagina, was the beak, and that this beak pressed it only during the application of the instrument and during extraction of the head: that its pressure was very transitory, and that this transitory pressure was exerted *all along the course of the urethra, and not at one point more than another*—they would not, I can scarcely think, charge the forceps so often with the crime of vesico-vaginal fistula. I firmly maintain, that in careful hands, no injury can be inflicted by this instrument either upon mother or child. But the subjects concerning which I wish chiefly to comment, in connection with this instrument, are the post-partum conditions of the lying-in woman.

Firstly, then, I contend that the early application of the forceps prevents inflammation of the mother's tissues and its results.

Secondly, that the free and timely use of this instrument is a powerful means for warding off disease and death, especially in public obstetric institutions.

With respect to the first assertion, I regret it is not in my power to offer any statistics relative to sloughing of the vagina from authors who are hostile to the free use of the forceps; and it is a curious fact that some of these very authors *have* published obstetric statistics, and yet are silent upon this subject; they give no account of their cases of sloughing. I can, however, produce the statistics of a gentleman upon this point, who at one period used the forceps most freely. And premising that the majority of the cases of sloughing occurred during a period when this physician was still prejudiced by old and respected opinions, I think it will be considered, when I have exhibited the figures, that not only is the forceps guiltless of the charges laid against it, but also that there was an extraordinary immunity from sloughing of the soft parts of the mother, during Dr. Shekleton's mastership of the Dublin Lying-In Hospital, and that in consequence of the free use of this instrument. I can answer for the truth of the statistics I produce; they have been published by myself and Dr. George Johnson, in our report of Dr. Shekleton's mastership, all the facts having been registered at the moment of their occurrence.

Out of 13,748 deliveries of every description (225 of which were delivered with the forceps), there occurred but twenty cases of sloughing (more or less) of the soft parts; seven of these happened in deformed pelves after perforation and crotchet delivery; five occurred in tedious labours, where no instruments had been used; two in prolapse of funis, and one in retained placenta; in neither of which the forceps was used, but they were subsequently seized with erysipelas, sloughing, and died—or, fifteen cases of sloughing, in which the forceps had not been used; leaving five cases only which occurred after forceps delivery—three of these latter were in single births, out of 200 such forceps cases, all slight, and after long labour: these recovered. There were two instances in twin-forceps labours, in both after long delay, and one of these died.

Out of 13,748 deliveries, there were only four examples of urethro-vaginal fistula after convalescence, two of which completely closed; one contracted

to a minute opening; and in one instance the woman died, subsequently, of peritonitis.

But had the forceps been used more freely, and much more early, in the first periods of Dr. Shekleton's mastership, I am almost certain that the five cases which occurred in tedious labour, when instruments had not been used, and the five which occurred under the head of forceps deliveries, would not have been recorded.

That the free and timely use of the forceps is a powerful means of warding off disease and death, especially in public obstetric institutions, I think I can produce strong reasons for maintaining.

Burns, when speaking of the effect of delay in labour, says, "There is a strong disposition given to puerperal diseases, not merely to those troublesome though less dangerous complaints known under the name of weids or irregular febrile paroxysms, but also to more formidable affections of an inflammatory nature, especially of the womb, and peritonitis; accordingly we find that a much larger proportion of women die after protracted than after natural labour." This has been corroborated in an elaborate dissertation by Dr. Simpson, of Edinburgh; and I believe no one will have the hardihood to contradict it. Lessen, then, the number of tedious cases, and it follows that the practitioner shall have a cleaner bill of health, and the minimum of mortality.

To illustrate this, I shall again consult the statistics of the Dublin Lying-In Hospital; and for the purpose of more fully proving my position, I shall divide Dr. Shekleton's mastership into three periods, namely, the 1st, during the months of Nov. and Dec., 1847; 2nd, during the years 1848 and 1849; and 3d, from Jan., 1850, to Nov. 1854. I have made this division, because during the first period the forceps was not used at all; during the second, prejudice was merely commencing to yield to common sense; and at the commencement of the five last years, the necessity of the early application of the forceps was fully appreciated.

Now, comparing the number of deaths with that of the deliveries and the ratio of forceps cases during each period, I find the following to be the result:—

During November and December, 1847, there were 242 deliveries, and as many as 17 deaths (to say nothing of cases of disease), that is, one in about every 14 women delivered died; there was no forceps delivery. During 1848-49 there were 3,886 deliveries, and but 70 deaths from all causes, or one in about 55 of those delivered; and the forceps, during this period, was used 38 times, or once in every 108th case. And during the years 1850 to November, 1854, there occurred 9,620 deliveries, of which but 78 died from all causes, or one in every 123, nearly 124; and the forceps was used 187 times, or once in about every 50th case. And were those deaths taken into account only which had originated from puerperal causes, the absolute puerperal mortality would have been one in about 350 of those delivered.

Comparing the ratio of disease with that of forceps deliveries, and confining ourselves merely to such seizures as may come under the term puerperal fever, I find the following to be the results during these three epochs:—

During the two months of 1847 puerperal fever was rife, Dr. Shekleton having received charge of the hospital during an epidemic, and all the cases of illness might have been included under that head. This state of the house first induced Dr. Shekleton to lessen his second stages, and was the means of bringing forward the forceps. But during the years 1848-49, when the forceps was used once in every 108 cases, puerperal fever and its allies were in the ratio of one to every 54 of those delivered, the numbers being 72 cases of puerperal fever out of 3,886 deliveries; whereas, during the last five years, when there were 9,620 deliveries, and the forceps was used once in in every 50th case, puerperal fever and its allied affections were as low as one in every 104 of those confined, the examples of these affections amounting only to 47.

As I have spoken of the uncertainty of the rules laid down for the guidance of the practitioner as to the application of the forceps, it may naturally

be expected that I should attempt to remove the difficulty; I cannot then do better than present the rules which guided us in our practice during the latter portion of Dr. Shekleton's mastership of the Dublin Lying-in Hospital. These were pretty nearly as follows:—

Time was never taken so much into consideration as the existing state of the woman's constitution, and that of the child's circulation. No system of axioms was laid down defining precisely time and circumstances. The *peculiar nature of each case* alone formed the basis or ground of action. But if we *had any axiom upon which we acted, it was this, and this only*—"*in doubtful cases, and where the application was not difficult, the error was on the right side to deliver with the forceps.*" We never waited for bad symptoms to set in; experience had taught us to discriminate the cases in which unpleasant symptoms *might be apprehended;* and if we did apprehend them, the labour was cut short without hesitation. If we found the fœtal heart about to fail after ergot, or otherwise, we used the forceps at once. We never perforated without first trying to extract the fœtus by the forceps; and we have successfully used it in slight narrowing of the pelvis. We never required to feel the child's ear, deeming it of no utility whatever; as I have elsewhere observed, it was not felt in the majority of our cases. The above practice is, to a certain extent, corroborated by Dr. Churchill, who, in the last edition of his 'Practical Midwifery,' remarks—"There are many cases in which, from the character of the labour, it may be certainly foreseen that *these symptoms* (alluding to those which practitioners were formerly in the habit of *waiting* for) will arise, and that the woman will not *previously* deliver herself. In all such cases, I would maintain that the forceps should be used as soon as we feel justified in coming to that conclusion." I may safely add, that acting thus, should we have been subsequently found to have erred, the mistake shall be on the right side. I emphatically assert, and I have had opportunities of forming an opinion, that no harm at least can result in thus acting; that much life will be saved, and much suffering spared to parturient women.—*Dublin Quarterly Journal, Aug.*, 1861, *p.* 60.

109.—ON THE OBLIQUITY OF THE FŒTAL HEAD IN THE MECHANISM OF PARTURITION.

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The object of this paper is to show that the obliquity, or lateral obliquity, of the fœtal head when passing through the brim of the pelvis, described by Nægele, by some of his predecessors, and by his followers down to the latest authors, does not exist in natural parturition; and that obliquity, or lateral obliquity, of the fœtal head when passing through the outlet of the pelvis, not described by Nægele and his followers, does occur in natural parturition.

This word, obliquity, and the alternative expression, lateral obliquity, are terms requiring, for most readers, some explanation; their meaning certainly not lying on the surface. The words ordinarily used to express the state, considered as a fœtal attitude, seem to me far more appropriate, and scarcely susceptible of being misunderstood—viz., lateral flexion.

I have for several years ceased to teach in my class-room the doctrine of Nægele on this subject, and it has long been my intention to publicly claim attention to my opinions, and thus to secure for them a wider scope, as well as careful criticism. And although it is now three years since Dr. Tyler Smith's 'Manual of Obstetrics' was published, it is the stimulus from a first perusal of that excellent compendium that has led to my taking up the pen. I need scarcely assure my professional brethren that it is with the greatest diffidence that I venture to adduce views opposed to those of Nægele, of Dubois, and of subsequent authors too numerous to mention. It is for the profession to decide whether truth lies with them or with me.

But I am not alone and unsupported in the views I entertain. Many authors, indeed, enter at length into descriptions of, and arguments for,

Nægele's opinions; but I know of none who has taken up the opposite side with any fulness. At the same time, I have, since writing this paper, found, on reference to the works of Velpeau, Caseaux, and of Dr. R. U. West, that these gentlemen have reached conclusions similar to my own regarding the direct entrance of the fœtal head into the pelvis.

Nægele's teaching is to be found in his original paper 'über den Mechanismus der Gebert,' published in 1819, in 'Mekel's Archiv für die Physiologie.' I shall quote from Dr. Rigby's translation of it, published in London ten years afterwards. Before doing so, I must premise that the quotations refer to the first or commonest position of the head in labour, and that in the whole of this paper I shall suppose this position to be understood. The doctrines apply to the other positions as well as to the first, and if proved or disproved for the one, stand or fall for the others.

"At the entrance to the pelvis (says Nægele), the head does not take a perpendicular, but a perfectly oblique direction, so that the part which lies lowest or deepest, is neither the vertex nor the sagittal suture, but the right parietal bone. The sagittal suture is much nearer to the promontorium of the sacrum than to the os pubis, and divides the os uteri, which projects backwards and generally somewhat to the left, across into two very unequal segments." (p. 13).

"The higher the head is . . . the more oblique is its direction; from which reason the right ear can generally be felt behind the pubis without difficulty, which would not be the case if the head had a perpendicular direction." (p. 16).

"On account of the oblique position of the head, the greatest width of the cranium (from one tuber parietale to the other), as well as that of its basis can never, during its passage coincide with the diameters of the pelvic entrance." (p. 19).

In further illustration of this doctrine, I shall quote the account of it in Tyler Smith's 'Manual,' the latest British systematic work on midwifery:—

"The right side of the cranium is considerably lower than the left, so that the most depending part of the cranial surface is the protuberance of the right parietal bone. This lateral depression is called the obliquity of the head." (p. 268).

"The movemeht of the fœtal head upon its occipito-frontal axis. . . . This movement causes one side of the fœtal head to become lower than the other during the whole progress of labour, after the head has entered the brim constituting the obliquity of the head." (p. 274).

If it be recollected that the axis of the child's body, the axis of the uterus, and the axis of the brim of the pelvis, are represented by the same line in the normal or standard condition, or very nearly so, then it is easy to see that this obliquity implies lateral flexion of the child's head, or the approximation of its left ear to its left shoulder. In respect of this lateral flexion, the child's attitude is thus represented as being changed from that maintained in utero before labour. The position of the child's head, as described in this changed attitude, is oblique; that is, the vertical axis of its head is said not to impinge upon the plane of the brim at right angles to it, but obliquely forming an acute angle looking forwards.

I do not believe that, in normal or standard conditions, any such lateral flexion or obliquity exists, but that Nægele and his followers are in error in this particular, and that the head enters the brim without any lateral flexion, and directly—that is, with its vertical axis at right angles to the plane of the brim.

I.—The first and chief reason for denying the existence of obliquity of the fœtal head at the brim of the pelvis, is, that it is not observed. I have been in the habit of carefully making out the position of the fœtal head in the brim of the pelvis at the beginning of labour: and, although I have observed that it varies to a slight extent in different cases, I have satisfied myself that it enters the brim, very generally, directly and not obliquely.

The obliquity which is described has never been seen. It is only a supposed result of the observation of many particulars in many cases; and the

observations are of a kind demanding great information and carefulness. The liability to be misled by preconceived theories is very great; and I cannot help thinking that the obliquity under discussion is the fruit of a mind powerfully impressed with the important part that obliquity undoubtedly plays in other departments of the mechanism of parturition.

I have not found the sagittal suture nearer the promontory of the sacrum than to the symphisis pubis. I have not found more of the right parietal bone approachable by the examining finger than would be so if the head entered the brim without obliquity. I have not found the parietal protuberance lying near the centre of the brim of the pelvis, nor approaching to it. I have not found the right ear of the child easily reached, while the vertex of the child was at or near the brim of the pelvis.

It is needless to pursue a career of assertions opposed to the statements of the believers in the obliquity. I shall proceed to evidence corroborative of the position I maintain. But before doing so, I shall simply mention that in cases which, for various reasons, I have introduced my hand, and felt the whole head as it lies in the brim, I have not found the obliquity under discussion. This is valuable evidence, especially in cases where there is no deformity of the head or structures in the neighborhood.

II.—The descriptions of the oblique position of the head given by Nægele and others are more truly applicable to its direct than to its supposed oblique position. This will be best shown by an examination of Nægele's remarks bearing on this matter.

"*The finger* (says he) *which is introduced in the direction of the central or middle line of the pelvic cavity, and brought in contact with the head, will touch the right parietal bone in the vicinity of its tuber.*" It is to be remarked, *firstly*, that the finger is not introduced in the central line of the pelvic cavity, but only in the direction of it; *secondly*, that it is not made evident that the head is to be touched or approached in the direction of the axis of that part of the pelvis where the head is lying—that is, in the axis of the brim. The finger may approach the head in the direction of the axis of the brim, but it is scarcely long enough to do so in the axis of the brim, or as nearly in it as, in the circumstances, can be guessed. The consideration of these points shows Nægele's statement to be so vague as to be without much value in this discussion. But it will be evident to all that the quotation cited is perfectly true, on the supposition that the head enters the brim directly, not obliquely.

To bear satisfactorily upon this question, Nægele's statement should have been to the effect, that the finger approaching the child's head in the axis of the brim, or where the imaginary axis of the brim passes through the surface of the vertex, touches the tuber of the parietal bone or its vicinity. Now, while I admit that the finger introduced in the direction of the axis of the brim, as may be done in vaginal examinations, touches the cranium near the tuber parietale, I assert that, on the other hand, the finger carefully introduced in the axis of the brim touches the cranium in or near the sagittal suture, and at a point in its length varying according to the degree of flexion of the head.

"*At the entrance of the pelvis* (says Nægele) *the head does not take a perpendicular, but a perfectly oblique direction, so that the part which lies lowest or deepest, is neither the vertex nor the sagittal suture, but the right parietal bone.*" Now, it is evident that, at the entrance of the pelvis, the head does not take a perpendicular, but a perfectly oblique direction. It does so because the whole child is lying obliquely; and to enter the brim of the pelvis directly—that is, in the direction of the axis of the brim—it must take a perfectly oblique direction. If it took a perpendicular direction—that is, a direction at right angles to the horizon—it would necessarily enter the pelvis with obliquity; but it takes an oblique direction in order to enter the opposed oblique brim of the pelvis directly, that is, in the direction of its own axis and of the axis of the brim.

Further, as Nægele says, the part which lies lowest or deepest is neither the vertex nor the sagittal suture, but the right parietal bone. All this is a

necessary consequence of the direct entrance of the head. No doubt it may also be a consequence of the oblique descent of the head, but its occurrence is no proof of the obliquity. The direction of the head entering the brim is nearly that of a line striking the horizon at an angle of 30 degrees. This is a very considerable obliquity to the horizon, but is perpendicular to the brim of the pelvis, which is inclined to the horizon at an angle of 60 degrees. To enter the brim of the pelvis obliquely, the child's head must advance horizontally, or in a line of direction striking the horizon at an angle less than 30 degrees. Nægele does not say that the part which lies lowest or deepest in the brim is the right parietal bone. If he intended to say that, I am at issue with him; and appeal to observation in support of my assertion, that both parietal bones enter and pass the brim simultaneously, both being in the plane of the brim at the same time.

"*The sagittal suture* (says Nægele) *is much nearer to the promontorium of the sacrum than to the os pubis, and divides the os uteri, which projects backwards, and generally somewhat to the left, across into two very unequal segments.*" The position of the sagittal suture in regard to the promontory of the sacrum cannot be discovered by an examining finger, the parts being too distant to be reached in that way. When the hand is introduced into the vagina to feel the whole relations of the parts before the fœtal head has passed the brim; the sagittal suture is not found nearer the promontorium than to the pubis symphisis. I am thus, at this point, quite at variance with Nægele, and I may venture to point out the cause of his error. It is my opinion that it has arisen from not making the observations relied upon, while the fœtal head was at the brim of the pelvis, and then only, for, after the head has passed the brim and entered the pelvic cavity, the sagittal suture is generally found nearer to the sacrum than to the pubis; and this is not very unfrequently observed even before the os uteri is much dilated, or the labour has been long continued. This approximation of the sagittal suture to the sacrum arises from the descent of the head in the axis of the brim, which coincides with the axis of nearly the whole upper half of the bony pelvis. This axis, when prolonged, strikes the sacrum at or near its point. The fœtal head has a tendency to advance in this axis, and does so till it is arrested by the posterior wall of the pelvis. While it is passing between the symphisis pubis and the two upper bones of the sacrum, it has the sagittal suture equidistant between them; but afterwards, and until the head begins to advance more or less forwards, the sagittal suture approaches to the sacrum, as it descends in an axis which leads it in that direction.

The last part of the quotation just given from Nægele has very little value, for two reasons,—viz., because the situation of the os uteri is far from being fixed or invariable, and because no observations have been made even as to what is its most ordinary position with minute exactness. Nægele himself mentions a frequent deviation to the left, which has not received much notice from subsequent authors. While the head is at the brim of the pelvis, it is difficult to examine satisfactorily the relations of the sagittal suture to the moderately dilated os uteri; and the greater facility of reaching and examining the right and anterior than the left and posterior parts of the head naturally leads, and I believe has led, to exaggeration of the extent of the anterior half over the posterior, and of the part corresponding to the circle of the os uteri. But it is out of my intention to pursue this particular point further, because its settlement would lead to nothing, and lead only to the further question of the exact position of the os uteri in early labour, for the ascertainment of which we have no satisfactory data.

"*The higher the head is* (says Nægele) *the nearer its long diameter corresponds to the lateral diameter of the pelvis, and the more oblique is its direction; from which reason the right ear can generally be felt behind the pubis without difficulty, which would not be the case if the head had a perpendicular direction.*" In regard to the increase of obliquity according to the height of the head, I have not one word to say, except that it is a mere statement on Nægele's part, unaccompanied by any corroborative evidence. As I do not believe in the obliquity at all, I can find no place for this refinement.

Nægele's assertion regarding the right ear is quite as much in accordance with the theory of the direct entrance, or entrance in a perpendicular to the brim, as with his own theory of obliquity. I must confess myself completely at a loss, however, as to the full bearings of his argument; for I know well that under no circumstances is the right ear felt behind the pubis without difficulty, and that its position when felt, and the forcing of the finger between the pubis and the head in order to reach it, indicate the direct, not the oblique, entrance of the fœtal head into the brim of the pelvis.

III.—The third reason for rejecting the theory of obliquity at the brim of the pelvis is based upon a careful study of the production of the caput succedaneum, and of the relations of this swelling to the presentation,—a subject in regard to which much has been assumed without evidence, or in defiance of it.

"*Under certain circumstances* (says Nægele), *a swelling of the integuments of the head frequently forms soon after the os uteri has begun to delate . , . This swelling is situated upon the right parietal bone, close to its upper edge, and equally distant from both angles: a small piece sometimes extends over the sagittal suture unto the other parietal bone; its circumference depends upon the degree of dilatation which the os uteri had attained.*" This statement of facts by Nægele may be added to, but cannot, I believe, be controverted. The caput succedaneum of the first stage of labour can be felt and seen to be as described in the passage. The statements in the quotation immediately preceding this last, and in others, is in quite a different position, involving points not of easy observation, and in regard to which there are manifest inducements to error. But, while I agree entirely with Nægele's statements in this last passage, I would make two additions thereto, of which only the second has an important bearing on the subject of this memoir.

1. It is a condition, necessary for the formation of a true caput succedaneum, that the liquor amnii be evacuated, or that it be in such minute quantity as to have no hydrodynamical properties. 2. While the extent of the caput succedaneum of the first stage may be limited to the right parietal bone, it is generally so limited only when it is not well developed. When it is well developed, it is found to extend over the upper part of the left as well as of the right parietal bone; but its greatest thickness is, as a rule, always in the portion overlying the right parietal bone. Nægele himself mentions the extension of the swelling over the left parietal as an occasional occurrence of which he gives no explanation.

Before further advancing, it is necessary to inquire what evidence is derivable from the caput succedaneum. In answer, it is certain that it only indicates what was the unsupported part of the head—in the present instance, what part lay over the os uteri. And as the position of the os uteri is uncertain, and denotes nothing exact topographically, so the position of the caput succedaneum will denote nothing exact topographically, or relative to the position of the fœtal head in the brim of the pelvis. For a fuller development of this subject I beg to refer the reader to my paper on this topic in the *Edinburgh Medical Journal* for July, 1860.

But, before leaving this point, I think it advisable to show that, supposing, as Nægele seems to do, that the os uteri occupies exactly the centre of the brim (except deviation to the left), and that the caput succedaneum formed on the part of the head lying over it marks the part lying in the centre or axis of the brim, the indications afforded by this swelling are not truly read off; and that, if truly read off, they indicate direct, not oblique, entrance of the head into the brim.

The caput succedaneum of the first stage of labour is often formed after the head has passed the brim of the pelvis, and is lodged in the upper half of the cavity of the bony pelvis. Were we to be cautious and exact in reasoning, all such swellings should be excluded from the argument, for evident reasons. It is only those formed at the plane of the brim, or very near it, that can, under any circumstances, afford assistance in settling this question. Under the actual deficiency of exact data, we must be content with stating principles. Now, it is evident that the direction of the caput succedaneum

of the first stage will be that of least resistance—that is, the direction of the axis of the undilated vagina; in other words, the caput will be thickest where the head is least supported, and may, in other parts within the circle of the os uteri, be so inconsiderable as not to attract notice. Further, and for the same reason, the centre of the caput succedaneum, or the centre of the os uteri, will not correspond with the thickest portion of the swelling, but in this case be behind it, or nearer the left parietal bone. The oblique direction downwards and forwards of the vagina will lead the caput in that direction; and the support given by the posterior wall of the vagina to the posterior half of the space enclosed in the circle of the os uteri will cause thickness of the swelling over the right, and comparative thinness over the left parietal bone, and displacement of the thickest portion of it forwards in the pelvis, that is, in the direction of the right parietal and away from the left parietal bone.

IV.—A very cogent argument against the existence of this lateral flexion of the child's head, or obliquity to the plane of the brim, at the commencement of labour, is derived from the impossibility of finding a mechanism to account for it.

If the membranes are still entire, and there is present any considerable quantity of liquor amnii, and if the axis of the child and uterus are parallel to or identical with the axis of the brim, all which conditions are usually found, it is impossible to conceive any cause of the obliquity but a spontaneous lateral flexion of the child's head; and I dare say no obstetrician will support so extraordinary a doctrine as that the child should, without any discoverable cause, and I may add without any desirable object, bend its head towards its left shoulder as it begins to pass the brim.

If the liquor amnii has been evacuated before the fœtal head has entered the brim, or if the liquor amnii be very scanty in amount, then forces produced by the pains, on the lower parts of the womb and adjacent structures, may be imagined to act directly upon the child, and cause the obliquity. But although they may be imagined, they do not, I believe, exist. If the uterus, for example, became, during pains, more nearly horizontal than it ordinarily is, or quite horizontal, then this grave anterior obliquity of the uterus would probably cause the head to present indirectly or obliquely at the brim. But the opposite of this is observed in nature. The uterus, during a pain, becomes, as it were, erect, and to a certain extent corrects any obliquity it may have during relaxation, becoming, when in contraction, perpendicular to the brim of the pelvis,—that is, occupying its axis. It is certain, then, that no anterior uterine obliquity is observed, which might account for the obliquity of the head at the brim. And it is necessary to remark that the obliquity of the brim, if supposed to be produced by anterior obliquity of the uterus, would not be accompanied by lateral flexion of the head as a change in the fœtal attitude. The flexion of the fœtal head which is so often observed in the early part of labour is easily accounted for by the circumstance that the fulcrum of the head—the spinal column—is nearer the occiput than the sinciput; and all forces acting equally on the various portions of the vertex, act with advantage on the sinciput, as it is at the end of a longer lever than the occiput. But in the case of the sides of the head, the right against the left tuber parietale, no such inequality is observed.

In short, no mechanism has been devised to account for the phenomenon, and it is a vain pursuit to seek it, at least on my part, as I deny the existence of what is to be accounted for.

V.—Assuming that the fœtal head enters the pelvic brim obliquely, Nægele claims for this condition a mechanical advantage over the direct entrance. "*On account*" says he, "*of the oblique position of the head, the greatest width of the cranium (from one tuber parietale to the other), as well as that of its basis can never during its passage coincide with the diameters of the pelvic entrance.*" (p. 19).

It is necessary, *in limine*, to state that discussion on this point, and conclusions in regard to it, can lend no aid to the settlement of the question under consideration in this paper. It is, indeed, quite a work of supererogation

to consider at all the advantages presented by an oblique or direct entrance of the head into the pelvic brim, until the previous question be settled, whether the entrance is direct or oblique.

The position apparently offering the greatest mechanical advantage is not always adopted by nature. Mechanical difficulties seem in various points to be sought, instead of mechanical advantages. The whole process of labour is, indeed, beset with difficulties, one of whose objects is, without doubt, to prevent its too easy and rapid accomplishment. One example, germain to the subject of this article, I may adduce from the entrance of the fœtal head into the pelvic brim. It is well known that its entrance considerably flexed has a great mechanical advantage over its entrance slightly flexed or not flexed at all; yet, in spite of this mechanical advantage on the side of the greater flexion of the head, we find that it generally passes the brim slightly flexed or not flexed at all.

Nægele places the mechanical advantages of the supposed obliquity entirely in the dimensions presented to the plane of brim by the tranverse diameters of the cranium and of its basis. In including the basis in his statements, he is decidedly wrong. He would indeed, appear to forget that the fœtus has a neck, the addition of which to the basis, even when the head is laterally flexed, makes the direct entrance of the basis mechanically the most advantageous, and that so evidently that it is really needless to do more than assert it.

There is no doubt that if the fœtal head passes the brim directly, the greatest biparietal diameter (from one tuber parietale to the other) passes it, and that if the head enters the brim with obliquity, a small biparietal diameter supplants the greatest,—that is, in the first position, a diameter measured from below the right tuber to above the left. But Nægele seems, for the moment, to have forgotten that the long diameter of the fœtal head is not in the tranverse diameter of the brim, but in an oblique diameter of this part, and that consequently (as well as for other reasons), the diameters of the fœtal head which he wishes to be compared are not the diameters it is necessary to compare, for they do not pass the smallest diameter of the brim. If mechanical advantage in the way of dimensions is to be of service in the mechanism, it must meet the difficulty,—that is, the gain in diminution of dimension must be in the part traversing the conjugate, or small diameter of the brim; and this is not true of the gain spoken of by Nægele. But, in truth, no gain is desiderated in any natural case, and when the comparison of the oblique and direct diameters of the part of the head traversing the conjugate diameter, as suggested by Nægele, is made so as to be true to the mechanical conditions, it is found that no appreciable gain is got from obliquity. This comparison is too difficult to give in words so as to be useful to a reader. It is necessary to institute it with the fœtal head and callipers in the hands in order to verify it.

There are, however, mechanical conditions of the laterally flexed head, or of the head presenting one parietal bone to the brim, which, if such a position were assumed, would lead to great and perhaps insurmountable difficulties in a labour. For, if the vertex was by any arrangement displaced from its position at the brim, as Nægele describes, it would tend always to be more and more displaced, till an ear, or even a shoulder, descended. And if the canal of the uterus were rigid and contracted enough to resist such unnatural dislocation, the uterine efforts would be directed along the body of the child to its head, at a great disadvantage. Other mechanical evils might be suggested; but it is in vain to raise difficulties, which, if the object of my memoir is gained, are all chimeras.

While I hold it proved that the child's head passes through the pelvic brim directly, I have, before leaving the subject, to point out that, after it has passed through the brim and upper half of the ligamentous pelvis, it does advance obliquely in its subsequent progress,—that is, the head of the child impinges on the planes of the parts of the pelvis through which it is passing, or on planes at right angles to the axis of the lower parts of the pelvis, not directly, but indirectly or obliquely. A mesial part of the fœtus does not first touch these planes, but a lateral point.

In the first half of the head's course through the ligamentous pelvis, a point in or near the sagittal suture is the presenting point. There the caput succedaneum is formed. During this part of its course the head advances in the axis of the brim, which almost exactly corresponds with the axis of the upper half of the ligamentous pelvis. During all this time, if the head is not covered by the cervix uteri, the right parietal bone is the part first and easiest felt; and the further it advances, the more is this the case. As it advances, and passes the first bone of the sacrum, the sagittal suture approaches nearer to the sacrum, or rather to its lower portions, and becomes more distant from the symphisis pubis. When the biparietal diameter of the head has reached the lower boundary of the upper half of the pelvis, it is arrested in its direct progress. The vertex impinges on the posterior wall of the pelvis, and, in its further advance, the head, as a whole, must change the direction of its course. This change of course is too abrupt for the parts of the head to follow it perfectly. Moreover, there is no room in the pelvis for such a degree of lateral flexion as this would imply,—that is, as would be necessary to maintain the head presenting directly to the plane of the pelvis through which it is passing. While advancing at this point of its progress, the presenting part, therefore, is changed. It soon becomes the upper and posterior part of the right parietal bone, instead of, as before, a point in the mesial line of the head. With this point advancing in the axis of the pelvis, it is evident that the sagittal suture or mesial parts are far removed from it, and consequently that the head is passing through the lower half of the pelvis, the outlet, over the perineum, and through the vulva, more or less obliquely, and not directly. In accordance with this obliquity, the child's head is flexed laterally, or, to be more exact, flexed obliquely,—that is, bent not directly over the right shoulder, but in a direction midway between extension and direct lateral flexion. As it approaches the orifice of the vulva, and rotates so as to bring the occiput nearer to the pubes than it was in the earlier parts of its progress, this flexion gradually approaches nearer to extension; but it does not become direct extension, almost always maintaining an obliquity,—that is, a direction between extension and flexion.

Under two sets of circumstances, not observed in ordinary labours, the presentation of the fœtal head may be direct from the beginning to the end of the process. In the rare cases where the head enters the brim and passes through the whole pelvis with its long axis in the antero-posterior diameter of the passage, the head will offer itself not obliquely, but directly, in its whole course. The presentation, indeed, will only shift backwards upon the child's head as it descends, maintaining always a position in the mesial line. Again, it is possible that the fœtal head may descend directly with its long axis in the transverse diameter of the pelvis, till it makes a complete quarter of a circle rotation, bringing it into a direct antero-posterior position. Such cases are not subjected to the ordinary laws of the mechanism of parturition.

It is to be remarked, then, that in the second half of its progress the head does not present directly, but obliquely, and that it is born with this obliquity. But this last obliquity is unlike the former, in being quite in accordance with Nægele's statement of the phenomena, though he omits to mention this special point, the obliquity which he describes in this portion of labour being the position of the long axis of the head in the right oblique diameter of the pelvis, not the oblique presentation of the head to the plane of the outlet, or other portions of the pelvis through which it passes in the latter parts of its course. Further, not only is there observed this obliquity to the planes of the pelvis, but there is a change in the attitude of the fœtus simultaneously produced. The head is at first, in this second part of its course, laterally flexed to the right with a backward obliquity; and when passing the vulva this is slightly changed, the condition being one of extension, with a lateral obliquity to the right shoulder.

This obliquity of the child's head to the planes of the lower parts of the pelvic passages is not only observed, but is easily explained. In its descent, the head, if of its ordinary size, must follow the direction of the curved axis of the pelvis. It is possible to imagine the presentation continuing direct

while the other parts of the mechanism remain unchanged; but there is no room in the pelvis for the great right lateral flexion of the head that would be necessary to maintain the presentation direct, and the mechanism does not demand it. A certain amount of lateral flexion is made, and this diminishes the obliquity. This moderate lateral flexion is not produced by spontaneous fœtal motion, but by the powers of labour urging the child through a canal which at this part is rigid and contracted enough to force the soft fœtus to adapt itself to its graduated curvature.—*Edin. Medical Journal, Aug. and Sept.*, 1861, *pp.* 127, 245.

110.—AN EFFORT TO SHORTEN THE DURATION AND DIMINISH THE PAIN OF THE FIRST STAGE OF LABOUR.

By Br. B. Fordyce Barker.

Modern obstetricians divide labour into three stages. The first stage is said to commence with the beginning of labour, and to end with the complete dilatation of the cervix uteri; the second stage ends with the expulsion of the child, and the third includes the delivery of the placenta. But there is a precursory period, during which the uterine neck is preparing for the first stage of labour. This period commences in some two weeks, and in others not more than five or six days before labour may be said to actually begin. During this time the uterine tumour becomes sensibly lower, and this change is accompanied with a corresponding change in the symptoms. The respiration becomes free, the action of the diaphragm is less impeded, and the stomach is no longer compressed; while nausea and vomiting, if they have existed in the latter stages of pregnancy, now disappear. At the same time that the thoracic and epigastric symptoms disappear a new order of symptoms, referrible to the pelvic organs, take their place. There is a pressure on the rectum and bladder, producing sometimes frequent desire to evacuate the bowels and bladder, tenesmus and dysury, and increased œdema of the genital parts and lower extremities. Walking often becomes painful and difficult, and sometimes even impossible. During this time, also, the woman frequently suffers from pains, short in duration, and occurring at distant intervals, but gradually increasing in length and frequency. This is very generally the fact with primiparæ. These pains are coincident with the uterine contractions; for if the hand be placed over the uterine globe during the time of the pain, it will be found hardening, and, as the pain disappears, gradually relaxing. In the multiparæ these contractions frequently exist with scarcely any pain. In the primiparæ, it has been remarked, and the experience of most obstetricians will confirm the remark, that when no cause of dystocia exists, the labour is usually much more rapid in those females who have suffered from frequent pains during the last fortnight of their pregnancy.

We are indebted to modern science for the true explanation of these precursory phenomena. It was formerly taught that at the end of the sixth month of pregnancy the os internum or the upper portion of the cervix uteri began to open, so as to aid in the enlargement of the body of the uterus; and that this spreading at the upper part continued to advance in proportion as the term of gestation approaches, and consequently, that the length of the neck decreases from above downward, so that the neck entirely disappears at the end of pregnancy. But it has now been established beyond cavil that the neck has no participation with the body in forming the uterine walls to inclose the fœtus; but that, on the contrary, the cervix uteri preserves its whole length until the last fortnight of pregnancy. The neck of the uterus undergoes most important changes during the period of gestation, but a discussion of these changes is not pertinent to the subject of this paper. It is sufficient to say here that the neck of the uterus contributes in no degree to the capacity of the body for retaining the fœtus. But during the last fortnight of pregnancy its length diminishes very rapidly, opening from above downward until it is wholly effaced. The precursory phenomena alluded to above are due to

these changes. To use the language of Caseaux, "in the last fortnight the internal orifice softens and yields to distension, then expands from above, so that the upper half of the neck gradually becomes confounded with the cavity of the body; the lower part of the ovum will evidently engage in the dilated portion, and soon come in contact with the parts in the neighbourhood of the external orifice. This contact occasions a progressive irritation of the nervous fibres of the lower half of the cervix, which, by reacting upon the body, excites its contractions, until finally the entire neck being effaced, the irritation reaches its maximum, and labour commences."

The phenomena of the first stage of labour pertain, then, to the dilatation of the *external orifice* of the neck of the uterus. Obstetricians are generally agreed in saying that the duration of this stage is two or three times the length of the second stage, where no cause of dystocia exists. The pains of this stage are of a peculiar character. They are usually borne with more impatience, and very much less fortitude, than those of the second stage. They are greatly dreaded by the patient, who has an instinctive feeling that they are doing no good. She gives utterance to sharp expressions of suffering when the glottis is free and open during the act of expiration. The pains of the second stage seem to be suppressed, like a person carrying a heavy burden, and are scarcely heard except during inspiration. The pains of the first stage are avoided as much as possible, while the patient seems frequently to solicit and invite those of the second. The expressions of the first stage have been characterised as those of *suffering*—the expressions of the second as those of *exertion*. In ordinary language, the term pain in labour is used as synonymous with contraction, and we speak of its return, duration, intensity, and weakness, to describe the force and efficiency of the uterine contractions. But this is by no means strictly correct. The muscular contractility of the uterus is precisely of the same nature as that of the other hollow organs, as the bladder and the rectum, and is never developed except under the influence of some stimulant or irritant. The exercise of this organic contractility is usually accompanied with pain in the human female, but this is by no means the invariable rule. Dilatation of the upper portion of the neck is very frequently accomplished under the influence of contractions which are not perceptible to the patient; and in some patients the same remark is true in regard to the dilatation of the os externum. The intensity of this organic contractility varies extremely in different females, being very strong in some and scarcely perceptible in others. Its energy bears no relation to the external muscular system; for some strong muscular women have very weak contractions during labour, and some very weak, feeble women have very strong contractions. This organic contractility is, like all muscular power, exhausted by prolonged exercise, as we often see the labour commence with strong, vigorous, and frequently recurring pains, which, after some hours, become slow and feeble, and sometimes cease altogether for a while. For this reason the duration of the first stage of labour may greatly influence the favourable progress and length of the second stage. It is usually said that the first stage may be prolonged without danger, and that the only inconveniences resulting are, the great fatigue caused particularly by the loss of sleep, and in nervous women considerable irritation, depression of spirits, and alarm; while the second stage cannot be prolonged beyond certain limits without greatly endangering the health of the patient, and oftentimes the life of the child.

The first part of this proposition is true, so far as the immediate results are concerned; but it may be quite the reverse from the exhaustion of the muscular contractility produced; thus inducing a feeble and inefficient action of the organ in the second stage of labour.

It is obvious that if the dilatation of the os externum could be accomplished by comparatively painless contractions, as is ordinarily the dilatation of the upper portion of the cervix uteri, a great point will be gained in mitigating "the pains and pangs of labour;" and where no cause of dystocia exists, there is a strong probability that the duration of the second stage would be decidedly decreased.

In 1854, at the close of one of my lectures, I was informed by one of the students that his father, a physician in Ohio, was in the habit of giving belladonna internally as a preparation for labour. I very much regret that I have lost the name of the gentleman, although I have made repeated efforts to ascertain it. The hint given led to a series of experiments, the results of which will be seen in the table of 147 cases of labour, in which I have been able to test the effects of this plan. I have aimed to commence the treatment about two weeks before the end of gestation; but of course the data for calculation are so uncertain, that the length of time that the medicine was taken varied much from this period. The treatment has been tried with only a part, I may say a small part, of my obstetric patients, for many reasons. In many cases, I have first seen the patient when labour has begun, without having been previously engaged. In others, labour has come on within two or three days after the treatment has been commenced. These are not included in the table. Others, again, I have attended in former labours and have not been called until the second stage of labour had commenced. Only those are included in the table where the plan has been to a satisfactory degree tested. I have found a very great difference in patients as to their susceptibility to the influence of the agent, and also a great difference in the purity and strength of the article. One extract would seem to have double the potency of another, without any corresponding difference in the appearance, colour, or odour.

I have aimed to commence always with a minimum dose; but in some cases, owing either to the idiosyncrasy of the patient, or the unusual strength of the article, I have been obliged to diminish the dose; but in most cases it has been gradually doubled, or even tripled. The test has been the constitutional effects of the article exhibited in a slight degree; as dryness of the throat, slight uneasiness or giddiness of the head, or dimness of the vision. I direct that any or all of these symptoms should be watched for, and if they appear, to slightly diminish the dose; that is, take it twice instead of thrice a day. In but one case have I had any really unpleasant effects produced. This patient had been poisoned in her childhood by eating the berries of the belladonna, and after taking the article for three days its constitutional effects were suddenly developed, and excited great alarm on the part of her friends. The symptoms, however, disappeared in the course of twenty-four hours. Labour came on the second day after the disappearance of the symptoms, and was so rapid that I did not see the patient until it was completed. Her first labour (this was the second) was reported to have been thirty-six hours in duration.

In not one of the cases was the child still-born, and in none of them was there post-partum hemorrhage or retention of the placenta. In one the function of lactation was entirely absent; and in two others the mammary secretion did not appear until the fifth day. Where the patient was plethoric, of a full habit, rigid fibre and active circulation, I have combined the antimon. et potass. tart., as in the following formula:—

℞.	Belladon. ext.	gr. viij.
	Antimon. et potass. tart.	gr. iij.
	Syr. aurantii	ʒ ij.
	Tinc. aurantii	
	Aquæ	āā ʒj.

M. S. A tea-spoonful three times a day.

In all cases, I have commenced with the extract of the belladonna, in one quarter-grain doses. With some I have made use of the following formula:

℞.	Tinct. cinchon. co.	ʒ iij.
	Syr.	ʒ j.
	Belladon. ext.	gr. viij. M.

Where special indications have existed, I have combined the belladonna with a great variety of other articles. The test of a larger experience of other careful observers is needed to determine whether this prophylactic plan is a real contribution to the progress of the obstetric art.—*Medical Circular, April* 24, 1861, *p.* 290.

111.—SPONTANEOUS EVOLUTION.

[The following interesting case was communicated to the 'Medical Times and Gazette,' by Mr. Heslop, of Haverfordwest.]

A few days ago I was called, at an early hour in the morning, to a patient residing in the vicinity of Haverfordwest, who had been in labour about twenty-four hours. A midwife was in attendance, and upon my entering the house, during a violent bearing-down pain, the right arm passed into the vagina, and was protruding externally through the vulva.

The uterine contractions were very violent, so much so, that I deemed it inexpedient to adopt the usual procedure of turning, until I had so far overcome the powerful contractions of the uterus, by the inhalation of chloroform, as to enable me without danger to turn the child and bring down the feet. On introducing my hand for this purpose, I found the head and arm closely impacted in the pelvis; and having, with much difficulty, passed my hand beyond the presenting part of the child, I was proceeding in the operation, when a violent contraction of the uterus took place, arresting my hand, and changing in a most sudden and complete manner the position of the child. The head and arm, which before were closely jammed in the pelvis, ascended, the breech and the legs simultaneously entered the pelvis, and by one vigorous contraction of the uterus, the child was immediately expelled, thus rendering further manipulation on my part unnecessary. The child was full grown and alive. The right side of the head, face and neck, &c., were much swollen, although great pressure on these parts could not have continued long without causing the death of the child and endangering the life of the mother.

This case shows how strong were the efforts of Nature to cast off her mould—and as in a *prima facie* view of the case, turning would appear inevitable, she, by her own unaided efforts, occasionally gives a wonderful illustration of that peculiar mode of delivery termed "spontaneous evolution."

The introduction of my hand, which was difficult, probably elevated in a slight degree the presenting part of the child,—thus beginning, as it were, what Nature so well completed.

The mother and child are doing well.—*Medical Times and Gazette, June 8, 1861, p. 614.*

112.—IMPEDED LABOUR FROM DORSAL DISPLACEMENT OF THE ARM.

By J. Jardine Murray, Esq., Surgeon to the Brighton and Howe Dispensary.

Some importance may be attached to the case which forms the subject of this communication, as the peculiar form of obstruction to parturition which occurred does not appear to have been recorded by any obstetrician, except Professor Simpson of Edinburgh. It consists in one of the arms of the infant being displaced backwards across the neck or occipital region; or rather the forearm is thrown across the back of the head and neck, the arm being thrown upwards in a line with the body in order to admit of the malposition of the forearm. In this abnormal position the displaced elbow and forearm of the child increase greatly the dimensions of the head, and form a projecting obstruction which hitches upon the brim of the pelvis and prevents the descent of the head.

Henrietta H., aged 28, had borne two living children at the full term and had one miscarriage at the fourth month. Her first labour occurred five years previously, when she was delivered of a large male child. After an interval of three years she was again confined in so easy and expeditious a manner that the medical man did not arrive till after the birth of the child, which, in this case also, was a well developed male.

At 12 p.m., on March 24, 1859, when Dr. Eastlake was summoned to at-

tend her, he found that she had already been in labour several hours. Liquor amnii escaped, os uteri well dilated, external parts hot. Notwithstanding strong expulsive pains, the fœtal head made no advance, and as there was great prostration, Dr. Eastlake requested me to see her with him. I first saw the patient at 5 a.m. She was much exhausted and delirious, the skin dry, and the pulse weak and rapid. The maternal passages, though swollen, were perfectly relaxed and open, there were strong uterine contractions, the pelvic brim seemed of ample dimensions, and the fœtal head appeared by no means unusually large. It was a vertex presentation in the first position of Nægele. During each pain the fœtal head was pressed down on the pelvic brim, but again receded on the discontinuance of uterine action. The cord was ascertained to be partially prolapsed and perfectly pulseless.

As there was evidently some unusual cause of obstruction, and as it was necessary for the safety of the mother that delivery should be accomplished without further delay, she was placed under the full influence of chloroform. On now passing the examining fingers upwards, I detected the right elbow lying behind the right ear of the child; and on still further pursuing the examination, found that the right forearm lay behind the occiput. After this diagnosis had been carefully and fully verified by Dr. Eastlake, I delivered by podalic turning. The child was a normally-developed female of average size. The mother made an excellent recovery.

Here, then, we had a case in which, without proper assistance, rupture of the uterus might have occurred, or in which the woman might have died undelivered. The delivery by turning was, as far as circumstances allowed, perfectly satisfactory. In a similar case, it seems highly improbable that the malposition of the arm could be rectified by the natural expulsive efforts. If, therefore, the cause of obstruction were detected early in labour, the accoucheur might possibly hesitate whether to attempt to improve the position of the arm, or to have recourse at once to delivery by turning. But the malposition of the arm which has been described, is not likely to be discovered till the apparently natural labour has been unexpectedly prolonged, for the diagnosis can hardly be satisfactorily made out till the patient has been subjected to the full influence of anæsthetics.

Dr. Eastlake, who took much interest in this case, and who has lately had opportunities of profiting by the extensive practice of the Dublin Rotunda Hospital, entertains a different view of the treatment to be pursued, as may be seen by reference to his remarks appended to this paper. But I cannot agree with him as to the propriety of attempting to deliver by forceps in this form of obstruction, unless the misplaced arm has previously been properly adjusted, for I should have little hope that extraction of the child could be effected by such means. Nor do I think that craniotomy is the preferable operation when the child is dead; for in cases like that above related, the head has not entered the pelvic brim, which is however of ample dimensions, and the maternal passages are well relaxed. In such circumstances I have not found that delivery by podalic turning, when performed under the influence of chloroform, is an operation of great difficulty or danger to the mother, even after the liquor amnii has escaped.

In the case recorded by Dr. Simpson, the patient had previously had nine easy and speedy labours. His case was also a vertex presentation, but the face was directed towards the left, and not as in the present instance towards the right sacro-iliac synchondrosis, and there was misplacement of the left and not of the right arm.

In all probability, this peculiar form of obstruction sometimes passes undetected. Drs. Simpson and Cumming alluded to a case in which "there was no pelvic or other deformity on the part of the mother, no want of uterine contraction, and no disproportionate size of the head of the child, and yet in which the late Dr. Campbell and others entirely failed in extracting the detained infant by the forceps, and were at last obliged to open its head." And it would not be difficult to cite analogous cases from the records of Obstetric Surgery. For instance, the following note appears in the last published record of the practice of the Dublin Lying-in Hospital:—"The

first stage occupied about twelve hours. *The head never entered the brim, though the pains were powerful.* Chloroform was administered, and produced some rest which was much required. The fœtal heart was found to have become extinct soon after the effects of the chloroform were removed, and the head was lessened. The cranium was large and highly ossified, but *no evidence existed of narrowing of the inlet.*"

Remarks on the Foregoing Case. By Dr. HENRY E. EASTLAKE, *late House-Surgeon to the Royal Maternity Hospital of Edinburgh.*

This case cannot but be interesting to all who are engaged in practical obstetrics. I think this posterior displacement of the arm may justly be regarded as a variety of natural labour, in common with hand and head presentation. I quite agree with Mr. Murray in believing that the true nature of this peculiar impediment to the progress of labour may often escape the observations of accoucheurs. The diagnosis of such cases cannot, I think, be ever thoroughly established, unless, as in this case; the whole hand be introduced into the vagina, for it must be at the pelvic brim that the advance of the fœtal head is arrested. The point that becomes so interesting and so important practically is, how such cases should be treated. In the Rotunda Hospital in Dublin the forceps has been successfully applied in two similar instances within the last twelve months; and supposing the fœtal heart to be audible when the true nature of the presentation is ascertained, it seems the most rational practice, although it may perhaps be difficult to explain exactly the mode in which the arm becomes adapted to the maternal passages during the traction of the head. It must, however, be apparent that the life of the fœtus may be placed in great danger, owing to the second stage of labour being unduly prolonged, if we are ignorant of the cause of impediment; and again, the very nature of the displacement may tend to allow of the funis being prolapsed. If, then, from the pulseless state of the cord (should it be prolapsed), or from the cessation of the fœtal heart after its being distinctly audible, or, indeed, from any other source, we can positively diagnose the death of the fœtus, it becomes a matter of importance to determine upon the management of the case under such conditions. The operation of turning cannot, I think, be regarded as the most judicious. First, because, in all probability, the liquor amnii would have long escaped, necessarily involving difficulty, and in some cases impossibility, of version; and secondly, because this operation, performed under such unfavourable circumstances, would, to say the least, be far more hazardous to the mother than either the use of the forceps, or the operation of craniotomy.

The use of the forceps, again, does not seem to be altogether desirable, as the great merit it possesses in giving a chance of life to the infant would be here unavailing. Supposing, therefore, a case in which the infant was dead. I should, in that conviction, be inclined to follow the treatment generally adopted in the Dublin School, when fœtal life is extinct, viz., craniotomy, which is there held to be an operation *per se*, far less dangerous to the mother than the use of the forceps. This is the experience and teaching of Dr. M'Clintock, the present master of the Rotunda Hospital, who has, I think, shown most satisfactorily that the operation is not attended with that amount of danger to the mother generally supposed, and that the great mortality in the statistics of such cases is not due to the operation in itself, but may be fairly attributed to the fact, that craniotomy is often adopted after other modes of treatment, themselves attended by danger, have failed in their object.—*Medical Times and Gazette, June* 15, 1861, *p.* 627.

113.—ON UTERINE HEMORRHAGE.

By Dr. G. HAMILTON, Falkirk.

In the 'Edinburgh Monthly Medical Journal' for October 1850, I described a vaginal plug which I had used with success in a case of uterine hemorrhage

from cancerous ulceration. Since then I have frequently employed a similar instrument in ante-partum and other cases, and have found it extremely useful. A number of professional brethren, from having read the paper referred to, or from having seen the instrument, have asked me where such a one could be had. I may therefore now mention that I have made some modifications on the original instrument, and that it can be got at Thornton & Co.'s india-rubber warehouse, Princes Street, Edinburgh, as may be seen from an advertisement accompanying the present number of this Journal. This instrument is extremely simple, so that any one can readily get it made or modified so as to suit any particular case. It consists of a brass tube of the length required (say about 3 inches), with two buttons about 1¼ inch in diameter, shaped like a watch-glass, the upper of which is fixed to the end of the tube, while the other is moveable, and can be pushed upwards by means of a nut and screw working on the tube. At the other end of the tube are slight prominences, to allow of the attachment of a tape or narrow belt. This tube is attached to one of the ordinary vulcanized india-rubber balls, of the size required, by slightly enlarging the hole in it with a pair of scissors, stretching it, and passing the fixed button through the hole, and then forcing the other button up against the ball and fixing it by means of the nut and screw. The tube can be coated with a varnish of gutta-percha dissolved in naphtha to prevent erosion. The ball used may of course be of any size, but one having a diameter of 2¼ inches is most suitable in ordinary cases. Its walls also should not be too thick, as this renders its introduction difficult when the vaginal opening is small. It must, however, be of sufficient strength to re-expand when introduced. When this is attended to, no injection of air or water, which was necessary in the first instruments I used, is now required—the access of air through the tube allowing the ball to expand freely within the vagina. After the ball has been introduced, it has to be pushed up against the uterus by the stalk, and fixed, with a moderate degree of pressure, by a tape or band attached to it, which has to be fastened before and behind to a belt going round the waist—or what is better, to elastic rings at back and front passed over the belt. In most instances, the mere introduction of the ball, as directed, will be found to restrain the hemorrhage; but in others, where there is inequality of surface from disease, or where the neck of the uterus projects much into the vagina, it is proper, before doing so, to fill up the inequalities with charpie in the former, and in the latter to stuff the upper part of the vagina with lint to the level of the os uteri, and the os uteri itself if necessary. It will be found by the practitioner, in protracted cases of flooding, or where the patient is at a distance, that this little instrument is singularly useful, for the patient herself, or an assistant, can easily be taught to use it. The great advantage of this form of plug seems to me to be, that it allows the pressure to restrain the hemorrhage to be applied *ab externo*. In women who have borne a family, the vaginal relaxation that follows generally renders the introduction of a mere bag useless, from the enlargement of the orifice allowing it to escape, and from the size required causing bearing down efforts to expel it. The stalk in such cases is therefore a necessary part of the instrument. In women, however, who have not had a family, an india-rubber bag, which is not bulky, and which can be inflated after introduction, is sometimes very useful. The little instrument I have described, it will be perceived, also makes a capital vaginal syringe.

In the early stages of labour, with placenta prævia, restraining the hemorrhage is of course a matter of the utmost moment. A case of this kind not having occurred in my practice for many years, I am unable to say whether a plug of the kind described might be used successfully. From the somewhat globular shape mostly assumed by the lower portion of the uterus when labour is commencing, it has struck me that a piece of gutta-percha could easily be moulded into the proper shape, and applied by means of a stalk of the kind described; or that Professor Simpson's india-rubber suction instrument might be adapted to this purpose, applying it to the uterus as he applied it to the head of the child. I have got one of these india-rubber

cups fixed on a wooden tube, which I think looks likely to answer the purpose contemplated. When fixed over the os uteri, the external opening of the tube could be closed with a wooden peg, the cup being partially filled, if necessary, with charpie, or with some glutinous substance, such as treacle and dissolved glue. Pressure could be applied either by means of elastic straps, or, if found more effectual, by the hand of an assistant. These latter suggestions are theoretical, and I give them only for what they are worth, as probably each case of this kind will require adaptations suited to its peculiar requirements.—*Edinburgh Medical Journal, Oct.* 1861, *p.* 313.

114.—EXTRA-UTERINE GESTATION.

By Dr. George Cheesman, Surgeon to the Southampton Dispensary.

Though many obstacles, such as are too familiar to medical practitioners in this country, interfered to prevent a sufficiently minute and exact detail of the post-mortem appearances from being obtained, I nevertheless think the case I am about to relate worthy of record. The inspection after death proved unequivocally that the case was one of extra-uterine gestation, of the form termed "tubal;" moreover, it occurred in a married woman not more than forty years of age, who had previously borne four children, and yet up to the time when I was called in, probably eleven months after conception, she had entertained no suspicion that she had become pregnant for the fifth time; lastly, till about six weeks before I saw her she had been, during nearly the three previous months, under the advice of a highly esteemed medical practitioner, who appears to have dealt with the case as one of dropsy of the abdomen.

When I first visited E. S., the subject of this case, she was sitting on the edge of the bed, and presented to me the appearance of a woman who was suppressing the pains of labour. She, however, declared that she was not even pregnant, and had been suffering from the same kind of pain for more than eleven months. Her countenance was anxious, the pulse 96, the tongue clean. I made her stand up, and found the parietes of the abdomen in the erect posture to correspond in every respect to their state in a woman far advanced in pregnancy. In this posture, even firm pressure caused no particular uneasiness. I then asked her to lie down, which she did not accomplish without great pain and difficulty. On the right side she could not rest at all, and the attempt to place herself in the supine position was attended with much pain. It was only on the left side that she could lie, and this had been the case ever since the commencement of her illness. On examination per vaginam, I found the os uteri sufficiently dilated to admit the forefinger of my right hand, while there appeared to be something roundish presenting, which I took to be either a tumour or some part of a child's head. On further inquiry, she informed me that three of her children were still alive, the youngest being eleven years old, and that since the birth of the last-mentioned she had never felt any sign of being pregnant. She said further that since the birth of her youngest child till about Christmas, 1859, she had enjoyed a uniformly good state of health, with no other exception than that there had been a slight irregularity in the monthly periods. She described her illness as having commenced at the time just indicated, with pain in the left side, which gradually increased; that this pain went on augmenting in the same proportion in which her bulk became greater; that the enlargement of her person was very slow at first, but that within the last four months she had rapidly increased in size. The pain was constant, and extended along the lower part of the abdomen to the opposite iliac region. Since the commencement of her illness menstruation had been scanty, occurring at intervals of five or six weeks, and coming away mostly in clots. She had besides had occasional discharges of an offensive greenish liquid from the uterus. The bowels had been confined throughout. I prescribed an anodyne mixture to be taken at intervals.

Next day I found her lying on the left side with the knees drawn up. There had been some slight abatement of her sufferings, but the pain around the lower part of the abdomen still continued. The bowels had not been relieved; she was annoyed with flatulence, and thought herself troubled with spasms. Another vaginal examination afforded no further information than that of the day before. The naked parietes of the abdomen felt hard on pressure; but at the lower part, below the umbilicus, fluid seemed to be detected by percussion. The umbilicus protruded as in pregnancy; the superficial abdominal veins were much enlarged. A dose of castor oil was ordered, and subsequently to its effect the same mixture was to be continued.

On the third day, Nov. 30th, the bowels had been relieved. The pain was the same as the previous day. She complained of thirst; the pulse was 110, and feeble. The anodyne mixture was continued.

Dec. 2nd. Dr. Welch visited the patient with me. We examined the abdomen with a stethoscope, and we both believed we distinctly heard the sounds of fœtal circulation above the level of the umbilicus, and about two inches and a half to the left. He also made an examination per vaginam, but found the os uteri too little dilated to throw any light on the case. The tongue was now becoming covered with aphthous spots; the pulse was 120, and thready; the countenance anxious. A slight change was made on the anodyne mixture. Beef-tea and wine were allowed *ad libitum*.

3rd. The aphthous spots were enlarged, and extending to the soft palate and to the sides of the cheeks. The patient could retain nothing on her stomach. The pain around the lower part of the abdomen remained much the same. The sounds of the fœtal circulation seemed still audible. An effervescing draught, with excess of ammonia, was added to the anodyne.

4th. There were now great anxiety of countenance and much prostration, without any abatement of the pain. The tongue, cheeks, palate, and fauces were thickly coated with aphthæ; the nausea was undiminished. The sounds referred to the fœtal circulation were no longer audible, while through the parietes of the abdomen the extremities of a fœtus could be felt. The same treatment was continued.

On Dec. 5th there was no improvement, while there was a gradual aggravation of some of the symptoms. The bowels being confined, castor oil was prescribed.

7th. Incessant vomiting of matter, smelling of wine, and having the appearance of feculent substance. It was evident the patient was gradually sinking. The same treatment was continued.

8th. Dr. Palk visited the patient with me. We were both of opinion that she could hardly outlive the day. She died at half-past four a. m. on December 9th.

Autopsy.—On inspecting the body previously to the post-mortem examination, the abdomen was found enormously distended; the parietes were very tense, and percussion indicated a highly tympanitic state of the cavity. The uterine region gave a dull sound on percussion, and there were distinct signs of fluctuation. A section was made in the mesial line, extending from the pubes to the ensiform cartilage. There was much fat, especially over the uterine and sternal regions. When the peritoneum was cut into, a large quantity of most offensive gas escaped, so as to very considerably reduce the size of the abdomen. A distinct moveable tumour could now be felt within the abdomen, which might have been taken for the gravid uterus. After a large incision was made, the body of a full-grown fœtus was exposed, lying transversely under the great omentum, with its head towards the right side. By following the funis, the placenta was found, nearly normal in appearance, manifestly involved in a dilatation of the left Fallopian tube. The uterus was found pressed down towards the right side of the pelvis. The uterus was about the size of a child's head, and its walls were thickened to the extent of two inches and a half, its substance exhibiting a fibrous character; the lining membrane presented a congested appearance. Beyond this lining membrane there was nothing in its cavity. The os uteri was undilated. The right Fal-

lopian tube was normal in character. The body of the fœtus was distended with gas, particularly the head, which, from this cause, presented a hydrocephalic appearance. A large quantity—nearly two gallons, of a dark grumous fluid, with flakes of coagulable lymph, was found in the cavity of the abdomen, and dark gangrenous patches observed in several places on the peritoneum.

It must be at once apparent in a case like this, where so many parts are forcibly deranged from their natural positions, how little likely a post-mortem examination, made in haste, is to disclose all the particulars requisite to illustrate its exact nature. I have said that the instance before us belongs to the form of extra-uterine gestation termed "tubal." Some may demur to this decision. But it appears to me that the examination was carried far enough to put this beyond doubt. In the first place, it was not an example of what authors call "ventral" extra-uterine gestation; for, while many deny that any such form of this anomaly can occur, by those who deem it possible it is held to be marked by the absence of any kind of cyst around the ordinary fœtal membranes, and by irregular conditions of the placental development. The dissection, however, in our patient showed unequivocally a firm cyst, which, as was remarked at the time, might have been mistaken for the gravid uterus, while the placenta exhibited nothing abnormal in its character, if its unusual position be excepted. In the next place, that the case in question was not of the form termed "ovarian," or of that named "ovario tubal," seems certain, as no connexion was observed between the ovary of the side concerned and any part of the sac. Lastly, it was not interstitial, for the substance of the uterus was wholly isolated from the sac.

It appears from the statements of authorities on this subject, that the tubal form of extra-uterine gestation is that which most frequently occurs, and that, as in the example before us, the left Fallopian tube is most frequently the seat of the development of the fœtus. The well-known mechanism of conception, and the experiments made in illustration of that mechanism, render it easy to understand how the arrest of an ovule may occur at any point in the cavity of one of the Fallopian tubes, so as to establish there an extra-uterine development. It would be easy to cite from authorities many well-authenticated examples. The Fallopian tube distended comes to form a thick sac, with walls of a fibrous structure, resulting from hypertrophy of the contracted fibres of the enlarged canal. The natural mobility of the Fallopian tube accounts for the diversities of the position which it occupies when thus loaded with a body, by successive steps more and more developed in volume. Most commonly it has its seat in the hypogastrium, whence it rises to the level of the umbilicus; but at other times, while the bulk is still moderate, the fœtus sinks between the uterus and the rectum, so as to remain immersed in the cavity of the pelvis, beyond the reach of discovery, unless by the fingers being introduced into the vagina or into the rectum.

One of the chief obstacles to the early diagnosis of the real nature of the case before us was the total absence of suspicion of pregnancy, on the part of the patient. This circumstance in respect of a woman who had borne children, and had no reason to conceal the suspicion, if it had arisen, was sufficient to throw a medical practitioner off his guard as to the true diagnosis. It may be presumed, therefore, notwithstanding the evidence of the sensible movements of the fœtus in extra-uterine pregnancies being often both more striking and of earlier occurrence than in natural gestation, that there was here an exception to this rule, such as has been remarked on other occasions of the same character. The apparent continuance of menstruation to which, probably, the patient was altogether unaccustomed in her previous pregnancies, might serve to confirm her in her belief that she was not with child. It is true that authors have laid down the continuance of the menstruation amongst the signs which should lead to a suspicion of extra-uterine gestation; but the statement obtained from the patient in this case, that a discharge took place from the vagina during her illness every five or six weeks, chiefly in clots, is not sufficient evidence that real menstruation was one of its concomitants.

In the early stage of this gestation it seems probable that an exact diagnosis was altogether impossible. In the more advanced stage, however, the stethoscope should have been sufficient to indicate the presence of pregnancy, while the anomalous condition of the os and cervix uteri, and the deficiency of the appropriate weight of that organ as gestation proceeded, should have formed data for the opinion that the development of the fœtus was extra-uterine.

This case not only belongs to a rare kind of anomaly, but as an example of that anomaly is rare in respect of its attendant circumstances. For here the infant obviously lived to the full term of its development, whereas the common rule of such deviations from nature is that the death of the fœtus takes place after the third or fourth month. At this period the sac which supplies the place of the uterus gives way, sometimes with one result, sometimes with another, yet not always with an event fatal to the mother. There were no signs discovered in the post-mortem examination of this woman that rupture of the sac had in any part taken place; nevertheless, the great quantity of altered blood found in the peritoneal cavity creates a regret that a more minute examination with regard to this point was impracticable.

The only remedy that could have been available where the place of the fœtus was so distant from the natural passages of parturition was the Cæsarean section, or the similar operation as proposed by Graves, *en deux temps*—that is, the division of the skin, the muscles, and the aponeuroses as far as the perineum at one time, and then after eight or ten days, the opening of the cyst and the extraction of the fœtus.

It is true that in extra-uterine gestation it has happened that both mother and child have been saved; but how rarely has this been the issue! How seldom has the operation been performed after the child had reached the full term! And, whatever may have been practicable at an earlier period of this woman's case, I believe no one will venture to say that the Cæsarean section should have been attempted in the state in which she was after the time when I first visited her.—*Lancet, Sept.* 14, 1861, *p.* 250.

115.—IMPROVEMENT IN THE SPECULUM.

Messrs. Weiss have brought out a useful improvement of the vaginal speculum. A serious inconvenience in the use of the bivalve speculum is the liability to pinch up folds of skin or hair between the blades. The practised obstetrist no doubt easily avoids this accident; but it does not fall to every one to use the instrument so frequently as to be able to guard against it. The speculum is shielded by a thin caoutchouc sheath. The speculum itself is materially improved by the action being what is mechanically termed direct, and the mouth of the instrument is bell-shaped. Greater power and perfect inability to strain the instrument are thus gained, as well as a much wider opening to admit light and favour manipulation. Another improvement consists in the form of the tampon used to facilitate the introduction of this speculum. A caoutchouc bag, attached to a stem, is made to expand so as to overlap the edges of the blades. The inflation is effected by pressing with the thumb upon a communicating bag containing water. The pressure removed, the tampon collapses, and is easily withdrawn when the speculum is *in situ*. We have tried the instrument, and are much pleased with its construction and action.—*Lancet, April* 27, 1861, *p.* 415.

116.—ON VESICO-VAGINAL AND RECTO-VAGINAL FISTULÆ.

By T. Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

[It is interesting to see the progress of surgery simply in the union of injured parts, as well as in more important ways, such as excision of joints, and the radical cure of hernia.

But of all the triumphs of modern surgery, the cure of vesico-vaginal fistula is perhaps the most brilliant. Ten years ago, most surgeons believed this distressing condition to be incurable. At long intervals one heard of a case successfully treated by caustics or the cautery, or of a spontaneous cure after sloughing and cicatrisation; and occasionally, though very rarely, a cure was obtained by surgical operation. I believe I had been quite as fortunate as other surgeons, having had successful cases both after cautery and the suture between 1845 and 1853. I and my colleague Dr. Savage had each a case in which a small fistula healed after a single silk stitch. I had tried the lead-wire suture, after Dieffenbach; the hare-lip pin suture, after Wutzer; and various forms of silk and metallic sutures, with some encouragement, but I failed after several operations in some cases which I feel confident I could cure now at a single trial. I believe other surgeons were much more discouraged than I was. Many openly expressed their disbelief in the possibility of cure, and attributed the success in cases they could not deny to "luck."

Lecturing thirty years ago ('Lancet,' June 23rd, 1828,) Mr. Liston said:—"It was seldom that union took place. All, indeed, might appear to go on well for eight or ten days; but at the expiration of that time the wound probably would be found to have been enlarged by having been interfered with, and would become larger and larger every time the attempt at cure was made. When the communication is to a large extent, but little hope remains to the patient."

So in 1844, Dr. Churchill wrote in his 'Diseases incident to Pregnancy and Childbed:'—"In the majority of cases, I fear we shall find but little benefit. Indeed, vesico-vaginal fistula has long been considered as one of the *opprobria* of surgery, and with some exceptions of late years, the cure has been given up as hopeless." Churchill, 1844.)

This is but a recurrence to the opinion of Smellie, published now nearly a century ago, who said (Midwifery, vol. i. p. 386)—"If the opening is large, close it with a double stitch (the edges of the wound having been refreshed), keeping the flexible catheter in the bladder until it is entirely filled up. I wish this operation may not be found impracticable." (Smellie, 1766.)

Dieffenbach's graphic account of his own disappointments may be taken as representing the feeling of most surgeons ten years ago. In his 'Operative Surgery,' published in 1845, he writes, that all he has said about vesico-vaginal fistula he has learned "bit by bit" from innumerable cases. "I was once delighted to find a fistula extending along the whole roof of the vagina healed in eight days: and I was filled with hope that I was at last able to conquer the enemy at all times; but then I was again defied by a mere needle puncture remaining after an operation otherwise most successful, or a fistula of the size of the head of a probe resisted the most persevering efforts. I saw openings of the size of a small pea, after the use of the suture or the cautery, attain the circumference of a large pea; an opening of the size of a silver penny became as large as a twopenny piece; one, of a threepenny-piece, as large as a sixpence; and then I stopped. I operated on one woman eighteen times, and did not cure her after all. I collected whole wards full of these unhappy creatures from all parts of the country, and gave up a great deal of time to them, quite liking the trouble, and yet I cured but very few. Two died of cystitis or peritonitis; and one even after suture had led to a complete cure of the fistula."

To show how little faith some surgeons had in any operative treatment of vesico-vaginal fistula, I will just add that Mr. Syme, in the last edition of his 'Principles of Surgery,' published in 1856, does not even allude to the subject. Mr. Fergusson, in his last edition (1857), only gives one line to it; and Mr. Skey in the second edition of his 'Operative Surgery,' published a year later (1858), describes an operation for recto-vaginal and recto-vesical fistula, but does not say one word about vesico-vaginal fistula. I am not saying this to find fault with these teachers. I simply wish to remind you how very recent are the improvements which are now so generally accepted by the profession.

It is from 1852 that what I will call a new era in surgery must date. It is quite true that metallic sutures had been used long before that year. Putting aside all reference to hair-lip pins, and to the entomology pins so largely used in other plastic operations (where the surgeon looked to the support the pins afforded, and the opportunity they give of making lateral pressure upon the opposed surfaces of the wound, rather than to any superiority in metal over hemp or silk), it is well known that Mr. Gossett cured a vesico-vaginal fistula in London as early as 1834 by the use of a suture of gold wire. But his example was not followed. He was even ridiculed for supposing that the material of his suture could have had any influence on the result. So in 1840, I heard Mr. Morgan, at Guy's Hospital, lecture on the Uses of Platinum Wire as a Suture, and illustrate the non-irritating effects of metal by reference to the manner in which ladies' ears bear ear-rings. But his example was not followed to any extent, either by his colleagues or his pupils. Dieffenbach had been showing how successfully lead-wire replaces silk in many cases; and I applied this wire in cases of vesico-vaginal fistula in 1852 and 1853. Early in 1855 I operated upon a patient of Dr. Simpson's in the Edinburgh Infirmary, and intended to use lead-wire; but an accident put the needle out of order, and I had to trust to silk. Mattauer, of Virginia, also advocated Dieffenbach's suture of twisted lead-wire; but it was left to Sims, of New York, to make known, in 1852, the great use of silver-wire sutures; and to Dr. Simpson, to impress upon the mind of the profession the principles upon which the superiority of metallic over silk and other absorbing materials is established. Next to his discovery of the use of chloroform, I think his exposition of the use of metallic sutures is the best of the many gifts for which medicine and mankind have to thank this great man.

In addition to the introduction of metallic sutures, various modes of fastening and covering them, and of keeping the united surfaces immovable, were also promulgated. I shall allude to some of them presently, although they are now almost disused; and I shall also show you all that is good and useful in various contrivances for exposing the fistula, and paring its edges. But before doing so, I must just allude to one other *principle* upon which success depends. I am not quite sure whether it was Dieffenbach or Jobert who first exposed the error of former operators; but they both insisted upon (what we now wonder could ever have been overlooked) the importance of not cutting away any of the mucous membrane of the bladder in paring the edges of the fistula; and they also showed that if this membrane were perforated by the sutures, the track of each suture would become a new fistula. Hence the obvious rule to carry the sutures *close to*, but not *through*, the vesical mucous membrane; and in paring the edges to spare this membrane. In order to get as broad a surface as possible for adhesion, most recent operators pare the edges in an oblique direction—*bevel* them.

Some operators (Langenbeck some years ago, and more recently Mr. Collis of Dublin) have avoided cutting away anything from the opening, and have obtained their raw surfaces by splitting the border, or separating by horizontal incision the vaginal wall or surface from the vesical wall or surface; in other words, separating the vaginal from the vesical portion of the vesico-vaginal septum. Then, turning the vesical portion of the raw edges towards the bladder, and bringing the vaginal portion downwards, a large raw surface is obtained without cutting away a single shred of tissue. I have adopted this method occasionally; but when the margins of the fistula are hardened, or otherwise much altered from their natural condition, I think it is better to cut them away. But, whatever plan you adopt, you must bear in mind these two principles:—

1. In paring the edges of a vesico-vaginal or a recto-vaginal fistula, and in passing the sutures afterwards, to spare the mucous membrane of bladder or rectum, and make a broad raw surface;

2. In uniting the raw surfaces, to bring them into close apposition by metallic sutures.—*British Medical Journal*, *August* 31, 1861, *p.* 223.

117.—ON A NEW AND SUCCESSFUL MODE OF TREATING VESICO-VAGINAL FISTULA.

By Maurice H. Collis, Esq., Surgeon to the Meath Hospital.

[In a former number of the Dublin Quarterly Journal, Mr. Collis drew our attention to some improvements in this operation.]

The operation consists, first, in splitting the margin of the fistula all round, so as to separate the vesico-vaginal septum into two equal portions, one-half consisting of the vaginal mucous membrane and submucous tissue, and the other of vesical mucous membrane and submucous tissue, the muscular portion of the septum being equally divided between the two. The extent of this artificial separation is to be regulated by the extent of the fissure, by the condition of the margins, and to a certain extent by the position of the fissure. Where the fistula is near the vesical end of the urethra, or near the cervix uteri, the dissection need not be carried to any great depth—in the former case from the substantial and highly vascular condition of the parts, which ensures ready union, and in the latter from the toughness of the structure, and also because there would be some risk of opening into the canal of the cervix, an accident which would give rise to a troublesome complication, in the form of a fistula, most difficult to close. On the other hand, when a fistula is situated in the intervening space or floor of the bladder, the dissection requires to be somewhat more extensive, owing to the thinness of the septum, which would otherwise afford too small an extent of raw surface to secure union.

The condition of the margins regulates the amount of splitting in this way—if the margins are unhealthy, or infiltrated, or unnaturally thinned away, the separation of the flaps must be proportionally extensive; if they are healthy, a less amount of dissection suffices. The measurement of the fissure has also a bearing on this point, as stated above; if it be extensive, the flaps must be ample; if small, they need not be large.

The second step of the operation consists in the insertion of the sutures; for my operation either wire or thread will answer; silk is now justly condemned by most surgeons as irritating, and productive of ulceration, as a suture. Good thread has always seemed to me preferable to iron wire. I have not an equal experience of silver-wire, and cannot speak to its merits; but I have frequently found iron-wire as irritating in the vagina as silk, although in cases of cleft palate it will remain for weeks unaltered and harmless. I have even gone to the trouble of securing the same kind of wire as Professor Simpson uses, but without having the satisfaction of obtaining the same immunity from sloughing which it seems to possess in his hand. I cannot account for this fact, and only state it that others may not be disappointed. Thread, on the other hand, seems not to produce much local irritation; and I have had thread-sutures lying in the vagina for ten days without producing injurious suppuration. The needles which I use are known as Liston's needles; they are fixed in long handles, with the eye near the point; with these it is easy to pass the thread; and if wire be used, it can be drawn through the flaps by attaching it to the thread. I have my ligatures about a quarter of an inch from each other, and at a considerable distance from the margin of the fistula; this is of great importance. Each suture consists of a doubled thread; and when all are passed through the flaps, a piece of vulcanized India-rubber cord is run through the looped extremities, which are successively pulled tight upon it, and the free ends are then tied over a similar piece of cord at the opposite side of the fissure; the sutures are not tightly drawn. The degree of tightness with which they are to be tied, and the selection of the distance from the margin of the fistula at which to enter them, will test the judgment of the operator, and will most probably insure success or failure as far as he is concerned. If the raw surfaces be tightly drawn together, so that their edges puff up between the India-rubber quills, the flaps will slough. The operator must remember that much swelling will inevitably arise in the course of twenty-four hours,

and that the effect of tightly confining the flaps between two bars will be to interfere with their nutrition, and bring about their death. In like manner, if the quills are too close to the margins, their pressure will produce a similar destructive effect. If there be any strain upon the quills, they must produce some slight ulcerations where they lie in contact with the vaginal surface; and if the strain be too great, or too close to the margin of the fistula, they will cut through, and cut off the flap. I have seen this happen in other hands, in spite of warning; and it has happened in my own very lately, owing to my using the hemp ligature, which, when wet, contracts most powerfully. I was not aware of this, and was seduced into its use by its beautiful roundness and smoothness, to meet with a result which was by no means agreeable. In my first case I used for quill a piece of soft black bougie, for which, in subsequent instances, at the suggestion of Dr. Thorpe, of Letterkenny, I substituted the India-rubber cord. Its advantages are its elasticity, which enables it to give a little in case of unexpected swelling, and its pliability, which enables us to adapt it to any inequalities of the margins of the fistula. This was strikingly illustrated in one case, where the rent was irregularly crucial, and where no unyielding bar would have suited.

Such are the steps of my operation; and I shall now briefly explain in what particulars it seems to be an improvement on the ordinary and older method.

First—It affords an extent of raw surface far and away greater than where the margin is simply pared. It is manifest that any reasonable extent of surface can be obtained in this way; it is only necessary to extend the ring of dissection, to obtain this end; within reasonable limits there is no objection to this procedure. The ridges which might be supposed to result, and which do in fact exist when the cure is complete, will in a few days be smoothed away, and be productive of no permanent inconvenience.

Secondly—This extensive surface is obtained without the loss of a particle of structure, whether in small or large gaps; this is of inestimable advantage. Any one who has removed a ring of mucous membrane from ever so small a fistula, will have observed how large it becomes under the process, and sooner or later will have probable cause to regret the loss of substance thus entailed; whereas, in my operation, if carefully done, no loss of material occurs; and even if it fail, it leaves the patient in no worse condition for subsequent treatment than before. There are, as I shall presently show, cases in which my operation is not expedient, and in these it may be necessary to sacrifice material; but, with these exceptions, I do not think a surgeon is justified in selecting any mode of operating which necessitates the removal of material where there is already a deficiency, and when other methods, which give at least as good results, are offered for his use.

Thirdly—In this operation, we have a double prospect of success. It will be seen, when the raw surfaces are drawn together by quilled suture, that a ridge rises up on the vaginal surface between the quills. A similar and larger ridge is thrown up towards the bladder; this acts as a valve to prevent the water escaping, or even coming in contact with the wound. And even if the parts included between the quills should slough, the flaps which point towards the bladder are exempt from the pressure of the quills and the strain of the sutures, so that they escape from sloughing, and actually unite before the other flaps, and remain united, even if the latter give way.

Fourthly—The operation is simple, and requires no very complex armamentarium, and only that amount of dexterity which should be possessed by every surgeon deserving of the name. I generally prefer the lithotomy posture. It admits of the use of chloroform, and in most instances the fistula can be brought into sufficient command. Sometimes, however, I have found it expedient to adopt another position, viz., placing the patient standing with her back to me, and making her lean across a table, as in the position for examination of the rectum. I use Dr. Sawyer's divaricators, which are of strong brass, bent at about four inches from the end, at right angles; two of half inch width for the sides and one of inch width for depressing the rectum. My knives are slightly bent on the flat, an inch from the point, and only differ

from those figured by Simpson in being double-edged. One has a lancet point, the other has the angle rounded off; the former is useful in commencing the incisions, the latter in continuing them. However, if such knives are not at hand, any pocket-case will supply the want; for it is not so much the implement as the hand that guides it that secures success; and all surgeons ought to be able to do their work with as few implements and as simple as possible. A good sharp scalpel will do the work as well, if no more convenient knife should be at hand; and a few common iron spoons, properly bent, will make divaricators at a pinch. I have even known an operator pass sutures with common curved needles held in a dressing forceps, when those with which he had supplied himself became bent or broken in the progress of the operation.

Lastly—My operation is suitable to almost every case, and to many cases which could not be subjected to the older methods with the smallest chance of success.

In a large gap, where the loss of substance is to be measured by square inches, no person could expect union by simply paring the edges and drawing them together by the interrupted suture. The strain on the threads would be too great, and they would inevitably cut out. Nor is the success of autoplastic operations, by which flaps are transplanted from neighbouring parts, such as to lead us to expect much from them. In these cases it is of great importance to have a mode of operating which can be frequently repeated without repeated diminution of the already scanty material. In more than one instance I have succeeded in gradually reducing the aperture to moderate dimensions by a succession of operations, and finally have closed it completely. I have invariably included the entire margin in the first operation, as it is impossible to say what part or how much may unite at once. The advantage of this appeared in one case, by the almost complete closure, at one operation, of a fissure which measured three inches in length; a single point only remained open, and this subsequently contracted so as to cause no further inconvenience. In another case, one-third of a large gap closed at the angles; and a subsequent case of almost equal dimensions was rendered manageable by the centre portion uniting firmly, and so subdividing the fissure into two portions, at which I was enabled to work alternately without loss of time as well as without loss of substance.

In small gaps, on the other hand, it will not redound to the credit of the operator, if the rent is made worse each time that he interferes. Such a misfortune cannot happen by my operation in any case to which it is suited. There is no loss of substance; and the surgeon can begin again *de novo* in a few weeks, with the parts in at least as favourable condition as before.

In comparing it with Bozeman's method of operating, it will be found to possess both advantages and the opposite; but these will be more intelligible when I have explained, as I shall now do, the treatment of a case subsequent to operation. The vagina is washed out with cold water; a catheter is passed for a moment to empty the bladder of any few drops of urine that may have collected, and it is then withdrawn until the patient has been placed in bed. If the fistula inclines more to one side than the other, she is placed on the side that is farthest removed from the fistula, with instructions to the nurse to keep her from turning over on her back, as patients are apt to do while recovering from chloroform. As soon as she is conscious, she is similarly cautioned; and is further advised to lean over as much on her face as she can. In this position she is kept for at least two days. The catheter is again inserted immediately on her being placed in bed, and a small vessel laid beside her to catch the urine as it drops from her. I have used various catheters, and find the ordinary male gum-elastic the best, for more than one reason—it is long, soft, pliable, and you can insure its cleanliness by having a new one for each case. Further, I have found the urethra less impatient of it than of metal instruments. In any case, it should be carefully watched for the first day, lest a clot of blood should get into the eye and check the flow of water. In case the urine is not seen to drop from it, it should be gently drawn back and forward once; if the urine does not then flow, it should be withdrawn entirely, cleared out and re-in-

troduced. At the end of two full days (forty-eight hours), the patient may be turned carefully on her back, the bed having been wheeled opposite a good light, and with the fingers, or with the gentlest and most cautious use of one or two divaricators, the quills and wound may be brought into view with the smallest amount of disturbance possible. If the wound looks healthy; if the flaps are not blue from strangulation (over-tight sutures); if the quills appear not to have corroded the mucous membrane on which they lie, the fingers and divaricators may be carefully withdrawn, the patient turned on the opposite side, a fresh catheter introduced, and all things left as before.

The next day it will be well to repeat the examination, and, if necessary, to cut the loops of the sutures, or at least of the alternate ones. This is best done by touching them with a sharp knife as they cross the quill, cutting them on the quill in fact, and using no force, no pulling or dragging of the quills, and no too curious inspection to see if union is perfect. On the next day the remaining sutures (if any) are to be divided, and one quill removed if loose; and on the succeeding day the remaining quill and sutures will probably be found loose in the vagina, and may be removed. At each inspection it may be well to syringe out the vagina with tepid or cold water. After the third day the catheter had better be withdrawn, when the urethra will be found patulous. The catheter may be introduced morning and evening, to insure the complete emptying of the bladder; or at any time, if the urine should be felt or seen to cease from flowing. I have found, as a general rule, that it irritates the urethra so much as to induce suppuration, if the instrument be kept in longer than three days; and the suppuration is apt to extend toward the wound, and may even give rise to subacute cystitis. Hence I recommend and practise its early removal. After the quills have been removed, great care is required for two or three days, lest any accident should rupture the soft union. Vomiting, diarrhœa, incautious movements, and accidental injuries by the catheter or syringe, might do irremediable mischief; but without some such cause, there is no likelihood of the union giving way. At first, a thickish ridge will mark the site of the line of union, and two lines of raw surface will show where the quills had lain; but in a very few days these will have healed up, and the former will have disappeared. And I think that we may fairly conclude that the ridges on the vesical surface follow the same course. For a variable period incontinence of urine will probably exist. Of this there are two causes—the patulous condition of the urethra, and the loss of dilating power in the bladder. Both are remediable by time and patience; a few words of advice to the patient to make water often at first, and by degrees to try and retain a little more in the bladder, will counteract both causes of incontinence. For obvious reasons, the patient should be kept in hospital, or at least should not be allowed to return to her household duties until the cicatrix has acquired complete solidity; or if such arrangements be not practicable, strong cautions should be given to all parties concerned to avoid for a time everything which could injure it. An example of the mischief of premature coitus will be found in one of the following cases, where, from the circumstances of the case, the usual prohibition had not been given.

As stated above, there are points in which my operation and Bozeman's may be advantageously compared with one another. Where a surgeon possesses judgment and nicety of touch, he will find my operation afford him excellent results in most cases. He requires both qualities for deciding on the depth to which the splitting process should be carried, the degree of tightness with which the ligatures should be tied, and the moment when to relax them. Bozeman's operation is more mechanical; and though complicated and troublesome, when once the details are mastered, and the apparatus is bought, it needs dexterity of hand alone to insure reasonable success. For reasons I have given a few pages back, it is as little applicable to large rents as the older method, on which it is an ingenious improvement. Its great advantage—and to my mind a very moderate one—is the comparative safety with which the sutures can be removed. The shield protects the

cicatrix from injury at the moment of their division; and as they cause no irritation, they need not be removed for several days, until all is perfectly firm. My operation is done much more quickly, and has been completed in periods varying from twenty minutes to an hour and a quarter. I have heard of Bozeman's occupying three hours and a half. There is, however, a form of fistula in which I prefer and always perform the latter—where the edge of the fistula is funnel-shaped and puckered. Such fistulæ are small, and generally high up. The condition of the margin makes it almost impossible to apply my plan of splitting; and I find it more expeditious and safer to pare away the mucous lining of the fistula and its funnel-shaped mouth, and to insert wire sutures, and apply Hardy's modification of Bozeman's button. In this we have two parallel rows of holes close together; and the ends of each wire being passed through two neighbouring holes, are simply twisted together, instead of being clamped with shot. I have even applied this plate to the palate in a case of syphilitic loss of substance, in the hope that it would effect a closure. The case was not favourable for success, and only partial improvement followed, but sufficient to show me that most important assistance could be had from a well-adapted button, in suitable cases of fissured palate.—*Dublin Quarterly Journal, May*, 1861, *p.* 302.

118.—ON SECONDARY ABSCESS WITH DISORGANIZATION OF THE KNEE-JOINT, FOLLOWING SCARLATINA AND PARTURITION.

By W. Coulson, Esq., Surgeon to St. Mary's Hospital, London. (Read before the Medical Society of London.)

The author observed that the case, though incomplete, was interesting from the circumstances under which it occurred. The question raised was connected with the state of blood-poisoning. How far was the fatal pyæmia connected with the puerperal condition? or was it to be regarded as connected with an intercurrent attack of scarlatina? The question whether secondary abscesses are deposits of pus from the blood, or are formed by pus carried from the original fountain of suppuration, was, he believed, now settled; and it was generally accepted that the secondary and local affections in the various forms of pyæmia might be purulent or non-purulent. Thus, in puerperal pyæmia, one joint might be the seat of a copious deposit of pus, while the joint next to it was the seat of apparently common inflammation; and again, these simple inflammations (or at least apparently simple) were just as dangerous as the most copious deposits of pus, both indeed being local manifestations of one cause, and deriving their gravity from that general condition. Pyæmia was now a recognized disease. Formerly it was ignored under the head of "typhoid symptoms." It was now acknowledged to be by no means unfrequent. This, it was contended, depended on the fact, not that the disease was less frequent formerly, but that it was now better known. Who, for example, ever thought, some years ago, of attributing a fatal termination after lithotomy to pyæmia? yet it was now known that purulent infection of the blood was no uncommon cause of death, not only after lithotomy, but after lithotrity; and records of practice in the last century proved that pyæmia after lithotomy was just as frequent at that period as at the present day. Another interesting point established by a more careful and recent investigation of this subject was the fact that pyæmia, like other blood-poisonings, had its degrees of intensity, and not unfrequently put on a chronic form.

Mr. Coulson then turned to the details of the case. On the 23rd of October, 1860, a woman (Jane A.) was admitted into St. Mary's Hospital, labouring under an affection of the left knee-joint. She was thirty years of age, healthy, and of sound constitution. She had borne five children; and on the 27th of Sept. she had been confined of her sixth child, which was still-born. In a week she was nearly convalescent. At this time her children were attacked with scarlatina, and she slept with one of them. This occurred

on the seventh day after her confinement. On the eleventh day, she dressed and got up; but on returning to bed she was seized with excruciating pain in the left knee. Fomentations were first applied; then a blister; but nothing gave her relief. On the fifteenth day after the symptoms first appeared, she was admitted into St. Mary's Hospital, under the care of his colleague, Dr. Tyler Smith. During the whole of her illness she was watched by himself (Mr. Coulson). It was now found that an abscess had formed over the patella of the left knee. The abscess was opened; and subsequently a number of abscesses formed around the joint, and were also opened in like manner. The discharge from them was never very considerable. The patient stated that she had been very feverish at the commencement of the attack; that her throat had been sore, her tongue very red; and that the cuticle had been peeling off during the last month. A stimulant and tonic treatment was adopted from the first—nourishing food, wine, brandy, bark, and ammonia. It was not attended by any marked improvement, though it seemed to retard the progress of the disease. The left lower extremity, however, became gradually contracted; and on the 21st of January last, had assumed a semi-flexed position. Hardly any movement of the joint could be effected; and from the appearance of the parts it seemed probable that secondary dislocation of the tibia backwards had taken place. From this time until the patient's death, which occurred on the 3rd of March, no change particularly worthy of notice took place.

Unfortunately her friends would not permit a post-mortem examination to be made. The body was greatly emaciated. The left lower extremity was flexed, both at the knee and hip-joints. The limb was rotated inwards, and adducted at the hip. On the inner side of the knee-joint was a large sinus, through which could be felt the inner condyle of the femur, denuded of its periosteum. The sinus, however, seemed rather to pass amongst the muscles than to communicate with the joint.

The question naturally arises—What was the nature of the disease, under the influence of which this rapid destruction took place? It could not be attributed to mere local inflammation. Mr. Coulson inferred that the disease had its origin in blood-poisoning, and next inquired, in what manner was the blood contaminated? There was here a choice between two causes, either of which was sufficient to explain the effect produced. The patient had been recently delivered, and it was well known that secondary abscesses and disorganization of joints occasionally occur after parturition. But the patient also laboured under a disease which is occasionally followed by blood-poisoning with its effects. She had undoubtedly been affected by scarlatina, and some would probably consider this as the source of the secondary affection.

Scarlatina, according to the best authorities was often accompanied by a mild articular affection—by synovial inflammation, which closely resembled rheumatic inflammation of the joint, and soon passed away. This form had been described by Dr. Copland, Dr. Watson, Dr. Williams, and more recently by Dr. Willshire; but a more severe form was from time to time observed. Some five or six cases of the kind were on record. There were two forms of arthritis connected with scarlatina—the simple form, and the secondary suppuration of blood-poisoning. The same kind of secondary suppuration might be observed now and then after small-pox, and still more rarely after measles. But the connection was incidental. Scarlatina and small-pox did not give rise to these severe attacks of arthritis in their character as exanthematous diseases, but because they were occasionally attended by local suppurations. The same effects were observed in certain cases of glanders, of dissecting wounds, &c., where the secondary abscesses and disorganization of joints did not occur, unless they had been preceded by suppurative inflammation of the absorbents, by phlebitis, or by some local deposit of pus. In the case of A., Mr. Coulson was inclined to conclude that, although the secondary affection might have found its origin in the scarlatina, it more probably was connected with the puerperal state. The disease, then, under which the woman died was chronic pyæmia. This was

a form which he had frequently observed, and in the investigation of which he was an early labourer. It did not necessarily terminate in death, because its chronic character might depend on the smallness of the quantity of poison taken into the system, or on the great resistance opposed by nature; but the cause, course, and symptoms of the complaint could leave little doubt as to the family to which it belonged; yet puerperal women were liable to these attacks of purulent inflammation of the joints, just in the same way as patients were liable to the same accident after injuries of the head, amputation, lithotomy, or other operations and injuries. For a long time the cause of death was not understood in these cases, and the origin of these purulent deposits quite unaccounted for. Various hypotheses were started; but nothing based on true scientific observation was established until Mr. Arnott took up the subject, and investigated it in a complete manner. Since then many new observations had been made, and general accordance now prevailed on the three following points—

1st. The poisonous agent in these cases is pus.

2nd. The general symptoms under which the patient suffers or sinks are produced by the admixture of this pus with the circulating fluid.

3rd. The special lesions or secondary deposits found after death are the effects of the general poisoning of the blood.

The author would, however, go further, and say that any disease (as well as lesion) accompanied by the formation of pus, might give rise to the poisoning, provided there was a way for the poison to enter the circulation. The fact was, that in a majority of cases it was not the pus from inflamed cellular tissue which entered the circulation, but pus secreted by the veins in or near the part primarily affected. The immediate cause, in a word, was phlebitis. The train of events was briefly as follows:—First, secretion of pus—a drop or two would suffice. This pus, or some poisonous portion of it, either passed into the circulation or excited suppurative phlebitis somewhere. In either case there was contamination of the blood, with its dangerous and almost always fatal effects—typhoid fever and severe local disease. But great variations occurred both in the course and intensity of the general symptoms, as well as in the secondary effects. The exciting cause was a morbid poison, and it was known from experience—in syphilis, for example, how much the effects and course of such poisons were liable to be modified, partly by the quantity taken in, partly by the constitution of the patient or the weakness of the organs. Thus the poisoning might assume a chronic form, or it might be made up of a succession of subacute attacks, depending probably on successive introduction of pus. In such cases the general symptoms were irregular and obscure. In some, the first symptom had been a violent attack of one or more joints.

Several years ago the author published some fatal cases of articular disease in puerperal females, and even at that early period expressed an opinion, "That contamination of the blood appeared to him the most probable explanation of the varied phenomena of these affections." That opinion need only be modified by adding the word "pus," to be correct according to present views. These attacks were not common or ordinary arthritis. They were developed under circumstances quite different from those which produce other forms of articular disease. The inflammation was often periarticular, and the pus confined to the exterior of the joint. In other cases we had copious effusion of pus into the joint without any trace of inflammation whatever. This was not ordinary disease. The formation of pus bore no relation whatever to the intensity or duration of the articular disease, as was the case in other affections of the joints. Besides this, the mere serous form, taken by itself, was just as dangerous in its results as the purulent form. Lastly, the constitutional symptoms accompanying these attacks bore no relation to, and could not be explained by, the arthritic disease, if regarded as common inflammation. We must look, then, deeper for the cause of these affections of the joints in puerperal women, and consider in each case whether the disease be of an ordinary kind, or one which must be referred to blood-poisoning. Articular disease appeared in puerperal women

under three circumstances—viz., during puerperal fever, after convalescence from the fever, or after parturition without its having been preceded by fever or other manifest disease. In all cases the disease was the same, though the symptoms were always more or less marked by those of the puerperal fever when that complaint existed. The secondary joint affections were the same under all these circumstances. They might be purulent, articular or peri-articular, acute or chronic. These differences likewise existed in cases of joint disease arising from blood-poisoning after amputations, &c., and this was a very important point to notice. Finally, then, why did he infer that these articular affections of puerperal women had their origin in purulent infection of the blood? Because their symptoms, course, termination, effects,—in a word, all the laws of their manifestation,—were precisely the same as those which prevailed in other cases where the influence of pus-poisoning was universally admitted. Besides, in a majority of cases, pus had been discovered in the uterine veins or lymphatics, or in some of the tissues about the uterus. In chronic cases this original source of the poisoning might not be discovered; it might have disappeared, but it was surely more in accordance with the spirit of philosophy to refer similar effects to similar causes rather than to seek in unknown influences the explanation of a few exceptional cases where direct agency might have escaped detection.

Mr. HENRY LEE considered that of all the blood-poisons, none was so deleterious as decomposed fibrin. It will, he said, produce much more formidable symptoms than the introduction of any amount of healthy pus; and he believed that the secondary abscess and typhoid symptoms in the case related by Mr. Coulson, and which had terminated fatally, were to be referred to this cause. After parturition, the healthy veins were blocked by fibrin; scarlet fever then attacked the patient; the healthy action of the occlusion of the vessels was interfered with; a portion of fibrin became decomposed, and was carried into the circulation. He suggested as a question worthy of consideration, whether other kinds of inflammation besides those of a purulent character might not also be followed by their own peculiar secondary inflammations.

Dr. RICHARDSON thought that in these cases of purulent inflammation it was a question—first, whether there was a preliminary state in which pus was formed in an organ or tissue previous to the blood-poisoning; or, secondly, whether the change took place primarily in the blood he did not believe in the absorption of pus, and considered that the change originated from within, and not from without; and that pus formed outside the circulation and did not enter into the blood. The mere injection of healthy pus into the blood will not produce blood-poisoning. One drachm of pus when thrown into the circulation will not often effect a dog. But supposing the cause to occur from within, and looking on an increase of fibrin in the blood as productive of the peculiar class of inflammations as described by Mr. Coulson, we had in puerperal fever and scarlet fever increase of this element. In puerperal fever there was as much as 9 parts in 1000; and in scarlet fever, 4 in 1000. Hence in these affections depositions of fibrin were liable to take place. He thought that in all probability pus was formed out of fibrin; and he regarded in these cases the formation of abscess as beneficial.—*Lancet, Oct.* 19, 1861, *p.* 375.

119.—OPERATION FOR AND PERFECT CURE OF PROCIDENTIA UTERI FROM PERINEAL LACERATION OF MANY YEARS STANDING.

By Dr. D. LLOYD ROBERTS, on the Medical Staff of St. Mary's Hospital for Diseases of Women and Children, Manchester, &c.

Jane W., aged 49, married at 20; admitted an in-patient of St. Mary's Hospital on November 24, 1860; she has had eight children all born alive at the full period of utero-gestation. The menstrual crisis commenced at 13,

without any derangement of health—was always scanty and light-coloured, and accompanied by severe dysmenorrhœa. She has always been subject to a yellow leucorrhœal discharge since her marriage. The perineum was lacerated in her third labour, now many years ago, and she attributes the falling of her womb to this circumstance.

Present Condition.—The uterus is completely procident and hanging between the thighs; the inverted mucous membrane of the vagina, which now covers the womb, has assumed almost entirely the character of integument; there are two patches of deep-seated ulceration on the sides of the anterior surface, which are ragged and unhealthy, and encroach on the lips, which are enormously hypertrophied, very vascular, excoriated, and in a state of intense inflammation. The surface of the os and cervix is cracked in two or three places, and a free discharge of blood oozes from the fissured surfaces. The uterus can be returned, though with difficulty, and any attempt to reduce it causes extreme pain.

In consequence of the weight of the uterus, the bladder is pulled down in front of it, and the meatus directed upwards. There is great irritation of the bladder, evinced by frequent desire to void the urine, which is high-coloured and ammoniacal; the perineum is lacerated, the greater portion of the sphincter ani being included in the rent; and when the bowels are acted upon unduly, as when under the influence of medicine, she loses all control over the passage of the fæces; the orifice of the anus is much dilated and relaxed, and its mucous membrane everted; upon the latter are three small piles. She also complains of dragging pain over the lumbar vertebræ, which is increased on pressure, and extends also down the front of the thighs. There is symptomatic fever, furred tongue, quick pulse, &c; she is unable either to walk, sit, or stand, and frequently complains that "life is a burden to her." These distressing circumstances, in addition to the miseries arising from poverty, induced me to recommend her to become an in-patient of St. Mary's Hospital.

The treatment, after admission, consisted in the application of soothing lotions to the uterus, so long as inflammatory action remained; grey oxide of mercury, suspended in glycerine, to the ulcers; tonics, as quinia and iron, hydrargyri bichloridum cum cinchona, to reduce the hypertrophied uterus. This treatment was persevered in for seven weeks, at the end of which time the ulcers had healed, and all inflammatory action had ceased. I now turned my attention to the replacing and retaining the uterus in its natural position. I proposed to effect this by the restoration of the perineum, and contracting the calibre of the vagina, by removing slips of mucous membrane from the latter.

Accordingly, on January 5th, 1861, I proceeded to perform the operation, chloroform being administered by my friend, Mr. Runcarn. The steps of the operation were as follows:—The patient having been placed in the position for lithotomy, I passed a needle, armed with a double ligature, through a portion of the mucous membrane on the lower part of the uterus, for the purpose of making traction upon it to facilitate manipulation; I dissected off a triangular slip of the mucous membrane covering the posterior surface of the uterus; brought the edges of the wound together by silver wire; removed a similar piece from the anterior surface, and drew the edges together in the same manner; I now pared the margin of the perineum for half an inch in thickness and two and a half inches in length, brought the edges together by passing two silver hare-lip pins through the whole substance of the perineum, from without to within, and from within to without, after the manner recommended by Mr. Spencer Wells for bringing together the abdominal parietes, after ovariotomy. The operation occupied one hour and a half. The wound was dressed and secured by a perineal bandage, and the patient put to bed. She had a good sleep, and awoke refreshed at four o'clock p. m.

The progress of the case was very favourable, and quite free from bad symptoms. On the eighth day the hare-lip pins were removed, and I was gratified to find that perfect union had taken place along the whole of the united edges, with the exception only of a very inconsiderable portion. This

afterwards left the appearance of a small aperture in the perineum. The ligatures of silver wire on the anterior and posterior surface of the uterus came away at the end of the third week.

The patient was kept for some time rigidly in the recumbent position, to secure strength and permanence of the union of the parts; and was finally discharged on April 20, 1861.

Remarks.—I attribute the successful termination of this case to the following circumstances:

1st. To the strict maintenance of the recumbent posture, both before and after the operation, and also to the therapeutic influence of the hydrargyri bichloridum in reducing the hypertrophied condition of the uterus. I learned the use of this remedy from an excellent paper by Dr. Oldham, in the sixth volume of Guy's Hospital Reports, and have verified its efficacy in a great number of cases which have occurred under my care at St. Mary's Hospital.

2ndly. To the removal of a *sufficient quantity* of the mucous membrane from the vagina to secure its more perfect contraction. Some previous operations of mine have had only partial success, from want of this precaution.

3rdly. To the use of silver hare-lip pins, instead of the ordinary quilled suture: the advantages are, greater facility in use, and a firmer and more secure apposition of the parts.—*Dublin Quarterly Journal, August*, 1861, *p.* 19.

120.—ON ABDOMINAL COLLAPSE.

By Dr. Robert Barnes, Assistant Obstetric Physician to the London Hospital; Physician to the Royal Maternity Charity.

[Shock from severe injury is a common case observed in surgery. It may be noticed also in other instances besides those of external injury; it may occur, for example, from the injury done to the peritoneum and abdominal viscera, when an ovarian cyst is extensively adherent to the neighbouring parts. Dr. Barnes says:]

We see severe injuries inflicted upon a moderate extent of peritoneum, such as surgical incisions for the relief of herniæ, or in the Cæsarean operation, or for the excision of ovarian tumours, make a surprisingly feeble impression upon the nervous centres. But the case is very different when some untoward complication arises that exposes a large superficies of peritoneum to irritation. If, in attempting to remove a diseased ovary, the tumour prove to be extensively adherent, then must an enormous surface be subjected to bruising and delaceration by the hand of the operator; or if, by maladroitness or accident, even a small quantity of the contents of the cyst escape into the peritoneal cavity, the irritation may be fatal. I believe that this is the explanation of a considerable proportion of the deaths from ovariotomy. The fluid of ovarian cysts seems in some cases to possess a peculiarly irritating quality; there is every reason to believe that it may have a malignant property. The sudden effusion of such a fluid over the entire surface of the peritoneum, whether attended by an incision through the abdominal walls or not, is pretty sure to be resented by the system. This illustration will introduce us to the subject proposed for clinical inquiry.

In January last, I met Mr. Nicholson, of Stratford, in consultation upon the following case:—A lady, aged forty-two, had borne six children, and was at the date of our meeting in the fifth month of pregnancy. She had gone on well until three weeks ago, when she received a kick on the abdomen from a child in bed; she could not fix the spot where the blow fell; she had a little pain afterwards. On the 17th of January, being about eighteen days after the accident, she was seized rather suddenly with great pains in the hypogastric and lumbo-sacral regions; shivering, prostration, and fever followed successively. Twelve leeches were applied on two occasions to the hypograstrium with relief, and calomel-and-opium was administered. When I saw her on the 20th, the pulse was 120 and weak; she was lying on her

back, the knees drawn up; sighing, or any deep inspiration, increases the pain, which "shoots" from the lumbo-sacral region forward to the groins and pubes; there was excessive tenderness on touch, increasing on deep pressure, especially in the right iliac region; her voice was good, and the countenance not much depressed. The bowels had been relieved two days before, and had been kept quiet since.

Now here we had two leading conditions before us—first, shock, and prostration; secondly, some degree of reaction, attended with the ordinary symptoms of peritonitis. What was the cause of these conditions? We shall see by-and-by that there is a whole catalogue of accidents that might produce them. Some of these accidents are, indeed, of rare occurrence; but all the possible events must be present to the mind of the physician if he seeks to arrive at a correct diagnosis in an emergency of this nature, and would hope to bring relief. In the first place, attention would naturally be directed to the pregnant uterus. Had this been ruptured, as has sometimes happened in the course of gestation? A careful exploration by the vagina, and of the fundus through the abdominal walls, satisfied us that the fœtus was still contained in utero, and that the amniotic sac was uninjured. We excluded the idea of rupture of the uterus. The diagnosis given was, that a sudden effusion of blood had taken place into the peritoneum. The source was doubtful. It might proceed from a rupture of the Fallopian tube, or from the ovary. In any case it appeared to have caused peritonitis. Opium was prescribed, and an ointment, consisting of mercurial ointment and belladonna, was directed to be smeared over the abdomen. The result, which is very instructive, I afterwards learned from Mr. Nicholson, who wrote as follows:—"Mrs. J—— lived about a week longer after your visit; throughout, vomiting was excessive, and the pain only remitted at short intervals. She gradually sank exhausted, unable to retain any kind of food. I made a post-mortem examination. The body had decomposed more rapidly than ever I have seen. It was a most frightful spectacle of corruption. To my surprise there was no effusion of lymph or pus; in fact, the only evidence of inflammation of the peritoneum was an increased redness in some parts, but the shining character of the membrane was not impaired. I found a large cyst extending to the under surface of the liver, and discoloured by contact with bile. It proved to be the right ovary; it had burst into the peritoneal cavity; it had contracted no adhesions, and its surface was smooth and shining. A thick, treacly sanguineous fluid was still escaping, and I had removed perhaps, a pint of sero-sanguineous fluid previously. The tumour was multilocular, and contained several smaller cysts within the larger envelope. This was dense and thick near the ovary, but thin and yielding upon the slightest force at the loose extremity. The fimbriated extremity of the Fallopian tube was natural, admitting an ordinary probe for some inches. There was a corpus luteum, with its cavity all but obliterated. The uterus was perfectly healthy, containing a five months' fœtus; the membranes were entire, the placenta not separated, even partially; the os uteri closed by a thick mucus, but readily admitting the finger."

A remarkable feature in this case is the absence of the usual post-mortem evidences of inflammation. The patient had survived the first shock of the rupture of the ovarian cysts for at least ten days and yet there was no plastic exudation or purulent effusion. The persistent pain and vomiting, nevertheless, indicates, a continuous irritation of the peritoneum, which in the end destroyed the patient by what may be called protracted shock. From the extreme proneness of the body to decompose, it may also be conjectured that the blood had become poisoned, either by the absorption of fluid that escaped from the ruptured cyst, or by the disturbance of the blood-forming and blood-purifying functions induced by the shock.

Two questions must delay us for a moment before we pass on to other cases. Could the ovarian cyst complicating gestation have been diagnosed during life? Could anything farther in the way of medical or surgical treatment have been adopted? The co-existence of a gravid uterus and an ovarian dropsy is not unfrequent. I have known several women who carried

ovarian cysts of considerable size go through the pregnant state uninterruptedly more than once. In some of these cases it was possible to trace a furrow of demarcation in the general abdominal enlargement, suggesting the presence of either twins or of two cysts pressed together. You may, of course, infer that there is a twin-pregnancy if you can clearly trace through the abdominal walls the parts of two children, or hear two fœtal hearts beating; but it is not so safe to eliminate or set aside twin-pregnancy if you can detect only one fœtal heart. The history of the case may give you no assistance. The patient may be unconscious of having had any abnormal tumour in the abdomen prior to pregnancy. There is a peculiar cachectic aspect in patients bearing an ovarian dropsy, which may attract attention, but it is not always easy to distinguish this from the cachexia of pregnancy. If, however, we suppose a diagnosis to be established, shall we be justified in tapping the cyst or in extirpating it, in order to obviate the risk of its bursting under the influence of the double pressure to which it is subjected from its own growth and from that of the pregnant uterus? The case before us proves that the danger is a serious one. I have, I believe, seen it averted in another case by the spontaneous occurrence of premature labour. I think we may exclude ovariotomy during gestation. Circumstances may justify tapping; but I am of opinion that generally it would be the more proper course to terminate the pregnancy first by inducing labour, and then to deal with the dropsical ovary.

But ovarian cysts may cause death not only by discharging their ordinary contents into the abdomen, but also by hemorrhagic effusion. This point I will illustrate by a case, which is taken from the 'Lancet' of 1848. It is related by Mr. Pollard. A single woman, aged twenty-five, but not living in chastity, was taken on the 5th September with vomiting, and pain in the right side. When seen, her face was anxious; anæmic; pulse imperceptible; heart's action very feeble; extremities cold; respiration difficult; collapse complete. She died about twenty hours after the onset of symptoms. The abdominal cavity was found three-fourths filled with blood; there was enough to fill a large chamber-vessel. The uterus was slightly enlarged with a membrana decidua well marked. The uterine appendices and ligaments were much congested; in the right ovary was a distinct corpus luteum. There was a clot in the left tube containing a small cyst, but an ovule could not be made out. Near this clot was a small rent in the tube. The left ovary, as large as a small apple, was hollow; behind it was a rent a quarter of an inch long, in the lips of which was coagulated blood. This rupture had been the source of the hemorrhage. The immediate cause of death was shock complicated with anæmia from the sudden hemorrhage. There was in all probability a Fallopian pregnancy, as well as cystic disease of the ovary; and it is not a little singular that both the tube and the ovary should have been found ruptured together.

There is another way in which fatal collapse may ensue from ovarian disease. I quote a case related by Dr. C. Habit. A woman had aborted, when, without intermediate symptoms, nine hours later, acute pains in the abdomen, vomiting, collapse, pallor, and cold sweat appeared. Soon an ovarian tumour, which had been previously detected, was found to be larger, and its walls more tense. Collapse persisted, and thirty-six hours after expulsion of the fœtus the patient died. On dissection, the peritoneum, especially where it invested the cyst, uterus, and appendages, was injected, dulled, and covered with exudations. The cavity of the cyst, which occupied the left ovary, contained about two quarts of a deep-red fluid and little-altered blood.

Here is another instructive example of the danger attending pregnancy complicated with ovarian disease. Under the stimulus of gestation, the spermatic and uterine arteries carry a largely increased supply of blood; there is an active local hyperæmia. Under these circumstances rupture of vessels connected with diseased ovary readily occurs. In this case the patient died of shock, anæmia from loss of blood into the cyst, and general peritonitis.

Bleeding into the cavity of an ovarian cyst may also take place indepen-

dently of pregnancy, as the following case, related by Von Patauban, will show:—A pluripara, aged thirty-two, bore a fluctuating tumour in the hypogastric region, of the size of the uterus in the seventh month of pregnancy. When seen, this tumour was very painful on pressure: her extremities were cold; pulse small and intermitting; pallor. Diastolic after-rush indicated internal hemorrhage. The patient died from repeated attacks. On post-mortem examination, a large dark red tumour was seen emerging from the pelvis; it was stalked, and sprang from the left ovary; it was polycystic, and its cavities were filled with dark fluid blood, five pounds in quantity. The bleeding had arisen from apoplexy from vessels running in behind the tumour, the blood having been retarded through a twisting of the tumour on its axis, caused probably by a fall the patient had suffered shortly before the symptoms set in.

There is no mention here of peritonitis; the patient died of the combined effects of shock and anæmia from hemorrhage. The liability of ovarian cysts to become twisted on their axis, entailing rupture or strangulation of the vessels in the pedicle, must be borne in mind. Attention has been recently drawn to this accident by Rokitansky, who has described several cases.

It must not, however, be concluded that rupture of an ovarian cyst without hemorrhage is necessarily fatal. There are numerous cases on record of cysts bursting and discharging their contents into the abdominal cavity, the intestine, the bladder, or the vagina, in which recovery followed. Perhaps the issue in recovery or in death may sometimes be determined by the innocent or malignant character of the fluid poured out from the cyst.

An ovarian cyst may contain pus which, although not a malignant fluid, may be more irritating than the ordinary serous fluid of ovarian dropsy. Of this we have an instance in a case related by my friend Professor Faye, of Christiania. A woman, aged twenty-nine, had been delivered under chloroform by forceps of a living child. During pregnancy (her first) she suffered much from vomiting; and towards the end she had a fixed pain in the right side of the abdomen, and several convulsive fits. On the night after delivery she had severe pains, which were mistaken for after-pains. Next day these pains were more bearing-down; the abdomen was tender and tympanitic. She died fifty-three hours after delivery. On dissection, the Douglasian sac was found filled with a thin, purulent, sanguineous exudation. An abscess in the right ovary had burst; the remains of the organ had changed into a mere pulpy detritus. The left ovary was hypertrophied and œdematous. There was considerable degeneration of the cortical substance of the kidneys, and there were many extravasations of blood under the serous membrane covering the kidneys, liver, and lung.

In this case inflammation of the ovary, ending in suppuration, had occurred towards the end of pregnancy: the abscess had burst about the time of delivery. Death was the result of the combined operation of shock, peritonitis, and ichorrhæmia.

To show you the influence of pure shock, I will cite a case related by Dr. Lowenhardt, of death after injecting an ovarian cyst with iodine. A solution, consisting of two ounces of tincture of iodine, two ounces of water, and ten grains of iodide of potassium, was thrown into an ovarian cyst. As much was withdrawn as possible, after being allowed to remain four or five minutes. Collapse set in immediately, and the patient died in fourteen hours. The cyst was found collapsed; there was no trace of inflammation in the cyst or peritoneum. In the cyst was a pint of iodine-coloured fluid. There was also a small quantity of weakly-iodized fluid in the peritoneal sac. In this case it seems clear that death was due to shock alone; there was no inflammation. It can hardly be supposed that death was owing to the absorption of iodine. The quantity remaining in the cyst, or which escaped into the peritoneum, was too small. I have injected iodic solutions into ovarian cysts, and have seen iodism so intense produced, that even the saliva, the breath, and the perspiration yielded abundance of iodine, but the patients recovered from the operation.

In all the cases I have cited, there was disease of the ovary; but this

organ may be the source of a fatal hemorrhage into the peritoneum without being strictly speaking diseased.

I cannot on this occasion enter at any length upon the subject of what is called retro-uterine hæmatocle. It will be sufficient for our present purpose to call your attention to this affection as a cause of shock and collapse. Under certain conditions, most frequently connected with disorder of the menstrual function, a considerable effusion of blood may take place from the congested Graafian vesicles and the ovarian vessels; this blood falls into the utero-rectal sac of the peritoneum. If it be poured out slowly, it may set up a benign form of inflammation, the effect of which is to surround the effused mass of blood with plastic lymph, thus cutting it off from the general cavity of the peritoneum. This is called the *encysted* variety. The isolation of the morbid process to a limited area is favourable to recovery. In such cases the shock and collapse may be slight and transient. But if the effusion take place rapidly and in considerable quantity, then will the patient suffer from the combined shock of extensive peritoneal irritation and of hemorrhage, and from this there may be no rally to give the opportunity of a sequestrating peritonitis. This is the *non-encysted* variety, far more dangerous than the first. This is the form of retro-uterine hæmatocle that more especially concerns us now. The access is sudden, perhaps coming on in the midst of good health, and even suggesting the suspicion of poisoning. Intense abdominal pain, like that of peritonitis, occurs, the patient being thrown into the most violent distress and agitation. The symptoms closely resemble those of perforation of the intestine; but, in addition, we have the blanched countenance, the feeble dicrotous pulse of anæmia from hemorrhage. The belly becomes tender and hard, and dull on percussion. Collapse soon appears, and death, follows, not uncommonly in twelve hours, or less.

Such a case may be distinguished from those we have previously passed in review by the symptoms of internal hemorrhage being combined with the absence of any antecedent tumours in the abdomen, and with the presence of a tumour formed by the collection of blood behind the uterus, which may be felt in the roof of the vagina on exploration by the finger.

The cases which we have now passed in review are all connected with various conditions of the ovary. We have seen—

1st. That an ordinary ovarian cyst may rupture during pregnancy, and discharge its contents into the abdominal cavity, causing irritation, and perhaps inflammation.

2nd. That an ovarian cyst may be the source of a sudden effusion of blood into the peritoneal sac, causing anæmia, irritation, and perhaps inflammation.

3rd. That hemorrhagic effusion may take place into the interior of an ovarian cyst without rupture, causing irritation from sudden tension of the cyst, and anæmia from loss of blood.

4th. That an ovarian cyst containing pus may burst and occasion irritation and inflammation of the peritoneum.

5th. That a sudden impression upon the interior of an ovarian cyst may cause irritation and fatal shock, as by the injection of iodine solutions.

6th. That under a physiological stimulus blood may be poured out either from the surface of a healthy ovary or from a newly-burst Graafian vesicle into the retro-uterine peritoneal sac.

One thing may have struck us in hearing these cases; I mean, the apparently greater risk of rupture of a diseased ovary under the complication of pregnancy. This circumstance may have some weight in determining the question of the expediency of ovariotomy.—*Lancet, Aug.* 3, 1861, *p.* 105.

121.—ON DIPHTHERITIC ULCER OF THE UTERUS.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

[The real diphtheritic ulcer of the uterus is a rare and most intractable disease. It is scarcely noticed in works on diseases of women. Dr. Simpson incidentally mentions its existence. Mr. Ellis says:]

In the elaborate 'Clinical Researches on Ulceration of the Cervix Uteri,' by M. Boys de Loury and M. Costilhes, published in the 'Gazette Médicale' for June, 1845, we have the first account of the diphtheritic ulcer in this region:—"We are not aware that this (diphtheritic) form of ulceration of the cervix uteri has ever been described. Its characters are, nevertheless, of sufficient importance to deserve a separate mention, since it may be readily confounded with syphilitic ulceration, to which, at first sight, it has considerable resemblance. As in the case of diphtheritic angina, the complaint begins with redness and pain; but at this period there is no discharge. In a few days, however, the diseased surface puts on its characteristic appearance, which is that of *small, whitish, smooth, and shining patches, of different and irregular forms.* These patches are extremely adherent to the cervix uteri, so much so that the most forcible injections, or even friction with lint, fails to detach them. At the end of a few days, however, they fall off spontaneously, leaving an ulcer of a simple aspect, which cicatrizes with the greatest facility." From the latter part of this statement would I record my own dissent; for I believe no fact better established as a result of experience than that the diphtheritic ulcer is one of the most intractable lesions the obstetric practitioner has to deal with. And I am much disposed to believe either that these physicians have confounded aphthous ulceration of the cervix with diphtheritic, or that—which is more probable—they have lost sight of their patients before they were really cured; for it is a characteristic of this disease apparently to disappear for a time, and then to return again and again with great inveteracy. The diphtheritic ulcer is also casually noticed by Becquerel in his recent work 'On Diseases of the Uterus.'

The existence and characters of the diphtheritic ulcer can only be clearly determined by the use of the speculum. But there is a certain slightly rough and dry sensation communicated to the finger, which, passing over the otherwise polished surface of the os, comes in contact here and there with a diphtheritic patch, which is very peculiar, if not diagnostic. This sensation of the touch is wholly different from that of the ordinary ulcer, the surface of which is lubricated with the viscid secretions of the cervix, giving a feeling of a slippery and granular character. There is some heat of the vagina, and an absence of the thick mucous discharges of commoner ulcerations; but there is a thinner discharge, probably of an acrid character, issuing from the cervix, and moistening the vaginal walls. The os gapes, and its sides are seen to be deeply ulcerated, the surface of the sore being coated with a whitish membrane. Here and there the membrane has been removed, probably by the point of the speculum, or by a sort of desquamation, and a raw, vascular, bleeding surface is uncovered. One characteristic of the diphtheritic ulcer is that, unlike the ordinary kinds, it exists independently of the ulceration of the os uteri. All the commoner forms of uterine ulcer appear simply as an extension of the ulcerative process, commencing within or at the edge of the os. This, on the contrary, has an insular existence, and patches of white false membrane are visible, scattered here and there over the cervix, of various sizes, from that of a split pea to that of half a hazel-nut. These patches are quite white, have an irregular, sometimes serrated, edge, are not easily rubbed off, but when removed by the forceps, leave an eroded and bleeding sore behind them. If the practitioner think to treat this disease as he would aphtha of the mouth, or diphtheria of the same region, and pencils it over with lunar caustic, he (and his patient) will infallibly repent it; for every touch of the caustic will be the birthplace of a new sore, and lead to a fresh eruption of false membrane. There is no evidence

of the ordinary duration of this ulcer, and it might appear possible that it may ultimately terminate in malignant disease.

The constitutional symptoms are not to be distinguished from those of the severer forms of ulceration. There was in one well-marked case, which I will presently narrate, much pain felt night and day in the hypogastric and lumbar regions, and this pain was described as of a peculiar, dragging, tearing character. There is much painful sensibility of the internal parts on examination, and a sense of great weakness after exercise. Menstruation is irregular, the tongue is coated, bowels constipated, and there is a manifest vice in the constitution, which it is difficult to express by mere words. I think it probable that this disease will be most frequently met with at the close of the functional life of the uterus. Itself the evidence of a diminished vitality, it seems unlikely to occur in the youth or middle age of the menstrual function, and still less in the activities of the pregnant state.

The name of diphtheritic ulcer has been retained by me throughout this paper, not necessarily because I consider it analogous to diphtheria of the buccal and pharyngeal mucous membrane, but simply for convenience, and because it with some accuracy defines a disease which, if not specific, is certainly very peculiar, and deserving of closer investigation. I am well aware that diphtheria of the vulva, occurring co-existently with the invasion of other parts by the same disease, has been observed and recorded; but these cases have no relation to this disease. The diphtheritic ulcer is a localized malady, and, so far as I know, has no tendency to diffuse itself beyond the canal of the cervix, the margins and vicinity of the os, and *possibly* the upper portion of the vaginal passage. I can only characterize it as a very intractable sore, not secreting pus, but exuding a plasma, which takes the form of a false membrane, that after a time desquamates, and is renewed again. The colour of the false membrane is nearly white, and herein it differs from the ordinary colour of fibrinous exudations. This I believe to be due to the exclusion of the oxydizing influence of the air, and also possibly to the effect of the acidity of the secretions of the vagina, which, as Dr. Tyler Smith has well shown, have the property of coagulating albuminoid discharges, and turning them into an opaque white substance. I believe it to be a distinct affection from herpes and aphtha, the only eruptions with which it could be confounded—first, by reason of the peculiar character of the false membrane formed on the surface of the sore; secondly, because of its extreme intractability and rebellion against ordinary treatment; and thirdly, because of the obvious dyscrasis of constitution, of which it is, if not an exponent, at any rate a significant expression.

The treatment of this form of ulceration will probably perplex the practioner who meets with it for the first time in no ordinary degree. Any application of nitrate of silver in the pencil, or of the ordinary milder escharotics, will by the next examination have produced such an aggravation of the disease, and so great an extension of it, as at once to suggest the doubt as to whether this be really not a malignant instead of a simple and curable disorder. Dr. Tilt, whose casual notice of its existence I can only discover in his Notes on the Treatment of Ulcers in this region (published in the 'Lancet' for Feb. 23rd, 1861. p. 185), says, with much truth—"In diphtheritic inflammation of the neck of the womb and of the vagina, nitrate of silver acts as a poison. In a case now under treatment there is a small patch of false membrane on the posterior lip of the os uteri, and around it are numerous ulcerations. Were I to touch them with nitrate of silver, they would soon be covered with false membranes. Tincture of iodine would not produce this effect, neither would the potassa fusa c. calce; these therefore, are the best means of curing this most tedious complaint, of which I have seen two instances, and Dr. Bennet three, and he would endorse what I affirm of such cases." This corresponds precisely with my own experience, and I cannot too strongly urge the practitioner to pause before he applies any of the ordinary escharotics of the milder sort to the treatment of this disease.

I believe that an exclusively local treatment is nowhere less successful than in the disease before us. There is a deep-lying fault in the patient's

constitution, and the effort must be made to rectify that co-existently with the absolutely indispensable use of local means. Iron, quinine, and chlorate of potash, present themselves as the remedies for the restoration of the general health; and an occasional course of cod-liver oil will be useful, if the digestive powers can be got up to the point of its easy assimilation. The local means employed at first must be either very weak or very powerful. Nitrate of silver and the acid nitrate of mercury are positively injurious in the *early* treatment of the disease, but at a later stage they expedite the cure. Weak injections of alum may do good to a certain extent, and as a preliminary application; but a cure can only be effected by either the potassa fusa or the actual cautery. Several years ago, in the treatment of a case I was wholly baffled, and almost in despair, at witnessing the evil results of ordinary cauterization; and at length, finding the patient was rather the worse than the better for all my diligence, I resolved to apply cauterization by the electric wire. It was at first used over only a small surface, the wire being at white heat from the electric current evolved by a powerful Grove's battery, and lightly drawn over the ulcer. The most gratifying and rapid amendment followed this experiment, and it was consequently repeated once or twice more over all the diseased portions until the false membranes disappeared, and a healthy sore was left, which was then cured by very mild applications of nitrate of silver.

I believe the whole secret of the successful local treatment of this disease lies in this: that the vitality of the diseased surface must be first profoundly modified, as it can only be by either the actual cautery or potassa fusa, and its tendency to throw out this plastic exudation destroyed, and then the case gets rapidly well with very little further assistance. But when the disease has deeply penetrated into the os and canal of the cervix, it will obviously require unusually careful management thoroughly to eradicate it. The determination of this important point of treatment of diphtheritic ulcer, which was published by me several years ago in a little tract on the 'Electric Cautery,' is, I consider of great practical moment; for it is calculated to save the practitioner a vast amount of useless effort, and the patient, of suffering. As the potassa fusa or potassa fusa cum calce is a far more convenient escharotic than the electric cautery, and is equally efficacious with it, if carefully used, I should recommend its adoption in all similar cases.—*Lancet*, *July* 6, 1861, *p.* 6.

122.—ON INFLAMED ULCER OF THE UTERUS.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

The indiscriminating association in modern works of the term "inflammation" with "ulceration" of the neck of the womb, has been, in my opinion, a real mischief to uterine pathology. Inflammation may or may not have preceded, or co-exist with, the ulcer; but in many cases presenting themselves to notice all active inflammation is long since spent, and a condition of mere atony and congestion comes to characterize the sore which once took a higher pathological degree. Of this class is the fungous ulcer of my last article. The truly inflamed ulcer is, however a common disease; and it is, as its name imports, an ulcer itself the seat of inflammation, and seated on an inflamed basis. I am well aware that up to this time a very decided repugnance has been shown towards the classification of uterine ulcers, founded to a certain extent on the assumed little practical advantage of such distinctions. But it is really of the greatest practical importance to distinguish the diphtheritic or the fungous from the inflamed ulcer, since the whole question of their cure turns upon the right discrimination of the mode of treatment proper for each; and in my judgment the same remark applies to other phases of the ulcerated diseases of the os and cervix.

The inflamed ulcer, in its most characteristic form, is more common in women under than above thirty. Taking that age as the middle period of

uterine activities, the inflammatory character of disease in this region begins thence slowly to lose its intensity, and gradually sinks to the vanishing point soon after the cessation of the menstrual function. This is a mere general expression of the fact; there are, of course, many exceptions. By the inflamed ulcer, I mean, an ulcer of the cervix, irritable, red, with small granulations, hot and tender to the touch, resting on a basis which is also red (though not *so* red), swelled, hard, and sensitive. In these marks may be recognised a state of inflammation which has sunk through the mucous and subjacent tissues (supposing the ulcer to have arisen as a result of laceration in pregnancy or from other causes), and pervades the general structure of the uterine neck; or, on the supposition of a centric origin to the ulcer, we are to regard the whole organ (the cervix) as affected with interstitial inflammation and deposit, and the ulcer resting upon it and partaking of the character of its disorders. I believe the inflamed ulcer capable of taking origin out of either of these two conditions. The inflammatory process rises no higher than to the sub-acute, at which point it may remain stationary, with periodical exacerbations; and in process of time it begins to decline, and in the indolent ulcer we have the next stage of the metastasis. Between these phases of ulceration there are of necessity grades of transition, but this is, so far as I have observed, the most ordinary history of the disease. It is so much the less necessary for me to particularise the symptoms of this disorder, as they have been so often and so well given by others, and especially by Dr. Bennet. My purpose is to establish the position of the inflamed ulcer, so that—as is most necessary and important—it may not be confounded with other and later aspects of disease in this locality. I shall therefore merely take down a typical case—such a one as just before writing this article has presented itself to notice, and while its features are still fresh to mind they shall be transferred to this page.

The sufferer is twenty-two; is tall, thin, very pale, and has fair hair and skin; her face is drawn and anxious, and her general appearance is that of one much out of health. Her hands and feet are cold and wet with a constant clammy moisture. She has for several years been under the charge of physicians and surgeons, with only temporary relief. She has taken quantities of quinine, iron, and alum, but is wearied with perpetually recurring pain and weakness notwithstanding. She has most perseveringly used all kinds of injections, yet her leucorrhœa, her ache in the back, loins, and under the left breast, have never left her—have only sometimes been a little relieved. She was married at seventeen; in a year bore one child; and from that date has never had a day's real health, and has had no other children. She has wearying pains in her limbs; and a perpetual thick, slightly yellow discharge, "very different from the whites." She stands in a peculiar lolling attitude, as if her body had lost all its stiffness, and she leans for support anywhere rather than transmit its weight to the pelvis. She has pain during and after intercourse, and much pain at the monthly period—the discharge, though scanty, being pale, and hanging about for six or seven days. She is weary of her miserable state, and begins to despair of a cure. A local examination was never instituted.

To the finger the vagina is somewhat hot and tender—most hot at its two extremities—most tender at the cervix. The cervix is deeply lobulated; its anterior is separated from the posterior lip by a wide sulcus, as if in the child-birth it had been laterally torn open, and had never subsequently reunited—which is most probably true. There is a rough and granular feeling over a part of the cervix, which is hard, almost stony, to the touch. The os is open enough to admit the point of the finger, and the finger returns from the exploration bathed in thick mucus and stained with blood.

The speculum has disclosed a state of the cervix which is truly pitiable. Of a bright brick-red colour, it projects its tumid and glossy lips into the instrument, and gives exit by the os to a rope of clear, yellowish viscid mucus, in the core of which is a drop of dark blood. An angry-looking ulcerated patch occupies both lips, studded with bright-red points, and terminating by a most distinct livid-red line, about a twelfth of an inch in diameter.

Beyond this the cervix is inflamed, tumid, and tender to the touch. So far as the thick mucus permits the canal to be examined, it is seen to be very vascular, covered with small red granulations, which bleed a little when the cotton wool is used. The whole canal of the vagina is tender, and the instrument, though a small one, is borne with some impatience. The application of the caustic gives rise to a deep and overpowering pain, and an irregular, opaque, white mark indicates the extent of the ulceration. The heat, the greater degree of tenderness, the character of the mucous discharge, the colour of the cervix and granulations, and the tense and indurated character of the cervical structure, are the diagnostic marks of the inflamed ulcer, and they contrast strongly with the lineaments of fungous ulceration which were recently drawn in these pages.

The true explanation of this particular case is to be found in its history. The first pregnancy awoke the disease in a person otherwise of but feeble constitution. The ulcer did not originate out of an antecedent inflammation, but the inflammation is a result of the ulcer. The cervical structures are infiltrated with lymph, and at a later period this becomes organized to such a degree as to give an almost stony hardness to the whole of the cervix. The inflammatory process is most active around the canal and in the immediate vicinity of the ulcer, and shades off as it passes to the periphery of the neck. It seldom extends to the body of the uterus, and though the vagina is somewhat hot and tender it is only affected by sympathy with the state of the cervix, and there is not the least evidence of vaginitis. In the case before us this state of things had probably existed for nearly four years, and I have reason to believe this to be a very moderate period. Permitted to go on, it will, year by year, pull down the vital powers of the sufferer, the acute symptoms gradually declining—if the patient be not previously carried off by the phthisis, or some intercurrent malady, for which she has no health-capital provided.

This case—occurring as it does in dispensary practice, and consequently offering great difficulties in the way of home-management—will probably take four or five months to cure; in private practice it would take at least three. But it is capable of a complete, and—considering its previous duration—it may even be said a rapid cure. As four years have been already vainly spent in the sole use of tonics for general and injections for local treatment, and as the patient's health is obviously giving way, the treatment must be reversed; and energetic local means must be adopted to destroy the concealed disease, and then general treatment will complete the work.

The treatment of inflamed ulcer is simple, but it is important to be distinguished from that of the fungous or the indolent sore, and the value of a sound diagnosis will be felt in carrying out any plan for the removal of any of these diseases. The administration of quinine and iron must be at once suspended, and, as the bowels are generally constipated, a little saline medicine, sufficiently active to drain away fluid by the bowels, and thus relieve the congestion of the uterus and lower bowel, must be administered; and at the same time, small doses (one grain) of blue pill, with hyoscyamus and ipecacuanha, continued every night for a week or ten days, will in some cases do much good. Simultaneously with the use of these means, it will be most important to apply several leeches to the cervix, at intervals of five or six days, for three or four times. It is especially in this form of uterine disease that the application of leeches does good. In several others it is a positive error, yet one by no means unfrequently committed. The patient will keep strictly to her couch for a fortnight, and subsequently must lie down as much as possible. Emollient injections of linseed tea, milk and water, or thin arrow root, will follow the leeching, and be repeated two or three times a day. Careful attention to diet (avoiding stimulants), and the occasional use of tepid hip-baths, will also prepare the patient for the adoption of more energetic local treatment. In about ten or fourteen days from the commencement of this treatment cauterization may be practised. The solid nitrate of silver is less useful than the caustic nitrate of mercury, but it is valuable as an adjunct to the latter, and may be repeated at more frequent

intervals. It is most certain that still more powerful escharotics, such as the potassa fusa or the actual cautery, exercise a salutary effect even upon the inflamed ulcer in any stage; I believe the universal experience of surgeons who have carefully tried this plan will be found to agree on this point; and in dispensary practice there is no alternative but to adopt it almost from the first. There is more delay and much more pain in the cure, but it unfailingly cures at length. After a short use of the milder escharotics, one or two applications of potassa cum calce are eminently useful, and astringent injections may supersede those of the emollient class; and now, the tongue being clean, the ulceration much less inflamed and more healthy, and the cervix softer, the proper time for quinine, and if necessary for iron, has arrived, and very rapid improvement will ensue. The ultimate change in the patient's condition, from that of premature old age, weariness, and wretchedness, to one of restored happiness, vigour, and youth, constitutes one of the most interesting of the silent triumphs of our art.—*Lancet, Aug.* 21, 1861, *p.* 181.

123.—ON REMOVAL OF THE CERVIX UTERI.

By Dr. Robert Greenhalgh, Physician to the Samaritan Hospital for Women, &c.

The safety and desirability of removal of the neck of the uterus in certain abnormal and diseased conditions, may still be regarded as questions *sub judice*. Notwithstanding the success which has attended this operation in the hands of Lisfranc, Hugier, and others,—still, fatal results have ensued, which have made some practitioners regard this procedure with a reasonable dread. It appears that the two following important questions have yet to be decided:—1st. What are the most suitable cases? and, 2ndly. By what means can the removal be most safely effected? As a small contribution towards the settlement of these points, I beg to submit the following facts to the attention of your readers.

I may mention that a similar case, at which I assisted, with equally satisfactory results, has occurred in the practice of one of my colleagues. Mrs. H. applied at the Samaritan Hospital on March 18th last. She stated that her age was 25, and had been married six years, but had never been pregnant. Her complexion, fair and ruddy, was much pitted by the small-pox. She commenced to menstruate when between fourteen and fifteen years of age, and always suffered more or less pain about the back and loins a day previous to the appearance of the discharge, which was free, not clotted, and lasted from four to five days. She has generally enjoyed excellent health. On one occasion, however, about two years and a half ago, the catamenia continued about eleven weeks, and was attended with great pain; the discharge was bright red, and she is sure "no lumps came away." Before marriage she had no idea that "anything was wrong," but shortly afterwards a "swelling came down," which so interfered with her duties as a wife, and led to such frequent disagreements with her husband, as to induce her to seek my advice. The swelling, at first small, gradually increased to its present size. For the last two years the catamenia have become very scanty, lasting about five days, and always attended with much pain.

A vaginal examination detected the cervix uteri protruding between the labia to the extent of an inch, considerably enlarged, corrugated, but not unduly sensitive to the touch. On passing the finger to the extent of about two inches over the growth, which was clearly an hypertrophied cervix, a line of demarcation, was distinctly felt, which appeared to indicate the healthy from the diseased portion. The sound passed easily through the os into the body of the uterus to the extent of five inches and a-half-three inches beyond the normal distance.

On May 17, after a consultation with my colleagues, I proceeded, while the patient was under the influence of chloroform and in the presence of Drs. Savage and Routh, and others, to remove about two inches and a-half of the cervix with the écraseur, which took about half-an-hour, during and

after which scarcely a drop of blood was lost. She progressed most satisfactorily for ten days, when the catamenia appeared, and for the first time without pain, somewhat more copious, and lasting a week.

On June 1 the wound of the cervix was healthy and the os uteri pervious: tepid water to be injected into the vagina twice daily.

On the 10th and 17th the nitrate of silver was applied.

On the 25th she again began to menstruate without pain and lasting five days.

On July 11 a careful examination detected the cervix of normal size and structure, the wound having quite healed; the os uteri was pervious and easily admitted the sound to the extent of three inches.

Some time was occupied in adjusting the écraseur, which might have been avoided by transfixing the cervix with a vulsellum and drawing it beyond the vulva, but it was deemed far more judicious to effect its removal while the uterus was *in situ*, a principle I would strongly urge upon those who undertake similar operations upon that organ.—*Med. Times and Gazette, Sept.* 7, 1861, *p.* 237.

124.—ON THE EFFECTS OF UNDUE APPLICATION OF CAUSTIC POTASH TO THE UTERUS.

[Dr. WHITEHEAD, of Manchester, in a letter to Dr. Tilt, makes the following observation on the result which sometimes follows the application of the potassa cum calce.]

In a number of cases wherein it was applied both to the surfaces of the indurated labia and around their inner circle, the reduction of bulk after healing was attended by total closure of the orificium uteri: so complete was the occlusion, that I found it necessary to re-open the orifice with a lancet, and use means to keep it open until the healing was complete. I have the record of several such cases, some of which have been since fertile, and their parturitions been unattended with difficulty.—*Lancet, June* 8, 1861, *p.* 537.

125.—AN OVARIOTOMY AND ITS RESULTS.

By Dr. CHARLES CLAY, late Senior Surgeon to and Lecturer on Midwifery at St. Mary's Hospital, Manchester.

[Dr. Clay has had as much experience in this disease, if not more, than most medical men. He says that the apparent prevalence of the affection may be that we know it better and pay more attention to it, but he is of opinion that it is and always was a very wide-spread disease. Since his first operation in 1842, 1600 cases have sought his opinion.]

Plans of treatment, with the view of lessening the bulk of the tumour by absorption or otherwise, as by iodine, mercury, &c., without being attended by the slightest possible advantage, yet so far prostrate and depress the physical powers of the system as entirely to destroy any prospects of success in an operation, should the patient seek it, although a fair average of success might have been anticipated if such pernicious treatment had not been put in force. Pressure and galvanic currents on and through the tumour have each been advocated very strongly by some parties. I have heard of no cases where any advantage has been gained by galvanism, nor do I believe any good result could possibly be gained by such process, while there is a probability of rousing into greater activity a disease almost dormant, or very slow in progress at the time. As to pressure, save and except in such rare cases where it can be certain that a single cyst unaccompanied by solid nucleus exists, in such a case it is just within the bounds of probability that success might attend the process; but in all other cases, that is in nineteen-twentieths of the cases I have seen, pressure would not only be unattended by any advantage, but decidedly the most pernicious and injurious

practice that could possibly be put in force. Suppose, for a single moment, a large multilocular tumour of many pounds weight (my own cases average 27lbs., and I have extirpated many heavier, and one of 73lbs.,) how is it at all likely that any pressure could ever effect, by *absorptive process*, a disappearance of such a mass in the short time allotted to the lives of individuals suffering under this disease,—a term of not more than three years on the average? The whole affair is not only a physiological absurdity, but an impossibility.

The above are not the only objections. Violent and long-continued pressure for a time, will be as certainly followed by severe attacks of inflammatory action, resulting in extensive adhesions of the tumour to the abdominal parietes, or both of them to the pelvic and abdominal viscera, forming a matted mass of living structures impossible to unravel (proved in a *post-mortem* examination), and presenting insurmountable obstacles to any future attempts at extirpation, which it would be madness to encounter. I have invariably sent such cases back to the parties adopting such plans, that they might see the terminations of the sufferings they had occasioned. Two deaths have come under my notice where the pressure system had been tried to the fullest extent, and the awful sufferings of the poor creatures during the progress of enlargement, where all the parts concerned were matted together firmly (as was proved by examination after death) cannot be described. All the cases where I have seen pressure tried confirm my opinion that the progress of the disease was greatly accelerated, except, as I have before stated, in single cysts, thin walled, and not accompanied by any solid nucleus, but these cases are extremely rare.

From these observations, I conscientiously believe that neither medical treatment external or internal applications, pressure, nor galvanism, are of the slightest benefit; they neither cure nor palliate the disease. All such attempts, then, are fallacious, and only throw obstacles in the way of any benefit that extirpation of the tumour offers, increasing the difficulties of that operation, if not defeating it altogether.

I ought in this place, before I proceed further, to admit one exception to this general condemnation, and that is in the case of one large single cyst with thin walls, which by tapping and injecting the sac afterwards with two or four ounces of the strong Edin. tinct. of iodine, might be productive of cure. I have seen three cases succeed by this plan, and as these operations took place now three years ago, the result has fully justified the means employed. Thus, then, in single cyst cases, I think such means ought first to be tried, even to a second or a third injection, before requiring the patient to submit to the process of extirpation. Still, I must not omit to mention that the chances of success of extirpation are not improved by the previous use of iodine; such cases never recover so well as those where iodine or mercury have not been used.

I have spent much time in endeavoring to decide if ovarian disease be more prevalent in young females or in older persons,—in single or married females. It is a question of great difficulty in the absence of a larger range of statistical information than I or any other single individual could possibly have at command; still, with the information I have been able to obtain, I believe the preponderance is with the young and unmarried females; and I feel also certain that the tumours in those classes are far worse, as a whole, to cope with, having generally a larger proportion of solid mass, accompanied by more severe symptoms, more rapid as to growth, and as a matter of course, lead to an earlier termination.

In elderly females, *unmarried*, the tumours are very frequent, and usually not of a very promising character; whilst, on the contrary, elderly *married* females seldom have the disease until after the child-bearing period; that is, after the cessation of menstruation. In such cases, the tumours are slow of growth, multilocular in character, and usually not much adhered, and I may add, but seldom accompanied by severe inflammatory attacks. It is this last class of cases that I should prefer, before all others, for extirpation; and if I must have a choice of age, I prefer those between forty and fifty to

any other. Of course, there may be good cases at all ages, and many good recoveries have been made at all ages; but my experience points out the above cases as most probable for success.

My attention was, contrary to my expectations, very early directed to one fact, which is, that the right ovary is far more frequently the seat of disease than the left, and I am yet unable to account for it. In my earliest cases I was impressed with the idea that the left would be found more frequently the seat of disease; experience, however, proves the contrary to be the fact; taking the whole number of my cases I may venture to say that at least two-thirds have been of the right ovary. With respect to the connexion of ovarian with the uterine diseases, I have found about one-fourth of the entire number of cases to be more or less connected with uterine disease. In my 'Manual of Obstetric Operative Surgery,' published in 1856, I stated about one-third of ovarian cases were connected with uterine disease, but I have since found out that in that statement the proportion was too large; the present announcement of one-fourth will be found to be nearer the truth. I need scarcely remark that all cases of ovarian disease, accompanied by uterine disease involving its structure, such as hypertrophy, ulcerations, cancer, &c., &c., place the operation of extirpation entirely out of the question, to attempt which, under such circumstances, would be altogether unjustifiable, as it would certainly only hasten the patient's death. Hence the great necessity of defining, which is often a matter of great difficulty, whether the case be accompanied by uterine disease. The large number of cases presented for diagnosis compared with the small number operated on in my own practice, will at once defend me from the charge of seizing on every opportunity for operating; 104 operations out of 1,600 diagnosed cases is but a very small proportion. I could easily have trebled the number of operations which would have added to my pecuniary advantages, but have preferred, after giving an opinion, rather to be sought after than to urge the sufferers to undertake the chances of an operation.

It might naturally be inferred, from the aptness of the ovaries to take upon them morbid derangements, that we should find both frequently affected at the same time. This is occasionally the case; but not nearly so often as we should be led to suppose. I consider it to happen not more than once in twenty cases. But when *both* ovaries are affected, it is almost always found that the uterus is also involved. Where both ovaries are affected, the operation for extirpation is scarcely admissable, though the uterus may be free, as it calls for the extirpation of both ovaries at once; or another operation will soon after have to be performed on the second ovary. I have had one such case, where a large ovary of 37 lbs. was removed, and at the time it was found, the other ovary was enlarged to the size of a walnut. Both were excised, the case did well, and made a complete recovery, but was followed by some curious physiological phenomena, such as loss of sexual passions, loss of voice, at least it became croaky, and many characteristics of the female vanished.

The characters of the tumours vary very considerably; about one-fourth of the whole are of a solid lobular kind, often considerably adherent, slow of growth unless provoked by accidental injury, or irritated by treatment, as by pressure, galvanism, &c. Such a class of tumours are not very favourable for extirpation. Nearly one-half, or more than a third have well developed and more or less large cysts, with some portion of solid nucleus, pretty free from adhesion, are of more rapid growth, especially if any of the cysts should be emptied accidentally or intentionally; these are a class more favourable to extirpation. A still smaller proportion consist of thin-walled cysts, with scarcely any solid material (in some rare cases, of one large thin-walled cyst, with no traceable solid mass). This class is of rapid growth, seldom adherent, and very favourable to extirpation, and occurs mostly with females of younger ages. In such cases it is only reasonable, before extirpation is proposed, to try the injection of strong tinct. of iodine after tapping, particularly where there is but one large cyst, and I would persevere to the second or third time, before proposing extirpation. I have

seen three cases making rapid and permanent recoveries by this mode of proceeding, and I have also seen it fail in two other cases. Still it must be borne in mind, that the cases themselves are of the rarest kind, as I have not met with more than ten in the whole of my practice that I could call *single cyst*, and free from solid mass. It must also be recollected that, wherever iodine or mercury have been used to any extent, the chances of recovery from extirpation is considerably lessened. It is, therefore, desirable to use neither, as no good results from their use, except in such cases as has just been stated, where a reasonable prospect of success may be anticipated; although, if it fails, the subsequent operation is deprived of some of its chances of success. If there are more cysts than one, or if a cyst be accompanied by solid material, it would then be the height of absurdity to inject a cyst after tapping, as it cannot, in the slightest degree, check the formation of fresh cysts from the solid mass, or prevent the other cysts already formed from enlarging; therefore, as it is incapable of influencing the main disease, and would injure the prospects of success in the extirpation, such modes of treatment should be carefully avoided.

Mr. Safford Lee believes that married females are more liable to ovarian disease than single ones; my own opinion is, that the liability is about equal. As respects age, I believe the commencement and decline of menstruation are the periods most productive of ovarian disease, probably because of the irregularities attendant on the commencement and cessation of menstruation. I have said the duration of the disease, on the average, is seldom more than three years; this applies more strictly to younger persons; for if we divide the cases into those *under* 30 years of age, and those above 30, we should find that with the latter the disease is often of much longer duration than with the former.

The growth or progress of ovarian disease, when once established, forms a very important point of consideration, and often presents considerable difficulty in forming an opinion as to the future prospects of the case; speaking of these cases as a whole, I consider them on the average very rapid in their development, and taking this view, I should not hesitate in limiting the duration to two and a half, or at most three years. Notwithstanding, there are many cases that suffer under this disease for very many years, attaining an enormous size, and some of these cases appear to suffer but little, except from the inconvenience of bulk, and inability to move about. Then, again, some have borne tapping a great number of times without much suffering, whilst others have immediately sunk after the second or third tapping; the former being the characteristic of the disease in advanced life, and the latter that of earlier or younger lives. On the average, then, these cases seldom exceed three years, and if tapping is resorted to as a simple means of relief, the patients generally sink after the second or third operation.

The growth of ovarian tumours appears to be considerably influenced by age, that is, if we divide cases into those above *thirty*, and those under *thirty* years of age, the former are of much slower growth than the latter, and accompanied by much less constitutional disturbance. I have generally found the most unfavourable cases and the most rapid growths during the earlier ages, and arising *rather* from obstructed uterine and ovarian functions during the child-bearing period, than at later periods of life, and more immediately connected with the cessation of the menstrual function. I conclude, then, from these remarks, that there are but few cases which do not complete their termination in some way within three years from their commencement,

I do not intend to occupy the reader's time by speculating on the pathology of ovarian disease; the seat of this disease is still disputed, and, if settled, can in no way alter the means proposed for its treatment, whether that be for the simple alleviation of suffering, or its radical removal. I would, however, say a few words on the varieties of tumours which present themselves to the practitioner.

And first, the *simple cyst* attached to the ovary, or broad ligament, either by a distinct pedicle of its own, or arising from a broador base. This form

of the disease is very rare, and, as I have previously stated, I have seen but very few such in my practice; they are the only cases that are likely to yield to iodine injections after tapping; and in half of such, at least, tapping with injection will not succeed; still, if half succeed it would not be justifiable to neglect a trial. Spontaneous bursting and tapping have cured this form. *Cysts from breaking up of the Graafian vesicles* is a much more frequent form of ovarian disease, these cysts breaking up into each other until no remains of the original ovary are to be found; this class, as well as the simple cysts, are of rapid growth, and often attain a very large size. The fluid contents are generally coffee-coloured, but present considerable variety in appearance. From twenty to thirty pounds of fluid are often taken from cysts of this character, and I have in one case taken seventy-three pounds. In these cases the form of the tumour is more unequal or lobular than in the single cyst. Mr. S. Lee speaks of large cysts being formed attached to the liver, omentum, and peritoneum. I have not seen any such case, but, on more than one occasion I have found the abdomen filled by hydatids, which presented many of the symptoms characteristic of ovarian disease. The *multilocular tumour*, having very many cysts varying very considerably in size and in the contents of each cyst, is the most frequent of all forms of ovarian disease, and the kind of case where tapping can be of no use, and where, if adhesions are not very extensive, extirpation is most likely to be successful. The size of such tumours as are likely to be brought forward for the prospects of an operation is generally large, twenty-five to thirty, or thirty-five pounds, is very common, and I have removed one successfully weighing seventy-three pounds. From these remarks, some idea may be formed as to the extent of material removed in one hundred and four operations, which could scarcely average less than thirty pounds each.

The character or general appearance of ovarian tumours is of some importance, as it is just probable that great uncertainty may exist in the diagnosis, and it may be necessary to make an exploratory incision. In such a case, after every other mode of inquiry has failed, the first appearance of even a very small portion of the tumour surface will decide if ovarian or uterine; the pink blush, and moderately even surface, is the character of uterine tumour; whilst the whitish, muddy pearl-like appearance, with a bluish tint here and there, will never fail to characterize the ovarian mass, the surface is also more irregular, more lobulated; the vessels are large and flat in uterine tumours, and are scarcely discernible; in ovarian cases, they appear on the surface larger than they really are, being much flattened, and confined chiefly to the surface. In the uterine enlargements the vessels are very numerous, but more subdivided and smaller, and interspersed throughout the whole mass. In all exploratory incisions, if the surface shows the pink shade before spoken of, the incision should be immediately closed. As to the interior of ovarian tumours, the cells or sacs vary considerably as to size, from that of a pea to the enormous sacs spoken of in the preceding remarks. The contents are equally various, some gelatinous, some thin, some contain pus of almost every form and consistence; in some of the larger sacs hang pedunculated masses, like the main mass. I have not seen any hydatids in the interior of ovarian tumours.

The vessels supplying the pedicle will guide the opinion as to the vascular character of the whole mass; I have seldom found it larger than a crow-quill, although those on the surface appear much larger, and yet their supply must be derived from the vessel of the pedicle only.

Adhesions.—In many cases there is not the slightest adhesion beyond the pedicle, others are adherent, not only to the viscera, but also to the peritoneum, though not extensively; in both these classes I should operate without any hesitation; the character of the adhesions varies considerably, in respect to different organisms. Those to the intestines, are generally threads or bands of lymph, easily separated by the bistoury without hemorrhage, and are generally of no consequence. Those to the mesentery are patchlike, sometimes easily peeled off, but often firmly organized, and when separated are liable to hemorrhage; such separations should be watched, and if any tendency be

shown to bleed, the vessel should be secured with fine silk, and the ends cut off close. Lastly, the adhesions to the peritoneum are in broad patches, and often peel off with moderate ease, but in some cases are so firmly organized that it is better to cut off the tumour at the edges of the adhesion, leaving the broad patch of the adhered portion of the tumour attached to the peritoneal surface, to slough off and discharge itself by the aperture left for the ligature at the pedicle. I have frequently done this without any bad consequences arising from it. It is a curious fact, that those cases have often made the most rapid recoveries where there have been the most extensive adhesions, and considerable force required to overcome them.

As the contents of ovarian cysts are very variable, it would occupy considerable time to describe each variety, and as it would not be any advantage either in treatment or operation to dwell upon this portion of our subject, I shall next make a few remarks as to the development of symptoms in this formidable disease.

The Symptoms of Ovarian Disease are very different when the disease occupies the abdominal cavity to what they are when only in the pelvic cavity in the earlier stages of its development. The *early symptoms* are deep-seated pains in the groins, bearing-down sensation in the vagina, a feeling of fullness, throbbing pain at the anus, more especially when voiding fæces, numbness on the side affected, loss of motion, hemorrhoids, irregular menstruation, fluor albus, os uteri *in situ*, tenderness above the pubis inclining to one side; vaginal examination detects fullness on one side, with tenderness to the touch (these latter symptoms are more distinct on examination per rectum), constipation, flatulency, inability and desire to void urine, most of the symptoms simulating pregnancy. As the disease advances, and begins to occupy the abdominal cavity, many of the above symptoms are relieved, others aggravated; during the period of menstrual discharge very many of these symptoms are still further increased in severity.

Advanced symptoms.—Bladder somewhat displaced and more irritable, tympanites, sickness, occasionally œdema when the case is an extreme one, dyspnœa, often but not always fluctuation, belly shining, enlargement rather to one side, lobular, prominent at the centre, in extreme cases the ensiform cartilage and ribs forced upwards, umbrella form, if one or two cysts the fluctuation very distinct, if many cysts fluctuation obscure or altogether absent from the semi-solid character of the tumour; movements felt, often mistaken for fœtal, but only arising from the efforts of flatus passing along the intestines with difficulty, in consequence of the pressure of the tumour; the presence or absence of adhesion can only be attested by an experienced touch on the abdominal surface, and often great difficulties are encountered by the most experienced.

Examination per vaginam.—Vagina elongated, os uteri displaced, drawn upwards and laterally, the uterus free, natural in size, and easily moved about, unless the pelvic cavity is filled with tumour mass, in which case it is difficult to ascertain, as the uterus is often out of reach or flattened; the vaginal walls are pressed in on the diseased side.

Diagnosis.—From retroversion, it is distinguished by the situation of the os; from retroflexion, the uterine sound will easily determine; from ascites, by the system being in a healthier condition, by its lateral bearing, by duller fluctuation, by irregular surface, by elongated vagina, by os tilted on one side, and by seldom being accompanied by œdema of the legs. In cases where there is but one large cyst, with little or no solid mass, the distinction from ascites is more difficult, but such cases are rare.

From Pregnancy, by its lateral position, by regular or irregular menstruation, vaginal examination shows the uterus not enlarged, moveable, no fœtal pulsation; the latter, however, may be present in cases where pregnancy co-exists with ovarian disease.

From Cystic Tumours not Ovarian.—The history of the case; by the veering to one side, by the menstruation still less interfered with.

Enlarged Uterus.—Differs from ovarian, by being less lobulated, more central, easily detected by uterine sound, size and weight of uterus by vag.

examination, suppressed menstruation, general health more disturbed, and complexion more sallow.

From distended Bladder.—By proper attention to symptoms, by which it will be impossible to confound it with ovarian disease.

Accumulated flatus has been mistaken for ovarian disease, and operated upon for it; in the present state of our experience such an accident seems impossible.

Other enlarged Viscera.—An enlarged liver or spleen might possibly be mistaken for ovarian disease, but it is not very probable. As a rule, with enlarged viscera, the lower third of the abdomen is pretty free from tumefaction, whilst in ovarian disease that lower third is filled in preference. This point, coupled with the prominent constitutional disturbance in visceral enlargements compared with the trifling disturbance in ovarian mischief, will generally be a sufficient distinction.

With respect to the *Treatment of ovarian disease* when once fairly and extensively developed, I have so little faith in any benefit by medicine, that I shall not repeat what I have already stated on that question in my 'Manual of Obstetric Operative Surgery.' I have not found any remedy that can in the least be depended upon in the form of medicine. *Tapping*, as a curative measure, is the worst that can be attempted, and can only be justified in cases where the operation prospects are hopeless, or where the invalid is determined not to submit to its chances. Tapping at all times can only be advanced as a means of temporary relief, and as a rule, only spurs the secretory sacs to refill more rapidly.

Injecting with tinct. of iodine has succeeded in my hands frequently, but can only be justified where there is but one sac, where the walls are thin, and where there is no nucleus of solid tumour; lastly, in apparently the most favourable cases it has frequently failed.

Extirpation, where it can be done, is the only practical remedy, and is one in which, if it succeeds, the cure is permanent. By extirpation, I mean by the large incision, that is *as large* as is necessary for the operator to manipulate easily and rapidly for effecting its removal. My friend, Dr. T. Smith, of London, considers the exposure of viscera in the operation for a length of time as a great source of danger. I fully agree with him on this point, and it is on that account I advocate a free and large incision, believing when the operator has room to manipulate freely, he lessens the time of exposure very considerably, at which time, if the temperature of the room is carefully attended to, the danger cannot be increased.

I shall now conclude with a few statistical remarks on this operation. My own mind is still impressed with the importance of taking, as a guide to this question, the experience of those only, who have, like myself, operated a sufficient number of times to have formed some definite ideas on the points necessary to be enforced to secure the best prospective results, rather than to rely on general statistics which must of necessity be built up by a number of isolated cases by individuals who have scarcely ever operated a second time, or at most some two or three cases, both of which classes of individuals cannot have met with much to enhance their own experience sufficiently to enable them to become safe guides for others of still less experience to follow. To mix the results of these limited operators with the whole has a tendency to depreciate the general statistics materially, and lead to wrong impressions. This is the principal error in the list of cases collected by Drs. Robert Lee and Safford Lee, as also in the very excellent and elaborate table of ovarian operations by John Clay, Esq., of Birmingham. I prefer statistics drawn from those operators who have performed it frequently, men who have had frequent opportunities of testing their practice, and from these alone I would adduce the best rules for the guidance of other operators in future. It must be apparent to all on reflection that other information, however successful, must be so perfectly hap-hazard or accidental that such should not be mixed up with the general question. Indeed, such is often the unlooked for result of first cases undertaken without previous experience, that I am not certain if it would not benefit the general question more if the

first three or four cases of every operator were rejected, as it is well authenticated that they will often recover under the most extraordinary disadvantages, whilst, on the contrary, they may sink without an apparent cause. In the 'Dublin Quarterly Journal' of Feb. 1861, is reported a case of ovarian extirpation *with successful results*. In the report are some very lengthy instructions how to conduct such cases to a favourable issue, although it was the author's first and only operation, and, singularly enough, the case *never did recover*, but died almost as soon as the report of her cure was announced to the public, at least early in the March following, the date of the case in the journal being February. Since 1842 I have operated on 104 cases, 97 of which have been under my own treatment throughout. The remaining seven were operated on at a distance from home, and the care of the cases was left to others, assisted by any suggestions I could make from time to time, but not accepting the whole responsibility. The following figures show the general result up to the date of this communication :—

Cases.		Deaths.		Recoveries.
97	..	29	..	68
7	..	3	..	4
Total....104		32		72

Taking the odd figures aside, the recoveries in round numbers will be as 70 per cent. From the length of time the disease has been established, when the case is presented for operation, the great debility of the system from frequent tappings, or the secretory supply called into action, I do not think we can reasonably hope to extend our success beyond 70 per cent.; but even this far exceeds the first expectations, and is sufficient to establish the operation as a legitimate one in all well-selected cases, in the minds of those who are desirous of extending the benefits of surgical skill to these unfortunate sufferers.—*London Med. Review, Aug. and Nov.*, 1861, *pp.* 59, 205.

126.—ON OVARIOTOMY.

By T. Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

[Mr. Wells relates several cases of ovariotomy. The last is the most important. The patient died twenty-four hours after the operation. The case was a very unfavourable one, the cyst being a large compound one, having many adhesions. The peritoneal cavity contained from four to five pints of reddish serum.]

The serum found in the peritoneal cavity must have been a very active animal poison, for I suffered severely two days after the examination from a very slight scratch with the point of a needle on the left forefinger. I sucked the spot instantly, but the next day a small vesicle formed, and I applied caustic freely. On the second day I had severe rigors, lasting several hours, with intense headache, relieved by vomiting and a copious perspiration, which lasted about eighteen hours. For several days afterwards I was very weak, but all the severe symptoms had passed off by the fifth day after the puncture.

This recalls a question I have raised before. The peritoneum contained some pints of poisonous serum. It was probably formed by part of the membrane, and might be absorbed by other parts. If so, a poison which affected me so severely in a small dose might easily kill any one in a larger dose. I recovered after the absorption of a fraction of a drop; but the poor woman was overpowered by the quantity taken up by her own absorbents. I am quite aware of the objection some may offer, that a fluid poisonous after death might not have been a poison during the life of the woman; the reply is, that twenty-four hours was too short a period after death for decomposition to have occurred; and also, that all experience has proved inoculation of the fluids of those dying of puerperal peritonitis, erysipelas, &c., to be far more dangerous when the bodies have been fresh, than when putrefaction has commenced. This is not a mere question of theory or curiosity, for it

leads to an important rule in practice—or, rather, to the suggestion, that in cases where such poisonous serum may reasonably be supposed to be present, and *a fortiori* when there are physical signs of its presence, that a part of the wound should be opened to allow of the free escape of the serum by the side of the peduncle. In two cases I formerly published I acted upon this rule with the greatest advantage, and both patients recovered. The question also arises, whether one is not apt to unite the wound too closely round the peduncle. The fear is that peritonitis may be set up by leaving *any* opening; and one generally closes the wound carefully around the peduncle, partly to prevent entrance of air, and partly to prevent gravitation into the abdomen of the putrid fluid formed around the stump during the time the ligature is in process of separation. This plan may be a good one on the whole; but I am disposed to think that in many cases where there is such a condition as I have described after operation, a free opening should be made for the escape of the serum.

Another question, of even greater importance, is suggested by this case—Is it right to perform such an operation as ovariotomy in unfavourable cases? It may be said that by doing so the surgeon not only risks his own reputation, but lowers the operation he performs in the estimation of the profession, and thus lessens the number of favourable cases who might be willing to undergo it, were it not known that one in two, three or four who do submit to it, die. It is quite clear that a surgeon who will only operate on very favourable cases ought to show far better returns than one who consents to stake his own reputation in order to give a dying patient a small chance of recovery; and it may possibly be right to follow the more prudent course. But in a case where a poor woman says, as many have said to me, "I suffer from a disease which must kill me. I cannot live very long. My life must be a life of suffering. If you operate, I know the risk I run; but I *may* be cured and return to my husband and children, and I would rather die than live as I am,"—in such a case as this, I do not envy the feelings of a man who—unless he saw the case was absolutely hopeless—would let any consideration for the general character of surgery, or for his own reputation as a successful operator, induce him to refuse the prayer of the poor dying creature who placed her life in his hands.

Still, I think it is right that some classification of cases should be adopted, for what we want in practice is not an answer to the question "What is the general success of ovariotomy?" What the patient and the surgeon want to know is, "What are the probabilities of success or failure in this particular case?" and this knowledge can only be gained by testing the operation in various classes of cases—distinguishing those patients who are young and healthy from those who are old, or worn out by frequent tappings or long-continued disease, and so on. If I were to exclude the fatal cases in my own practice which were regarded by myself and others in consultation as unfavorable before operation, I might say that I had scarcely lost a single case; and my experience of those cases which may be regarded as favourable is bringing me to the conclusion that ovariotomy is one of the most successful of our capital operations, and not very much more dangerous than a simple tapping.

Taking favourable and unfavourable cases together, the following is the general result of my experience of the operation in hospital and private practice:—

	Cases.	Deaths.	Recoveries.
Hospital	14	5	9
Private	10	3	7
	24	8	16

I need hardly say that amputation of the thigh, ligature of the larger arteries, lithotomy in the adult, and other capital operations called "legitimate," show a general result less favourable than this operation, which is still stigmatised by some writers as "unjustifiable."—*Med. Times and Gazette, May* 25, 1861, *p.* 546.

127.—HYSTERIA MASKING SECONDARY SYPHILIS.

(Case under the care of W. Coulson, Esq., St. Mary's Hospital.)

[There are cases in which the hysterical tendency is evinced less by the simulation of disease which does not exist, than by the exaggeration of symptoms of some affection which has an actual existence. In such a case is it better to treat the case from the hysterical point of view, or to strike at the malady upon which the hysteria is superadded? We believe that in practice the latter will be found to be the most successful treatment.]

This patient, a young woman aged twenty, when admitted, complained of stiffness of the elbow-joint, which was retained in a position midway between flexion and extension, but could neither be flexed nor extended. There was a history of a blow on the elbow some three months previously, but without applying the test of anæsthetization by chloroform, Mr. Coulson was able to conclude that there was no organic local cause for this affection, and that it was mainly of hysterical origin. Further inquiries, however, into the history of symptoms showed a suspicious condition of the throat, and some tenderness and nodular roughness along the shin bones. These were symptoms indicative of a secondary taint. Mr. Coulson ordered three grains of iodide of potassium in an ounce of infusion of quassia three times a day, and two grains each of blue pill and Dover's powder at night. The secondary symptoms yielded to this treatment, and the hysterical affection disappeared simultaneously. The patient was discharged at the end of three weeks, having the free use of her arm, and feeling no pain in it.—*Lancet, May* 18, 1861, *p.* 483.

128.—ON IRRITATION OF THE URETHRA IN FEMALES.

By John Hunter, Esq., M.A., Manchester.

[The following is one of a number of cases which the writer has lately had under his care, not particularly described in books, and not perhaps fully recognised. The age of the case must not be taken as an example of the ages of the others, for the third case related is only twenty-two.]

Mrs. C., aged seventy-two years, consulted me in the summer of 1859, whose case presented the following features:—During the past fourteen years she had suffered from incontinence of urine—in fact "she was never dry." Together with this there was a constant desire to void her urine, but instead of each attempt to do so bringing relief, it only caused an increase of her sufferings; yet still the desire was persistent. Walking or standing greatly aggravated her sufferings. As much of her urine as could be collected was much clouded with mucus; there was pain and a feeling of weight in the lumbar region. The treatment hitherto had consisted in lotions and other applications to the external parts and to the vagina, but without bringing any relief. On examination, the external parts appeared and felt to be quite natural, but the urethra, from the meatus up, as far as could be discerned, was very red and congested, quite as much so as the vascular tumours sometimes seen near the meatus. On the introduction of a silver catheter, the whole urethra was found to be exquisitely tender, more particularly so at about the external third. The bladder itself did not appear to be affected; in fact, the disease seemed altogether confined to the urethra. The urine, on examination, was found to be not more acid than natural.

The treatment I adopted consisted in the exhibition of alternatives, with the constant use of small doses of liquor potassæ. The urethra itself I treated locally in the following manner: I first injected a little warm water from a male glass syringe, in order to wash away the adherent mucus, taking care, that the nozzle of the syringe was fully introduced (with a slight rifling movement) and that it did not impinge on the walls of the urethra. I then threw up a syringeful of solution of nitrate of silver (five grains to the

ounce), and allowed it to come away of itself. I enjoined her to rest in the recumbent posture, and as far as possible to resist the desire to empty the bladder. I used the same local means every day, and gradually the mucous membrane of the urethra appeared to be less irritable, and, as a consequence, the contents of the bladder were retained for a longer time; till, in about a fortnight from the commencement of the treatment, the patient stated that she was able to hold her urine for about four hours, the pain had almost disappeared, and the general condition of the patient was satisfactory. I have since attended her for another disease, and learned with satisfaction that she had no return of the urinary derangement.—*Lancet*, *May* 18, 1861, *p.* 480.

129.—EXPLANATION OF HYSTERICAL PAINS IN THE HIP OR KNEE-JOINT.

By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital, &c.

The sacral ganglia and the lower lumbar ganglia of the sympathetic nerve, are connected with the great sciatic nerve, and in part with the obturator nerve. These same ganglia are connected likewise with the nerves proceeding through the broad ligament to the uterus and to the ovaries. I think we have here, then, an explanation of the frequent occurrence of what we call hysterical hip-joint or hysterical knee-joint. If the nerves in the ovaries or in the uterus be in a state of irritation, that irritation can be conducted to these sacral nerves or to the obturator, and then in accordance with the generally received law of distribution of nervous influence, irritation or pain may be manifested at the other peripheral or articular end of the same nerve; so that it may be expressed within the knee-joint, on the inner side of the knee-joint, or it may be within the hip-joint, because the hip as well as the knee-joint receives its nerves from these various sources. You will observe the posterior part of the hip-joint and the nerves coming to it from the sacral plexus. Part of the sacral plexus receives its filaments from the sacral ganglia; so do the uterine and ovarian nerves; and it is quite possible, nay, I think it is very likely, that the irritation commencing in the ovaries or the uterus might be conveyed to some of those filaments derived from the same ganglia in the sacrum, and irritation in the hip-joint be thus produced. You will observe that the two nerves, which are so distributed as to be capable of producing the pain in the joints, are the great sciatic and the obdurator; and I believe this is a probable explanation of the fact, that of all the joints in the human body affected, as we term it, hysterically, none are so frequently involved as the hip and the knee-joints. By tracing these two nerves I think we may see a probable interpretation of that frequency.

We know that the spinal nerves communicate with the ganglia; they go on through the ganglia and join the filaments of the sympathetic—travelling upon the arteries, for example—and are thus transported to the intestine, uterus, or ovaries. Let us assume, then, that a patient may have irritation from any cause in the intestine, in the uterus or ovaries, or in the broad ligaments. Thus we may trace the course of that intestine, uterine, or ovarian irritation through the ganglia, through the spinal nerve and spinal marrow, thence to be reflected to any part of the peripheral or articular distribution of that same spinal nerve. This condition I apprehend, is very clearly recognised sometimes in the case of the intestines. Who is there that has not felt the irritating pains and gripes in the interior of the intestines from some morbid agent lying there, or from drastic purgatives traversing the gut, and at the same time pains or cramps in the leg and pains in the loins?—conveyed in the latter instance by the filaments of spinal nerves, which pass to the posterior part of the body or the lumbar region. And it is not a common occurrence in cases of uterine and ovarian irritations for the patients to complain of pain in the loins, but particularly over the posterior part of the sacrum? The ovarian and uterine nerves traverse

the ganglia of the sympathetic, and so reach the spinal nerves, and hence the morbid influence conveyed by the posterior branches of the spinal nerves to the skin over the lumbar and sacral regions, explaining the lumbar and sacral pains experienced by such patients.—*Lancet, Sept.* 14, 1861, p. 247.

130.—ON LEUCORRHŒA AND VAGINITIS.

By Dr. CHARLES WALLER, Obstetric Physician to St. Thomas's Hospital.

[The subject of leucorrhœa is almost co-extensive with diseases of the generative organs, as there are but few affections of these parts unattended with leucorrhœal discharge.]

Leucorrhœa is sometimes the result of debility and relaxation: it is, however, not unfrequently the result of the opposite condition of parts, where there is active inflammation of the vaginal membrane, partial or more general; this has sometimes been termed "inflammatory leucorrhœa." Vaginitis is, however, the more appropriate appellation, as this is descriptive of the *disease*, whilst the other merely enunciates a symptom. The discharge sometimes proceeds from the uterine membrane, either entirely, or in addition to the vaginal flow; this has been termed uterine or utero-vaginal leucorrhœa. There is yet another form, first described by the late Sir Charles Clarke, and named by him "white, opaque mucous discharge," and he thus accurately describes it:—"This discharge is opaque, of a perfectly white colour, and it resembles in consistence a mixture of starch and water made without heat, or thin cream; it is easily washed from the fingers after an examination, and is capable of being diffused through water, rendering it turbid. A morbid state of the glands of the cervix of the uterus probably gives rise to *this* discharge; at least, the cases in which it comes away are those in which the symptoms are referred to that part; and when pressure is made upon it, the woman experiences considerable pain." And again, "the discharge of which the above definition is given belongs to one morbid state of the uterus only; but it characterises that state with marked constancy." Remember, lastly, that the malignant diseases of the womb are never unattended with leucorrhœal discharge: in these latter cases there is always more or less blood mingled with it.

Vaginitis.—There are two distinct varieties of this disease. In the one, the symptoms are those of acute inflammation; in the other, the membrane is in a state of passive congestion. In other words, there is active and there is passive vaginitis: let me now request your attention to the former variety. If vaginitis be of the active kind, and extend over a large portion of the canal, the symptoms are too characteristic to be mistaken for any other disease: the vascular excitement and irritation are very distinctly marked, and in the more severe forms constitutional derangement will be added to the local symptoms; pain is felt in the vaginal passage, varying in degree and character: it is sometimes described as darting and throbbing; at others, as hot, scalding, or smarting, closely resembling the pain felt at the extremity of the rectum during the action of some powerful drastic purgative. The inflammation often creeps along the membrane of the urethra, and then, in addition to the symptoms enumerated, there will be smarting and cutting pain whenever the patient passes water (*ardor urinæ*). It does not always stop here, but extends into the bladder itself, causing frequent and sudden *desire* to micturate, with considerable pain in accomplishing the act. The discharge does not consist in a mere increase of the natural secretion of the part, which is of a bland, unirritating quality, but becomes entirely changed, even in its appearance; it is copious but much thinner in consistence than ordinary, and very irritating in its character, often producing inflammation and sometimes excoriation of the skin outside the genital fissure, where there has not been sufficient attention to cleanliness. A very foul ulcerated surface is sometimes produced by this cause.—There is another symptom often if not generally present in acute vaginitis which is very distressing to the

patient; namely, a constant and painful sensation of "bearing" or "forcing down," probably inducing suspicion of prolapsus uteri. On examination, you detect no displacement of the organ; and on inquiry, you find that the recumbent position does not relieve the uneasiness. From what cause does this arise? Evidently from the inflammatory state of the upper portion of the vaginal membrane, which cannot then bear the pressure of that portion of the cervix uteri which projects into it, although insensible to it in the natural condition of the parts. If a digital examination be made, pain will be experienced: the extent of such pain, whether partial or more general, will enable you to determine with tolerable accuracy the extent of the inflamed surface; should the speculum be employed, the membrane will appear turgid with blood, either of a bright red or vermillion hue, or a more dusky crimson colour, according as the disease is more or less active in its character. I have already told you that in unmarried females the speculum cannot be easily introduced; nor is it necessary in the vast majority of instances, as the probability is great that simple inflammation exists. In those who have borne children, you often discover some structural change around the os uteri, in cases of protracted leucorrhœa, which will, of course, require special treatment. After long-continued leucorrhœa, changes occur in the vaginal membrane. There are, sometimes a number of slight excoriations along its entire surface, or confined to the portion reflected over the cervix uteri; or there may be distinct elevations without ulceration, producing that peculiar appearance which when occurring in the eye is called "granular conjunctivitis." This inflammation may proceed still further, and produce warty excrescences, few or many, soft in their texture, tender to the touch, secreting an irritating fluid which, if not constantly washed away, produces an extreme degree of heat and smarting, which greatly aggravates the sufferings of the patient.

These symptoms, you are aware, may be traced in many instances to gonorrhœal infection; but as they may and do occur as the result of simple acute vaginitis, it behoves the practitioner to be very careful, and not hastily to pronounce an opinion which might for ever damage the reputation of an innocent female, or, it may be, mar the happiness of domestic life by raising injurious and unjust suspicions of the conjugal fidelity of the wife or husband. So virulent is this discharge, that it will occasionally produce great irritation in the male organ if exposed to its influence: this may be either in the form of urethritis with muco-purulent discharge, or it may even cause ulceration. I once attended a gentleman who was suffering from a slight but irritable ulcer on the prepuce, clearly traceable to this cause; there was not the slightest reason to suspect a venereal taint, and the rapidity of its cure under very simple applications convinced me of the correctness of my opinion. This gentleman, when I first saw him, was in a considerable state of excitement; he could not but feel suspicious as to the cause of his malady: he soon calmed down, however, on having the nature of the case fully explained to him.

Treatment of Vaginitis.—The treatment of vaginitis must necessarily depend upon its character, whether active or passive—its extent, whether partial or general, and the state of the constitution in which it occurs—whether the patient be robust and plethoric, or in the opposite condition of debility and exhaustion.

Let us first take a case of recent acute vaginitis, with a constitution unimpaired, no debilitating effect having as yet been produced. Although inflammation of the mucous membranes of the body seldom if ever require the powerful mode of treatment adopted for the cure of inflammation when occurring in the serous membranes, you will nevertheless find it necessary in many cases to adopt mildly antiphlogistic measures, especially where the disease is extensive enough to produce general vascular excitement unattended with symptoms of depression. I need hardly tell you that the arrest of the flow is not the primary indication; for if this were suddenly suppressed, the natural result would be an increased engorgement of the vessels, which were to a certain extent relieving themselves by increased secretion.

Means must be had recourse to for the purpose of subduing the intensity of the inflammation; and if a favourable effect be produced by these means, the leucorrhœa will in all probability be increased in quantity and altered in quality: instead of remaining thin, ichorous, and irritating, it becomes more viscid and bland. In the severer forms of vaginitis, there is very often a little blood mixed with the vaginal discharge, so that the flow is streaked with blood. It is, as I have said, seldom necessary to make use of very powerfully depressing remedies for the subjugation of this form of inflammatory affection. The first thing to be done is to enjoin the recumbent position; and to this the patient must strictly and resolutely adhere; the cure will be greatly protracted without strict obedience to this injunction. In cases now under consideration, where the inflammation is acute, and the system in a plethoric state, marked by a hot skin, strong pulse, furred tongue, &c., a question as to venesection may probably arise: I say *possibly*, because it has been recommended by some; I have never seen a case in my own practice where I have considered general blood-letting to be either necessary or advisable. Great benefit is derived from the application of four or six leeches, just within the ostium vaginæ. This part is very vascular, and bleeds freely. The oozing from the leech-bites should be promoted by long-continued applications of warmth and moisture, either from the use of fomentations of warm water, or from the frequently-renewed applications of wetted spongeo-piline; the latter is in most cases preferable, as it preserves the bed-linen from being soiled or wetted. The bleeding orifices should be sponged occasionally, to wipe away any coagulum that may form and obstruct the flow of blood. In the early stage of acute vaginitis, relief is speedily obtained; the heat and pain subside, the sense of fulness diminshes, and the febrile symptoms lessen. The vagina should be frequently washed out with warm decoction of poppies, used as an injection. This acts beneficially in a twofold manner: in the first place it clears away the discharge, which has an irritating effect on the parts with which it comes in contact; and secondly, by acting as a fomentation on the affected parts, it assists in lessening inflammatory action. The necessity of frequent ablution, in some form or other, was insisted on by Heberden, whose words I now quote: "Humor iste, quanquam plerumque albus, ut vulgo appellatur, et aquæ similis, interdum tamen glutinosus est, et coloris subflavi, item subviridis et mali odoris, atque tam acris ut nisi partes in quas descendit sæpe eluanter, levis inflammatio fiat cum prurigine et dolore: cuticula quoque deteratur, et urina reddi nequeat nisi cum sensu pungentis caloris." In some few instances the application of cold is more agreeable to the sensations of the patient; and then the liquid should not be warmed before being used. Other forms of injection may be used, as the following:—

℞. Ext. conii. ʒ iij.; aq. destill., ℥ xij. Solve.

Or, ℞. Ext. belladonna, ʒ ij.; aq. destill., ℥ xij. Solve.

These may be continued or changed as circumstances require.

Mild laxatives are required in the early stage; and these should be repeated for a longer or shorter period of time, according to the urgency of the case. Drastic purgatives are to be avoided, as they are likely to increase the irritation already existing. The nitrate of potash combined with a saline aperient has often a beneficial effect. The following is my usual form of prescription:—

℞. Potas. sulphat., ʒ j.; potas. nitrat., ʒ j.; aq. menth. pip., ℥ iv.

M. et capiat ter die partem tertiam.

The diet must be spare and of an unstimulating quality, no fermented liquors being allowed until the inflammation has been subdued. This plan should be strictly pursued for a few days; and should there at that time be no relief—if the pain, &c., continue, leeches must again be applied, the potash mixture continued; and if the nights be restless and sleepless, an anodyne should be administered at night: half a grain of opium with one grain of chloride of mercury is generally sufficient to effect your purpose. It is seldom necessary to proceed further with the bleeding, and, indeed, it often happens that the second application is not required; the symptoms are alle-

viated, the uneasy feelings subside; the discharge may not be diminished in quantity, but it improves in appearance. Do not, however, discontinue the laxative altogether; let it be taken twice instead of three times in the day. In the more favourable cases this is all the treatment required: perfect rest; the application of leeches where necessary; the administration of laxative medicines; spare diet; and frequent ablution of the parts. The disease ends in resolution; that is to say, the inflammatory action ceases, and the membrane at once resumes its ordinary functions. Not so always; for after the active symptoms disappear, the disease perpetuates itself in the more chronic form: although there is to a great extent relief from pain, the discharge continues profuse, though not irritating; it becomes thick and glutinous, the patient's strength begins to yield, her vital powers diminish, and her spirits are depressed. If the parts be examined at this time, the membrane will be seen distended with blood, the colour being much darker than in the active form of the disease; little or no pain is produced by the examination. These cases are often very tedious and difficult of cure. Slightly astringent injections should be employed; *e.g.*,

℞. Plumbi diacetat., gr. xxiv.; aq. destillat., ℥ xij. Ft. lotio.

The general plan of treatment ought to be moderately tonic, and the recumbent position still adhered to. After a week or ten days the injection may be changed for the following:—

℞. Argent. nitrate, gr. xxiv. ad xxxvi.; aq. destill., ℥ xij. Ft. lotio.

The progress to convalescence will very much depend upon the patient's state of health; if this be seriously affected it will be long before the patient is restored to health, and a long course of tonic medicine will be required, together with the usual adjuvants for invigorating the powers of the system; namely, well-regulated generous diet, a due proportion of wine, and a moderate amount of exercise, taking care that this never amounts to fatigue.—*Med. Circular, Jan.* 9, 1861, *p.*, 9.

131.—ELECTRICITY IN AMENORRHŒA.

By Dr. Althaus.

I have found electricity externally applied a very valuable emmenagogue in young and unmarried women, in whom we are led to assume a torpid state of the vasomotor nerves of the ovaries and the uterus, and also in such cases where the catamenia have been supppressed in consequence of emotion or cold. Faradisation has proved especially useful; I have also employed the continuous current, but with less benefit; while I cannot speak from personal experience on the value in these cases of frictional electricity, which I consider an antiquated remedy, and which should no longer be employed in practice.

I am well aware that in many cases of amenorrhœa the application of electricity has been followed by negative results; and I am quite ready to admit that electricity, even when properly applied, is not infallible in the class of cases I have alluded to. But I consider that very often the method of application has been such as to render a beneficial result all but impossible. Only recently, a case has come under my notice in which a medical man believed himself to be engaged in applying the Faradic current to a woman suffering from amenorrhœa. On inspection of his manner of administering the current, however, it was discovered that he had united the conducting wires with the poles of the cell instead of with those of the coil! As the apparatus he had on the table was a powerful one, he was greatly astonished that his patient had not the slightest sensation of a current passing, and he concluded that it was a case of deep anæsthesia of the genitals! His astonishment increased when, the wires being put into their proper places, the anæthesia suddenly disappeared, and the patient called out that she was now fully sensible of the passage of the current! Although cases of

such gross ignorance may be rare, still there is no doubt that electricity is only seldom applied as it ought to be.

Another mistake is frequently committed which prevents beneficial results taking place. Doctors as well as patients are disappointed if no immediate benefit results from the employment of a "wonderful remedy," as electricity is unfortunately called. But perseverance is just as necessary with the electric treatment as it is with many internal remedies; and if perseveringly carried out, this plan of treating obstinate cases of amenorrhœa will be found to be attended with the best results, although signs of an improvement may be wanting for some time. In one patient whom I had recently under my care, the catamenia only appeared after the forty-sixth *séance*, when all hopes had been well-nigh given up by her; and it is only in a proportionately small number of cases that electricity yields rapid curative effects.

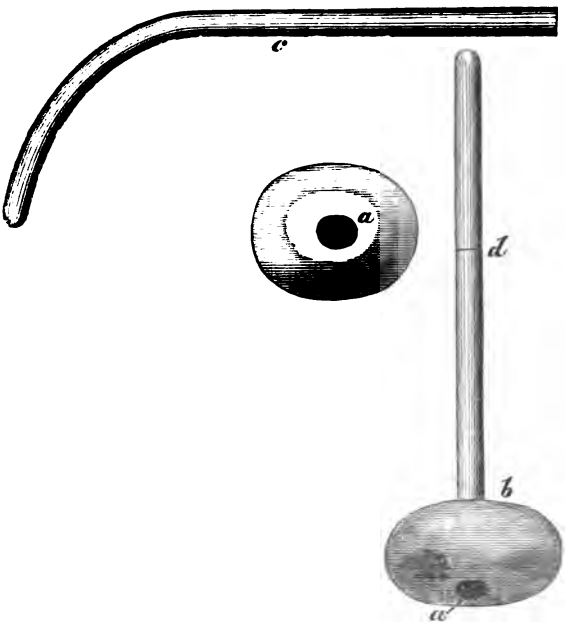
As to the parts of the body to which the current should be directed, I may add that Faradisation of the skin of the soles of the feet by means of wire brushes, or Faradisation of the abdominal parietes by means of moistened conductors, and finally the application of one moistened conductor to the nape of the neck and of another over the os pubis, are the most effectual methods in cases of this kind.—*Med. Times and Gazette, June* 22, 1861. *p.* 662.

132.—ON THE LOCAL TREATMENT OF AMENORRHŒA.

By Dr. J. Y. Simpson, F.R.S.E. Professor of Medicine and Midwifery in the University of Edinburgh.

[It often happens in cases where the constitutional causes of amenorrhœa are removed, that still the amenorrhœa persists, as if some local action or irritation required to be superadded in order to initiate the menstrual action. Under the term "local treatment" are comprehended various local emmenagogues applied to the vagina, and to the uterus. Direct stimulants applied to the uterus or uterine cavity, are undoubtedly the best means at our command for recalling the suspended menstrual functions. The best mode of applying a direct stimulant to the uterine cavity is the introduction and wearing of a small and usually galvanic intra-uterine pessary, respecting which Dr. Simpson makes the following observations].

Intra-uterine Pessaries.—This form of instrument consists simply, as you perceive, of a small copper bulb (see Fig. *a*, *b*), from the middle of which there rises a stem *d*, made half of copper and half of zinc, and measuring 2½ inches in length, or rather less than the length of the uterine cavity. In introducing it, the patient is placed as usual on her left side, and the forefinger of the right hand having been brought into contact with the os uteri, the instrument is guided along it either with the right thumb or with the fingers of the left hand until the point of it passes through the orifice into the canal of the cervix. Usually you will experience no great difficulty in passing the instrument in this way up as far as the os internum; and if you now remove the point of the forefinger from the os and apply it below the bulb of the instrument so as to press it gently upwards, you will sometimes succeed, with the greatest ease, in insinuating it entirely into the uterine cavity, till the upper surface of the bulb is felt to be in apposition with the lips of the cervix. In other cases, however, you will meet at this stage with some degree of difficulty, for the os internum is occasionally the site of a kind of stricture, and this state of matters is further not unfrequently complicated by a slight anteflexion of the whole organ at the union of the body and cervix, so that the point of the pessary is apt to be caught at this level, and its complete entrance into the uterine cavity obstructed. In such cases you will best effect your object by pressing the bulb of the bougie very much towards the hollow of the sacrum, so as to tilt forwards the point of it, and to make it move slightly from side to side, or from before backwards, when you will feel it suddenly passing the constricted spot, and the uterus settling down, as it were, on the instrument, without the exertion on your part of any active or injurious amount of upward pressure. I have spoken



Full-sized sketch of the galvanic intra-uterine pessary, *a b d*, and of the staff, *c*, used for introducing it. *a*, perforation on the lower surface of the bulb, *a b*, of the pessary, into which the point of the staff is made to fit; *b d*, stem, or intra-uterine portion of the instrument, consisting in the lower half, from *b* to *d*, of copper, and towards the point, of zinc.

of the introduction of the intra-uterine stem-pessary as being effected by the unaided action of fingers, because, for a long time past, I have never had recourse to any other means. But, as you may observe, there is, on the surface of the bulb opposed to that from which the stem springs, a small orifice, *a*, intended to admit the point of a staff, *c*, which may be used to facilitate the introduction of the pessary. The staff is made of steel, is about eight inches in length, and is slightly bent at the point, so that when it is fitted into the hole in the bulb of the bougie, the latter can be guided by means of it in any direction, with more precision and power. I fear, however, that no mere description I can give you as to the mode of passing the intra-uterine pessary will enable you in any case to effect its introduction. This is one of those little operations which you will only succeed in performing properly by a certain amount of practice; and I believe that after you have tried it a few times, so as to have gained a little tact and skill in the manipulation of the bougie, you will come to effect its introduction easily with the fingers alone, and discard altogether the use of the staff. There is one secret regarding the introduction of this instrument that I have to tell you. Sometimes you may have succeeded in passing the uterine sound easily enough, but are foiled in your effort to introduce the permanent pessary. In such a case you may save pain to your patient and trouble to yourself by re-introducing the sound, and leaving the patient lying with it in the uterus for half an hour or an hour, at the end of which time you will find it so placed or so patulous as readily to admit the intra-uterine bougie. But you may ask, will the instrument thus introduced remain of itself *in situ?* In the majority of cases it will; and always with most certainty in those where the internal orifice has been so contracted or curved as to render the introduction of it somewhat difficult. In other cases its re-introduction is attended with no difficulty or discomfort, and you will find that after

it has been worn for some days, the zinc half of the stem becomes covered with a white saline crust of greater or less thickness, which tends to enable it afterwards to keep its position. Though the zinc portion is generally deeply encrusted on the withdrawal of the instrument, the copper portion is always, on the contrary, clean and clear of all deposit. Before re-introducing it, remove the crust from the zinc portion, and wash it with vinegar. If you find it necessary you may sometimes introduce below the instrument, as I have not unfrequently done, a small gutta-percha pessary, such as we use in the treatment of prolapsus uteri, which will support the intra-uterine pessary and prevent it from slipping out. I never saw the use of this instrument for the cure of amenorrhœa attended with any untoward result, and I and my assistants have employed it in a very great number of cases. Often we see its introduction immediately followed by the most marked results, especially with regard to the cure of the headaches and other secondary symptoms; and if worn for a sufficient length of time, it does not often fail in exciting the uterus to the performance of its functions; or in recalling them when they have become suspended. I have seen menstruation speedily recalled by it after almost shop-loads of physic had been used in vain for the purpose. It produces its effect, I believe, simply by the gentle and continued stimulation of the whole uterine system. Probably other kinds of foreign body lodged there would produce much the same effect; and I have treated cases of amenorrhœa with success by the introduction of a stem pessary made entirely of copper or of German silver, such as is used for dilating the cervical canal in cases of dysmenorrhœa. But in cases of simple amenorrhœa I prefer to have the instrument made of two different metals, as suggested by Dr. Weir, because the slow galvanic action resulting from the chemical changes effected in the zinc increases the chances of a speedy and successful issue. For, let me remark, finally, there is yet one other means which has often been much relied on and bepraised as eminently serviceable for the cure of amenorrhœa, viz., the application to the uterus of

Electricity and Galvanism.—Among their multifarious medical applications, galvanism and electricity have been from time to time employed in the treatment of various uterine disorders; and in the latest American work on their use in medicine, by Dr. Garratt, of Boston, I find the author speaking of them as almost the only certain means of exciting the action of the uterus in cases of amenorrhœa. To apply them to this organ you need to be provided with special forms of conductors. I believe that the slow but long-continued action of the intra-uterine galvanic pessary will produce the effect with far more certainty, than the repeated application of a stronger current; and I am sure that when you have found how easy it is, after a little practice, to introduce the instrument, and when you have witnessed in a few cases the safety and success that attend its employment, you will become more and more inclined to treat those very obstinate cases of amenorrhœa by means of the galvanic bougie rather than by any more irksome or prolonged plan of treatment. The galvanic pessary, however, is a measure which you would not be justified in ever employing in young and unmarried patients, except the great obstinacy of the case, the severity of the sympathetic or secondary effects, or the danger to the patient's health and life, gave you adequate warrant for such interference. In the less claimant cases of amenorrhœa we would rejoice to find any external mode of applying electricity effectual as an emmenagogue, whether the electricity were derived from induction, as when the so-called "Faradaic currents" are used; or from chemical decomposition, as in the various forms of galvanic battery; or from friction, as when electric sparks and discharges from the Leyden jar are employed. Of the effects of Faradisation in amenorrhœa I know little practically. But I have seen the galvanic current employed some fifteen or twenty years ago often, and certainly, on the whole, with little or no emmenagogue effect. About a quarter of a century ago electric sparks directed through the uterine region were tried in many cases of amenorrhœa by my predecessor, Dr. Hamilton, and my friend Dr. Ziegler, who is most thoroughly acquainted with medical electricity and its applications in practice.

I have Dr. Ziegler's high authority for stating that, after making many trials of it in amenorrhœa the impression left on his mind is that, as an emmenagogue, it is in the main quite ineffectual and useless. It is but right, however, to add that some observers, as Dr. Golding Bird, have arrived at a different and far more favourable conclusion, but I am inclined to place great reliance upon the results of Dr. Ziegler's experience on this subject.—*Med. Times and Gazette, June* 15, 1861, *p.* 622.

133.—CASE OF DYSMENORRHŒAL MEMBRANE.

By Dr. Beatty.

At a meeting of the Pathological Society of Dublin, Dr. Beatty presented a specimen of membrane formed and thrown off during difficult menstruation by a young married female. He observed, that from the peculiar character and appearance of membranous growths of this kind identical in structural aspect with "decidua vera," and their being formed and thrown off during the paroxysms of dysmenorrhœa by unmarried females, they sometimes gave rise to painful suspicions and mental disquietude on the part of the patient's friends and relatives. These adventitious growths, peculiar to dysmenorrhœa, are found to occur in maidens as well as in married women, and the resemblance which they bear to some of the fruits of conception sometimes give rise to suspicion of pregnancy. The specimen now presented to the notice of the society had been obtained from a young woman, who had been, for a long time, suffering from painful menstruation, and for twelve months previous (since her marriage) had voided one of those membraneous growths every month, though in her maiden condition nothing of the kind had occurred. Dr. Beatty had seen a membraneous formation similar to that now produced, occurring in this patient on not less than four previous occasions. The membrane now exhibited was of recent formation and, in fact, had been passed only a few days previous. Its expulsion had been accompanied by considerable pain, and with scanty catamenial discharge. The severity of the pains and the peculiar appearance of this seemingly organized substance, gave rise to the supposition, that the female in whose case it occurred had aborted.

This growth, strongly resembled the "*decidua vera*," and in Dr. Beatty's opinion it was the result of a process similar to that which produced the true membrane. It was now generally believed that during each period of menstruation an ovum is detached, which, if not impregnated, of course loses vitality and becomes effete, and that a corresponding action is set up in the uterus for the reception of the disengaged ovum. Dr. Beatty said he believed that in those cases of dysmenorrhœa the formation of this pseudo-deciduous membrane is the result of the natural uterine effort exerted in an exaggerated degree. He said it was probable that in cases of natural and healthy menstruation there is always some membraneous formation, but it is generally so extremely thin and fragile, that it escapes notice, and passes away with the flow of the catamenia. In dysmenorrhœa, however, when the discharge is obstructed and attended with pain and symptoms of sub-acute local inflammation, the membraneous growth is of a more solid form, as in the present specimen, which, as regards thickness, consistence, and appearance, is similar to the *decidua vera* observable in the fourth month of pregnancy. In fact, no obstetrician, however experienced, on viewing this specimen, could take on himself to assert that it was not a result of impregnation. Dr. Beatty, being cognizant of the facts of the case, and knowing this membrane to be a consequence of dysmenorrhœa, had deemed it of sufficient importance to submit it to the notice of the society. He said it was a case exemplifying the necessity for great caution on the part of a physician in forming or enunciating an opinion on a subject so likely to involve serious considerations.

The specimen, which equalled in size the decidua vera of an ovum at the

third month of pregnancy, was fresh and in excellent preservation.—*Dub. Hospital Gazette, June* 1, 1861, *p.* 172.

134.—ON DYSMENORRHŒA AND STERILITY.

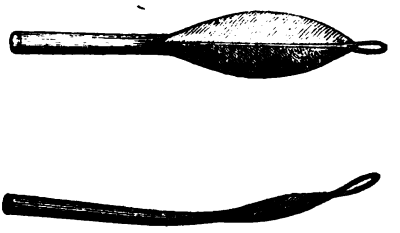
By Dr. JOHN COGHLAN.

[The writer has been very successful in relieving some cases of dysmenorrhœa depending on mechanical obstruction, by incising the os and cervix uteri in a mode which is different from that usually adopted.]

Dysmenorrhœa depending on mechanical constriction of the cervix uteri, I believe to be much more frequent than some of our writers on the subject would lead us to suppose, while I consider the pure neuralgic form of the disease to be very rare. In those cases where we find obstruction at the cervix uteri, and that great nervous susceptibility exists at the same time, we are sure to have uterine and ovarian congestion, and these causes acting and reacting one on the other, produce the great amount of suffering which we find in bad cases of dysmenorrhœa. It might be thought from a patient passing an occasional menstrual period without pain, that no mechanical obstruction existed, but from careful observation I have good reason to believe that we are not justified in this conclusion. For should the subject of dysmenorrhœa, at the accession of a menstrual period, be favourably situated for the gradual secretion of the fluid, and the prevention of local congestion (confined to bed, for example) it is by no means unlikely that she may pass the period with little or no pain, though more or less of mechanical constriction of the cervix uteri, when brought into play under ordinary circumstances, may be the whole source of her sufferings. Having entertained these opinions for some years, I have from time to time treated bad cases of dysmennorrhœa by endeavouring to dilate the os and cervix uteri by means of bougies and plugs of compressed wood. These latter, from their swelling by the absorption of moisture from the parts, produced rather satisfactory results for a time, but these results were not permanent, and the same proceeding had to be repeated. I never hesitate in my practice to make a vaginal examination, or use the speculum, when I consider it necessary, but I have an utter objection to have to repeat it day after day, as in dilating the cervix uteri by bougies, for in addition to its being disagreeable to our patients, I think it has a tendency with most women to diminish their self-respect. For these reasons I have given up the practice of dilatation for a considerable time.

About eight months ago, a woman, about twenty-six years old, suffering from a very bad form of dysmenorrhœa, applied to me for relief. She stated that she had been a great sufferer from her first menstrual period up to the present, in fact, that from that one cause she had spent a life of misery. She had been four years married, had no children, and her suffering at the menstrual period had become much increased since her marriage. On examination I found the os uteri much enlarged, and presenting a flat disk nearly an inch and a-half in diameter, instead of its usual rounded form, the external orifice was small, and the constriction was so great at the cervix that I could not pass a No. 1 flexible bougie. At a subsequent examination I carefully, but with some difficulty, passed the stylet of a catheter into the cavity of the uterus. I then passed into the cervix a small plug of pine-wood compressed by drawing it through five or six holes of a wire draw plate, and thus gained some amount of dilatation of the canal. On the following day I incised the os and cervix by means of the instrument represented below. Having perfected the incision, I took a piece of sheet lead rolled out so thin as to be very light and flexible, but sufficiently thick to retain its shape, about an inch long, and three-fourths of an inch wide, rolled it in its width into a small tube and turned the edges of the ends slightly out into a lip. I introduced this tube into the incision, and then by passing the blades of a long thin dressing forceps into the tube, and opening them a little,

I dilated it to the full extent of the incision. On examination two days after, I found the tube in its place, I had to replace it twice within the following week, from having fallen out, but recovery took place without any bad symptoms, I then lost sight of my patient till about a fortnight ago, when I learned that at the next menstrual period she had considerable pain, but so little in comparison to her former sufferings, that she had not recourse to the opiates which she had been accustomed to use; since then the pain has been gradually diminishing, she had not then menstruated for seven weeks, and I think it probable that she is pregnant.



Front and Profile View of Dr. Coghlan's Probe-pointed Metratome.

This instrument consists of a central blunt or probe-point less than a quarter of an inch long, and about the thickness of a No. 1 bougie, and proceeding from this are two cutting sides about three-eighths of an inch wide. The whole instrument, including the handle, is about eight and a-half inches long, and is slightly curved for three inches from the point, to correspond with the oblique position of the uterus. The advantage of the instrument is that, guided by the probe-point, we are sure with ordinary care to have our incision right into the uterine cavity, and perfectly central, and by using instruments of different widths we can have our incision of the exact extent we desire. By means of the leaden tube we get rid of the disadvantage of having an absorbent, offensive, and irritating plug, and of the necessity for frequent examinations.—*Med. Times and Gazette, June* 1, 1861, *p.* 572.

135.—ON INFLAMMATION OF THE BREAST, WITH AN ANALYSIS OF SEVENTY-TWO CASES.

By Thomas William Nunn, Esq., Assistant-Surgeon to the Middlesex Hospital.

[Of the total number of 72 cases which have fallen under the author's observation, 58 occurred during lactation. Of these 57 per cent. occurred during the first two months of lactation, and 14 per cent. during the subsequent seven months, and after the ninth month 29 per cent. This appears to show that there must be a special cachexia brought about by over lactation. Both breasts are equally liable to inflammation. In speaking of the treatment the author remarked upon the following points.]

1. That the continuous poulticing so often practised or permitted was decidedly mischievous, and hindered recovery, although it was not denied that occasionally, where there was deep sympathetic pain and hyperæsthesia of the surface, large warm poultices were soothing and grateful.

2. That the recumbent position, by preventing the undue infiltration of the lower lobes of the gland, was of the first importance.

3. That in the author's hands belladonna had not given encouraging results, although he could not doubt, from the evidence of Mr. Marley and others, that in the earliest stages of congestion and as a preventive it had a beneficial influence.

4. That the moment for making the incision in the abscess, when formed, should be vigilantly watched for, as the rapidity of cure much depended

upon this. That the extent of the incision should be such as to ensure efficient evacuation of the abscess, but nothing further.

5. The author strongly advocated the employment of galvanism of low intensity, such as is afforded by the ordinary single cell apparatus for yielding the interrupted current, in the treatment of sinus and painful œdema remaining after the more acute symptoms have subsided; acknowledging his indebtedness to a paper by Mr. Spencer Wells, published some years since, for the idea of so using galvanism. Whether galvanism stimulated the blood-vessels directly, or, by promoting the activity of the absorbents and the removal of the exudation products, permitted the capillary circulation, relieved of the *debris* by which it was clogged to resume its normal condition, no hypothesis was hazarded. Of the great value of galvanism as a therapeutic agent the author had seen abundant proof.

Mr. BALLARD remarked that he thought the author, in assigning the cause for mammary abscess, had laid too much stress upon the health of the mother, and had not made sufficiently prominent that which seemed to be really the exciting cause of the evil—namely, a demand made upon the breast for an amount of secretion it was at the time unable to yield. The statistics quoted were in accordance with general observation, and seemed to corroborate this view, the occurrence of the greater number of cases in the earlier periods of suckling being caused by the child sucking in search of a greater quantity of milk than the gland has yet acquired the power of secreting. Again, in the latter periods there may be a failure of secretion in consequence of the re-establishment of menstruation or of pregnancy; the demand continuing as before, the breast is exposed to the irritation of excessive sucking. Breasts of which the nipples are defective are very liable to become inflamed; their secreting power is usually very imperfect, although the determination of blood to them is the same as if they could secrete freely. Vain attempts to procure milk under these circumstances are very apt to induce inflammation and abscess. In the treatment of an abscess of the upper segment of the breast he had found the drainage-tube very useful; it enabled the pus to flow freely when otherwise none could escape.

Dr. TYLER SMITH observed that the chief causes of mammary abscess soon after parturition were obstructed milk-ducts, and the irritation of the gland by fissures of the nipple. The latter caused suppuration of the glands just in the way that irritation of the urethra or vagina caused suppuration in the inguinal glands. The best way to prevent the abscess was to cure the crack in the nipple, and he knew of nothing better than painting it with a strong solution of the nitrate of silver. Abscess depending on obstruction of the gland could often be prevented by belladonna. He had found smearing the breast with equal parts of the extract and water a better way of applying it than in the form of ointment. Bromide of potassium seemed to have a specific effect in diminishing the action of the mammary glands, both when given internally and used as a lotion. In the important class of cases first pointed out by Mr. Nunn as occurring at the end of lactation, and due apparently to fruitless sucking, the weaning of the child was always advisable. In chronic abscess, support and compression of the breast by strapping or belladonna plasters, leaving the openings of sinuses free for the discharge of matter, was of the greatest possible use.

Dr. TANNER observed as to the women who most frequently suffered from inflammation of the mammæ, that it was much more common in strumous, weakly subjects than in others, and that such labours as were attended with *flooding* seemed to predispose to it. Consequently he quite agreed with the author that, speaking generally, antiphlogistic remedies were contra-indicated. Then as regards the use of belladonna in preventing the inflammation running on to suppuration he (Dr. Tanner) had great faith in it. He thought that this drug often failed to be of use because it was not employed with sufficient freedom. The best plan was to paint the whole gland with it, and then apply a cold bread-and-water poultice, repeating the application twice or thrice in the day. As to the ill effects of over-lactation, no one who was engaged in the hospital practice could fail to notice them. He remarked

that he never sat in the out-patient room without seeing several cases where the women were doing themselves great mischief by undue lactation, and frequently he found remonstrance useless, as the patients knew they were acting unwisely.—*Lancet, June* 22, 1861, *p.* 612.

136.—ON SACCHARINE FERMENTATION IN THE MILK WITHIN THE FEMALE BREAST.

By Dr. George D. Gibb.

The discovery of vibriones in human milk, by Vogel was announced in a paper published in 'Schmidt's Jahrbucher,' in 1852. He clearly proved that these animalcules were developed within the mammary gland, from the fact of their being seen in the milk on the instant of withdrawal. He believed them to be the result of fermentation in the milk itself, the result of congestion and increased heat in these organs, connected with general excitement of the sexual system. Vogel's theory of their formation was combated by the observation, that, as the milk containing them was either alkaline or neutral, and not acid, were their fermentation, it was argued, the evolution of lactic acid would immediately destroy the infusoria.

In the latter part of 1854, I was induced to make some researches into this important question, from the circumstance of an infant being brought to me seven weeks old, in the most extreme state of emaciation, whose mother had the appearance of the most perfect health. The child, although merely skin and bone, was healthy and plump at birth, and on very careful examination no disease could be discovered. It had never been satisfied with the large amount of milk it received, but was ravenous, and had to be spoon-fed besides. This was a first child, and the mother seemed the *beau-ideal* of an excellent and healthy nurse. The child had no diarrhœa, but the most profuse diaphoresis and diuresis had worn it to a shadow.

What was the cause of this? An examination of the milk, carefully made, at once furnished the clue. It was rich in cream, neutral, sp. gr. 1032, and showed the presence of a large quantity of sugar. So far it seemed normal. Examined under the microscope with a high power, seven hours after withdrawal, it revealed myriads of living animalcules, those indeed known as *vibrio baculus*, but which I venture to change to *vibrio lactis* as more appropriate. These were, to my mind, unmistakably the result of fermentation of the saccharine element in the milk, and might be owing to the large quantity of sugar present; but whether occurring in or out of the breast I had yet to determine. Next day I examined the milk as drawn from the breast, and found the *vibriones* present as before, incontestibly proving that the fermentation took place within the gland, as I had previously believed. There was an absence of mammary congestion and heat, but much sexual excitement, which it became necessary to control by moral and suitable medical treatment.

I did not altogether order the child to be weaned, but prescribed a diet with plenty of good cow's milk, and occasionally the mother's milk which it did not seem prudent to stop altogether. From this time the improvement began; the extreme action of the skin and kidneys ceased, the secretions became normal; and in a few weeks the child had become fat and hearty, and after a time was wholly weaned. The mother's condition also improved; but the milk always remained neutral, its specific gravity varying from 1032 to 1035, very rich in sugar, and containing the animalcules for some weeks. The richness of the milk became less as the child was gradually weaned, when it assumed a bluish tint. As quantities of it were drawn artificially, I had many opportunities of examining it with other specimens of milk; and the general result of my experiments went to prove the presence of a large amount of sugar, and that it turned sour much sooner than cow's or healthy human milk.

From 1854 up to the present time I have examined many hundreds of spec-

imens of human milk, chemically and microscopically, and have occasionally found animalcules to be present, in that secreted from the glands of those whose general health was disordered from various causes during lactation, or where the process of lactation itself was unusually prolonged, or again where the quantity secreted was small and wholly insufficient to satisfy the wants of the infant.—*Glasgow Medical Journal, July*, 1861, *p.* 250.

137.—WEIGHT OF NEW-BORN INFANTS.

Dr. E. von Siebold presents a table of the weights of 3000 infants (1586 male and 1414 female), weighed immediately after birth. From this table (for which we have not space) it results that by far the greater number of the children (2215) weighed between 6 and 8 pounds. From 5¾ to 6 lbs. the numbers rose from 99 to 268; and from 8 to 8¼ lbs. they fell from 226 to 67, and never rose again at any weight to 100. From 8¾ to 9¼ lbs. they sank from 61 to 8, rising, however, at 9½ lbs. to 21. Only 6 weighed 10 lbs., 1 10¾ lbs., and 2, 11 lbs. The author has never but once met with a child weighing 11¾ lbs. The most frequent weight in the 3000 was 7 lbs. numbering 426. It is a remarkable fact that until the weight of 7 lbs., the female infants exceeded the males in number, the latter thenceforward predominating. This agrees with Scanzoni's figures, who found that the weight of the male infants in 9000 births averaged 7 lbs. 3 ij. or 3 iij; and of female infants, 6 lbs. 3 xxviij., the female infants weighing less than 7 lbs. more frequently than the males. From these statements and those of various other writers here quoted, the conclusion may be drawn that the normal weight of a mature new-born infant is not less than 6 nor more than 8 lbs., the average weight being 6½ or 7 lbs., the smaller number referring to female and the higher to male infants.

Chaussier and Quetelet have already shown that for the first few days after birth a diminution in the weight of the infant takes place, and since the beginning of 1859 the author has paid much attention to this subject, having carefully examined the weights of 49 children. In 35 of these diminution did take place, 16 of the number losing ¼ lb., 14 ½ lb., and 5 1 lb. In these last instances, however, the nutritive process was manifestly defective, and the children did not rally again, so that they cannot be included in the statement. The normal diminution of weight was observable in the 30 children from the second to the third day after birth, the weight remaining then the same to the fourth, or even the sixth day. From the fifth to the seventh day, mother and child being well, the weight which existed at birth was again attained, and then increased. In 14 of the children examined, neither decrease or increase of weight was observed until from the sixth to the eighth day, when increase commenced. The mean of Quetelet's observations is thus stated:—The child weighed at birth 3126 kilogrammes; on the second day, 3057; on the third, 3017; on the fourth, 3035; on the fifth, 3039; on the sixth, 3035; and on the seventh day, 3060 kilogrammes. This diminution in weight in most of the children, and the arrest of its increase in others, is evidently dependent upon the change in its mode of nutrition on arriving in the world. The character of the early milk is very different to that which it afterwards assumes, containing more colostrum corpuscles than milk globules, and possessing rather a purgative than nutritive action. Not only is the child's weight found to diminish, but its entire body gives signs of the defective nutrition, the redness of its surface being exchanged for one of a yellow, or at least a white colour, while there is also observable a certain relaxation of its limbs,—signs which all disappear again in a few days. The author is sensible that his numbers are too small to enable him to arrive at any decisive conclusions, and he appeals to other obstetricians for their co-operation in the investigation. The length of new-born infants exhibits much less difference than does their weight. Of the 3000 children measured, the length varied from 15 to 21 inches; but out of the number no less than 1674 measured 18 inches, and 695 17 inches; so that the mean

length was between 17 and 19 inches. As in the case of the weights, so in the lengths, the females exhibited the lesser numbers. Thus, of 695 children which measured 17 inches, 380 were females, and 315 males; while of the 1674 which measured 18 inches, 867 were males, and 807 females. Of the 305 children 19 inches long, 198 were males, and 107 females; and of the 49 which measured 20 inches, there were 37 males, and only 12 females. Elsasser and Quetelet observed similar results.—*Medical Circular, April* 10 1861, *p.* 259.

138.—RESEARCHES ON ASPHYXIA, WITH OBSERVATIONS ON THE EFFECTS OF THE HOT BATH.

By Dr. A. T. H. Waters, Lecturer on Anatomy and Physiology in the Royal Infirmary School of Medicine; Physician to the Northern Hospital, Liverpool.

Amongst the numerous researches which had been made with reference to asphyxia, the author was not aware that any had been directed to one of the subjects he had investigated. Physiologists are agreed as to the order in which the arrest of the vital action takes place in asphyxia, but not as to the duration of the heart's action, nor yet as to the best mode of treatment in suspended animation. The different societies, whose aim is to save life, issue rules of the most opposite character for the restoration of those apparently dead.

Two important points remain to be decided: first, the period after asphyxia has commenced during which treatment is likely to be successful in restoring animation; and, secondly, the value of the hot bath as a remedial agent.

Experiments had been instituted by the author with reference to the following questions:—

1. How long does the heart continue to beat in asphyxia?

2. What are the effects of the hot bath on an asphyxiated animal—firstly, after all respiratory movements have ceased, and are not re-excited; secondly, when respiration has been re-excited, and is being feebly carried on?

It is difficult to decide with any degree of certainty with regard to the first question. Certain circumstances tend to throw a doubt on the generally received opinion, that "in asphyxia the movements of the heart cease in a few minutes after the cessation of the functions of animal life." The second question is more readily answerable, but the author was not aware that any experiments bearing directly on it had been performed previous to his own.

The subjects of experiments were dogs, cats, and rabbits. They were drowned in water varying in temperature from 40° to 50° Fahr., and in one instance 36°. On being removed from the water, after every external symptom of life had disappeared, they were opened by the removal of the anterior part of the chest, so that the movements of the heart could be observed.

Some of the experiments were parallel—i. e., two animals of the same age and size were chosen, and after being drowned in the same way, were opened at the same time; the difference being that one animal was, previous to being opened, put into the hot bath at 100°, and the other was left exposed to the atmosphere. The number of animals experimented on as just mentioned was 28; of these, two being set aside which were submerged for an hour, in 18 the heart was found beating when first observed; in 8 its action had ceased.

The animals were opened at periods from the commencement of asphyxia varying from the fifth up to the thirteenth, and in one instance the twenty-first minute. The average period during which the ventricles continued to contract was nineteen minutes; the longest period was in a rabbit—forty-five minutes.

With regard to the first portion of the second question, the morbid appearances of the animals put into the hot bath were compared with those of the animals not so treated. In the animals put into the bath, the lungs were much more congested, more full of blood; they were firmer in substance and specifically heavier than those of the animals not so treated. Both sides of

the heart were loaded with blood. In some instances the blood was coagulated in the vessels of the lungs, the systemic veins, and the cavities of the heart. The blood was generally less fluid than in the animals not put into the bath, and coagulated more rapidly when removed from the vessels. In no instance did the bath produce a respiratory effort or any movement whatever on the part of the animal; it seemed, further, to shorten the duration of the heart's action.

With regard to the second portion of the second question, experiments of the following character were performed:—

Animals were drowned in water from 45° to 50° Fahr.; they were kept under water, some for one minute, some for one minute and a quarter, and one for two minutes. When removed from the water, they were placed on the table exposed to the air. They soon began to breathe feebly. In order to compare the effects of the hot bath with those where all treatment was omitted, some of the animals were left to themselves, others were put into the bath as soon as respiration had been re-excited.

Thirteen experiments were performed—twelve on rabbits, one on a cat. Of the thirteen, seven were put into the hot bath; of these, six died, at periods varying from two to twenty hours after submersion. Six animals were left to themselves; of these, four recovered, and two died, both between the eighth and twentieth hour after submersion.

The animals which had died after being put into the hot bath presented the following morbid appearances. The lungs were dark-coloured, full of blood, firm, almost liver-like in appearance. In some instances, portions sank in water; the air-tubes were empty; there was blood in all the cavities of the heart, in one instance coagulated. In the animals which died after submersion, but which were not put into the hot bath, the lungs were somewhat firm and congested, but to a less extent than in the others.

The experiments appear to the author to be sufficient to establish the principle of the injurious influence of the hot bath, both when asphyxia is complete and when recovery is commencing. In the former case, the bath momentarily increases the circulation; but respiration being in abeyance, the lungs become loaded with blood, and the left side of the heart distended. In the latter case, respiration being imperfect, engorgement of the lungs takes place, and subsequent arrest of the heart's action.

The following conclusions are drawn by the author from his experiments:—

1. That in asphyxia by submersion, the ventricles of the heart do not, as a rule, cease to contract "in a few minutes after the cessation of the functions of animal life," but that in many instances their action continues for a very considerable period, and that this serves to explain how recovery has taken place after lengthened submersion.

2. That in cases of asphyxia where respiration has altogether stopped, the effects of the hot bath are: to produce an accumulation of blood in the lungs and in the left side of the heart, together with a tendency to coagulate on the part of the blood; that it does not tend to prolong the action of the heart, but rather to paralyse its movements, and diminish the duration of its contractions; that it does not excite respiratory efforts, and prevents artificial respiration being properly carried out.

3. That in cases of asphyxia where respiration has been re-excited and is being feebly carried on, the hot bath, although in some instances it seems to have had no immediate bad result, yet has a tendency to produce a fatal issue some hours after its use, by causing extreme congestion of the lungs, together with consolidation and collapse of the pulmonary tissue.

The following practical inferences are drawn from the above conclusions:—

1st. That efforts to restore animation should be made in all cases where asphyxia has not been of very prolonged duration.

2nd. That the prolonged use of the hot bath in asphyxia is not only inefficacious, but dangerous; and that its temporary use appears to be attended by no direct benefit. So far as any means similar to that of the hot bath are likely to produce respiratory movements, the alternate dashing of hot and cold water on the body is probably the most efficacious.

3rd. That it appears safer practice to omit all artificial treatment, when respiration is going on feebly, than to make use of the hot bath.

4th. That in the treatment of asphyxia all efforts should be primarily directed to restoring, or continuing, the respiratory movements; and all measures tending to load the lungs or embarrass the respiration should be avoided.

The author believes that the best method of performing artificial respiration we are yet acquainted with is that recommended by Dr. Marshall Hall.

A letter was read at the Royal Medical and Chirurgical Society from Sir BENJAMIN BRODIE relative to the paper 'On Asphyxia and the use of the Hot Bath,' by Dr. A. T. H. Waters. The letter referred to the question of the period of the continuance of the heart's action in cases of asphyxia. Sir B. Brodie had never, in his experiments on the subject, known the rhythmical contractions of the heart to continue for more than four minutes and a half after complete submersion; and believed that if they had once ceased in asphyxia they could not be restored. Cases of recovery after a longer submersion he attributed to the action of the heart having previously ceased in a state of syncope.—*Lancet, May* 25, *and June* 8, 1861, *pp.* 513, 563.

139.—THE ATROPHY OF CHILDREN FROM DIRT EATING.

By Dr. WILLIAM MOORE, Physican to the Institution for Diseases of Children, Physician Extraordinary to Sir P. Dun's and Mercer's Hospitals, &c., Dublin.

Travellers have long since informed us of the earth-eating habits of the natives of many tropical countries, more particularly those of the islands in the Indian Ocean, where pregnant women made a practice of baking, roasting, and eating a reddish clay, impregnated with iron. Humboldt has mentioned earth-eating as common to various tribes in South America; he especially refers to the Ottomaques, who, during the floods in the Orinoco, which prevent their catching fish, eat a peculiar clay; these savages, by no means an unhealthy race, considered this clay nourishing, and Humboldt doubted whether or not portions of it might not be assimilated. Again, fugitive negroes in the West Indies have been known to subsist on earth for a considerable time, and a case is mentioned by Mr. Mason, of a fugitive negro boy, who, when conveyed to the property to which he belonged, was found to have swallowed a long rag of blue baize, the end of which projected from his mouth. Psychological reports have detailed extraordinary instances of lunatics indulging in the most indigestible and disgusting commodities with comparative impunity. The following cases which have come under my observation within the last few years, tend to show what a destructive habit or rather disease pica or dirt-eating in children becomes. The first case I shall adduce was that of Ann F. V., aged two and a-half years, who was brought to the Hospital for Children, Pitt-street, in November, 1859. The child's lips were anemic and bloodless, her skin of a yellowish hue, her expression downcast and forbidding; on examination I found palpitation, a tumid, irregular abdomen; bowels at times lax, at others, obstinately constipated; discharges generally of a pale, plastic, offensive character. On inquiry, I learned that this child, when left to herself, ate gravel, lime, cinders, in short, every refuse she could lay hold of. The treatment employed in this case was repeated enemata of soap and water, which brought away gravel, stones, and indigestibles of all sorts, at the same time small doses of grey and rhubarb powder were given night and morning, and a tepid bath at bed time. Under this treatment with careful watching to prevent her satisfying her vicious craving, this child was gradually restored to health, which improvement was further confirmed by the exhibition of syrup of iodide of iron and tepid sea-water baths.

Case 2.—Was that of William T., aged two years, a chlorotic boy, lips and gums blanched and anemic, with deep unhealthy fissures at each angle of the mouth: skin generally dry; his look dull and drowsy, and his temper

very irritable. On examining the abdomen, I found dullness over the right hypochondriac and even down to the iliac region; the belly was tumid, and there was evidence of slight effusion. The boy's extremities were œdematous. Diarrhœa had been persistent for weeks despite of all treatment. The mother of this child attributes his malady to eating earth, lime off the wall, and gravel, the passing quantities of which, by stool, first made her aware of the habit. The treatment adopted in this case was three grains of the powder of chalk with opium, twice daily, with tepid mustard baths at bed time, beef-tea and wine liberally; but all failed to control the diarrhœa; the case went on from bad to worse, and eventually proved fatal.

Case 3.—In August 1859 I was consulted about Michael B., aged five years, a fine stout boy, with a most remarkable expression, he is unwilling to raise his head or look up, and when obliged to do so, there is a heavy vicious look about him, so unnatural to a child of his age; there is not a general sallow hue of the skin, but a circle around each eye, engaging the upper part of both cheeks, partakes of that character. This child's mother has used every means to prevent him eating refuse in general, but coal and cinders in particular, and even when his hands have been sown up in linen or cotton bags, he has been found on all fours trying to get his head under the grate; he was always a very violent tempered child, and latterly has become even more so. The abdomen in this case was hard, but not irregular or tumid; occasionally he suffers from diarrhœa, when the dejections are of a putty-like character and in enormous quantity; when I first saw him, his bowels had been constipated for days. This lad took two grain doses of grey powder, with rhubarb and scammony, at bed-time, followed by infusion of senna the next morning, till the bowels were well-cleared of all their impactions; he got a tepid bath nightly for weeks, and a servant was set apart to prevent him indulging his vicious craving. Under this treatment his secretions were restored to a healthy condition; he lost the yellowish circles round the eyes, and, pari passu, the downcast expression. He is now a fine rosy-cheeked boy.

Case 4.—Mary M., aged two years, a cachectic child, of a pale straw colour, covered with lichen, was brought to the Pitt-street Institution for Children in March last. This girl's mother has failed to prevent her swallowing coal, pieces of glass, and prawn shells; except for some such unnatural edibles the child has no desire. The general atrophy, coupled with continuous diarrhœa of a slimy, bloody character, rendered the prognosis of this case far from hopeful; however, under the continued use of rice-milk, with small doses of the compound powder of chalk with opium, and tepid bran baths at bed-time, the diarrhœa was controlled, and the lichen disappeared to a considerable extent.

Case 5.—Ann B., aged two years and three months, was brought to me in September last. From the downcast appearance, or rather loss of expression, the fixed and sluggish state of the lids, coupled with the yellowish anemia, led me at once to suspect the origin of the mischief. The abdomen was hard and enlarged, the child was suffering from diarrhœa, the dejections, as her mother described them, being like "bad eggs." This child's craving was principally for earth and gravel. Under the use of small doses of gray rhubarb and aromatic powder, with tepid mustard baths at bed-time, and close watching as regards her diet, this child has gained a healthy look, and her expression is considerably restored; she has latterly been taking syrup of the iodine of iron.

Case 6.—Patrick C., a little over two years of age, was brought to Pitt-street within the last month; a pale, sallow child, with loss of expression; he has been in the habit of eating small pieces of bricks, lime and earth for the last six months; his breathing is hurried; he suffers from palpitation and incessant diarrhœa, from twelve to sixteen times during the day and night, the fæces being slimy, bloody and jelly-like, and extremely fetid; he is ascitic; and his extremities emaciated. Under the combination of grey and compound powder of chalk with opium, with mustard sitz baths at bed-time, rice-milk, and strong beef-tea as diet, he seems to be improving, but as yet the

amendment is not decided. The most remarkable features diagnostic of dirt-eating in children is the fixed, vacant, or downcast look, any expression remaining being forbidding, occasionally vicious. In most of these cases there is an unwillingness on the part of the child to raise the eyelids, and from this cause the levatores palpebrarum become atonic or semi-paralyzed from disuse, the lids drooping over the eyes; coupled with these signs, you have anemic lips and gums, a tinge of the cutaneous tissues, usually varying from a straw to a deep yellow; in addition, the arrest of secretions generally; with palpitation; tumid, irregular, occasionally ascitic abdomen; and intractable diarrhœa or the converse, leave little doubt as to the "prima origo mali." The pathology of these cases usually present hepatic enlargement, the gall-bladder filled with dark bile; in other respects the phenomena are similar to those of fatal cases of strumous peritoneal and mesenteric disease, occasionally pleuritic and meningeal effusion hastening the fatal issue.

As regards the treatment of these cases, our first efforts should be directed to dislodge the offending mass, and for this purpose I think enemata are peculiarly suitable; calomel or gray powder being simultaneously exhibited. Dr. Corrigan, who has recently contributed some most instructive cases on pica, recommends calomel in repeated doses, followed by castor oil and turpentine. Once the offending matters are removed, our aim should be directed to improve the character of the secretions generally, and for this purpose the alterative powders I have above mentioned, with the continued use of tepid baths, will be found most efficacious. To control the diarrhœa, I prefer the steady exhibition of the pulv. cret. opiatus; and in the convalescent stage the syrupus ferri iodidi, vinum ferri, or other chalybeate as indicated. Of course, the most careful watching, as regards diet, and to prevent the vicious propensity being indulged in, is indispensable.—*Dublin Hospital Gazette, Feb.* 1, 1861. *p.* 33.

140.—ON CEREBRAL DISEASE UNATTENDED BY CEREBRAL SYMPTOMS.

By Dr. CHARLES WEST, Physician to the Hospital for Sick Children.

[In phthisical or tubercular subjects, there is much risk of overlooking the advances of cerebral mischief. It is by no means always that the pain points to the seat of mischief.]

Solicitude has perhaps existed for some time with reference to the child's health; slight cough is present, and with it there is some loss of flesh. Presently the cough increases a little, and the parents attribute this to the child somehow having caught fresh cold. There is no marked head-affection, the child indeed is heavy and dull, but makes no special complaint of headache, or says at the most that his head hurts him when he coughs. The bowels are often somewhat constipated, but probably a disposition to constipation is habitual, while if vomiting occurs once or twice it is referred to the expectorants prescribed for the cough, and ceases either spontaneously, or as the result of some change in treatment. But still the patient is no better; the pulse continues rapid and feeble, the heaviness and dulness deepen into a constant drowsiness, the drowsiness into a stupor, and the stupor issues in coma which may or may not be preceded by convulsion; the riddle of diagnosis being solved by the evident head-symptoms which accompany the last few days or hours of the patient's life.

A girl, seven years and nine months old, who inherited a phthisical tendency from her maternal ancestors, was attacked by hooping-cough four months before she came under my notice. In the course of a week measles supervened which were complicated with severe diarrhœa; while the hooping-cough lasted, though with no great severity, for four months. A month after its cessation the child, who still had some cough, was admitted into the Children's Hospital. She was very weak, much emaciated, had various dyspeptic symptoms, and presented all the characters of advanced phthisis, while there were evidences of tubercular deposit in the left lung. Her pulse

was a hundred in frequency and very feeble; her tongue was much coated, her lips were dry, and she resembled not a little a patient with typhoid fever; between which disease and acute tuberculosis, I may just remind you in passing, the diagnosis is often by no means easy. She slept well the night after her admission, and made no complaint of headache, but in the morning she was sick once, a symptom which had occurred before, but which at no time was frequent or troublesome. On the following day she made slight complaint of headache, but her face was tranquil, and though not sleeping much by day she was always quiet. Her pupils on this day were observed to be rather more dilated and rather more sluggish than they ought to have been. Sickness had not returned, nor did it; but the complete distaste for all food and drink was very remarkable. On the fourth day all headache had disappeared, but drowsiness seemed creeping on; it deepened into coma on the sixth day; and, undisturbed, save by very slight twitching of the muscles of the face, on the eighth day the child died.

The emaciation was extreme, and there was abundant tubercular deposit in almost all the organs of the body, especially in the bronchial glands and spleen. There were traces of old mischief at the base of the brain in the form of loss of transparency of the membranes, and some deposit of tubercular granulations along the fissure of Sylvius. The ventricles did not contain above six drachms of fluid, and there was no softening of the central parts of the brain, while the congestion of the arachnoid was not considerable. In short, the post-mortem appearances harmonized with the symptoms during life in illustrating the fact that whenever the tuberculous cachexia is much developed, a very small amount of cerebral disease is quite sufficient to destroy the patient; and the slight symptoms during life will be found to be the expression of a very small amount of local mischief.

The younger the child, too, the smaller will usually be the amount, both of general and local disease that will suffice for its destruction. There are also certain epochs of infantile life at which unusual demands are made on constitutional vigour, and at which, therefore, more than ordinary watchfulness is called for, and symptoms have a more than ordinarily grave significance; such are the epochs of weaning, and of dentition; and one of the reasons for somewhat protracted suckling of a weakly infant is to prevent the coincidence of the irritation of teething with the constitutional disorder that too often accompanies the first attempt at the substitution of artificial food for the mother's or nurse's milk.

It is at this time that slight attacks of illness are apt to pass unnoticed, or to be referred for an easy solution of their meaning either to the change of diet, or to the irritation of teething. The child becomes fretful, sometimes feverish, generally restless at night, but still there are intervals of almost its wonted cheerfulness. There is no marked intolerance of light,—a symptom, by-the-bye, the existence of which it is not easy to ascertain in the infant; constipation is seldom obstinate, often does not exist at all, while loss of appetite and refusal of its usual food are often even more marked than actual vomiting. For days, sometimes even for weeks, these symptoms continue, affording nothing definite enough for treatment scarcely definite enough to call for grave solicitude. At length with no apparent cause, there sets in an increase of the fever, attended by considerable heat of head and frequent outcries. These last for a variable time, for two or three days, or less, or sometimes even only for a single night, and the next day brings quiet; the child is drowsy after its suffering of the day before, so it seems at least, and the drowsiness deepens, and then coma steals on so almost imperceptibly that the anxious mother clings to the confident expectation of her little one's recovery for days after the doctor has seen the certain issue of the disease in death. Now and then, too, the acute stage of excitement and pain and fever is altogether wanting, and the transition from mere discomfort, first to apparent ease, and then to drowsiness, then to sleep, and then from sleep to stupor, from stupor to death is so gentle, that not the friends only, but the doctor, too, are apt to be taken by surprise.

And how is such surprise to be avoided? Partly by bearing in mind that

the ill-marked febrile disorders—the fretfulness, the unrest, all the assemblage of indefinite symptoms which I have mentioned—is commonly indicative of the advance of tubercular deposit, or, at any rate, accompanies that state of health in which deposit of tubercle is most apt to take place. Next, by remembering that in the infant at the period of dentition, there is a special source of irritation of the nervous system which tends to render the brain most sensitive, and its functions most likely to become disordered. Lastly, that this being the case, any signs of constitutional disturbance not clearly traceable to some other cause are most likely due to disease of the brain, and that consequently, on every occasion, you are bound to disprove the existence of cerebral disease before you are at liberty to seek further for an explanation of the symptoms.

But it is not in comparatively acute cases only that you are liable to fall into error, owing to the absence of those pathognomonic symptoms, which usually you expect to meet with. The course of chronic disease may also be obscured in a similar manner, and this even when the appearances disclosed after death are such as to make it seem almost impossible that disease so grave should not have announced its presence most emphatically by symptoms of corresponding intensity.

Not many months ago a boy, ten years old, was admitted into the hospital who had been ailing for six weeks with occasional vomiting and a relaxed state of the bowels, which, for some days, had amounted to rather severe diarrhœa. He had lost his appetite and grown thinner, and had rested ill at night, but pain referred to the back of the head, nearly constant but never intense, was the only symptom which appeared indicative of cerebral disease. After a continuance of these symptoms for six weeks, he was admitted into the hospital as much on account of his failing health as of any solicitude excited by the headache. He was perfectly intelligent, and distinctly referred to the occiput as the seat of pain; but there was no tenderness on pressure there, or about the upper part of the cervical spine. A sluggish condition of the pupils, which were more dilated than natural, and slight divergence of the eyes existed, but neither of them to such an extent as at all to have forced itself upon observation.

He survived his admission sixteen days. His appetite improved; for a few days his headache considerably lessened, though it did not altogether cease, but then again returned, and was referred alternately to the occiput and forehead. His improvement occurred while taking small doses of the iodide of potassium, but I do not imagine that any considerable importance is to be attributed to it; and a blister was applied to the nape of the neck, on account of some increased headache two days before he died. Death took place unexpectedly, and so gently, that the attitude of quiet slumber in which the boy was seen more than once during the night was undisturbed.

A large mass of yellow tubercle, a little irregular, but about two inches in diameter, was imbedded in the left hemisphere of the cerebellum, pressing on and distinctly flattening the medulla oblongata. There was no change in the brain-substance in the neighborhood of the tubercle, or elsewhere, but the ventricles were much enlarged, their lining membrane was tough, and they contained eight ounces of perfectly transparent fluid.

It is worthy of note, that with the exception of slight old tubercular deposit at the apex of the right lung, the body presented no other trace of it. This is of importance, inasmuch as it shows that extensive disease in the brain may exist, independent of those general evidences of tuberculosis which often serve as a clue to the understanding of symptoms otherwise obscure.

In this case one symptom, and one only, was present to fix the attention on the probable existence of disease of the brain, and that was pain in the head. It was not intense pain, it was not sudden in its onset; brief in its stay, complete in its disappearance, relieved by attention to the state of the bowels, or even ceasing of its own accord, as did those neuralgic headaches which I spoke about at the last lecture, but it was constant, varying indeed, now better, now worse, but never entirely absent.

Be the symptom of cerebral disease, then, what it may, the mere fact of its persistence attaches to it an importance which it is almost impossible to overstate, and which far exceeds what would belong to it, even though tenfold more characteristic when present, if that presence were occasional and interrupted by intervals, during which it ceased completely. Of no symptom is this more emphatically true than of pain as a sign of cerebral disease.

In the great majority of instances the existence of any considerable deposit of tubercle in the brain produces not merely pain, but also impairment of power over some of the voluntary muscles, showing itself in paralysis, or in convulsive movements, or in both. It is unusual for the latter to precede the pain, and still more unusual for them to continue for any considerable time without becoming associated with pain and with other unmistakeable evidences of cerebral disorder.

Such exceptional cases do, however, now and then occur. A little girl, one of five children of healthy parents, in whose family no phthisical predisposition existed, had had uninterruptedly good health, not disturbed even by the process of teething. While at breakfast, when two years and a-half old, and apparently in perfect health, her mother noticed a tremulous movement of the right hand upon the wrist. In fourteen days the movement extended to the right arm also, and in another month to the right leg. At two years and nine months she came under my notice. At that time the leg and arm trembled in an almost equal degree, and the foot was drawn inwards. The child could stand, but could not walk; she could raise her hand to her head, could hold anything very light, but nothing of weight, in the hand. The tremblings ceased when in profound sleep, but the slightest irritation of the surface reproduced them without waking her. For about five weeks before I saw her, her head had been constantly drawn towards the left shoulder, though it became quite straight when she was asleep; and she had some power to elevate it, though she was not able to keep it raised. When these symptoms first came on she seemed somewhat out of health, lost flesh, slept badly, and her appetite failed; but for a month previous to her admission she had seemed well in all respects with the exception of the involuntary twitching.

The child was perfectly merry all day long, and some degree of constipation was the only symptom of disorder of her health. Aperients were given to her, but no other medicine. In five days there was marked diminution of the wry neck, and in nine the twitching of the arm and leg was also lessened. By degrees she acquired the power of standing firmly with very slight support, though, as often happens with children suffering from chorea, any excitement, or the consciouness of being watched, sufficed to bring back all the tremulousness almost in as great a degree as when admitted into the hospital. Simultaneously with this improvement, however, it was observed that the mouth was drawn somewhat to the left side; but this symptom gradually abated, and finally disappeared in the course of the next month. She became able to walk firmly, and to grasp objects well in her hand; and she left the hospital fourteen weeks after her admission with no other ailment than slight tremulousness of the hand and arm, and somewhat impaired power over the right leg. For five weeks only this improvement lasted, but at the end of that time she began to walk less well, gradually grew heavy and drowsy, and sank, at the end of ten days, into a semi-comatose condition, in which both pupils were dilated, but the right especially so. Convulsions then came on, accompanied by rigidity of the left limbs, while the right were in violent movement. Slighter convulsions followed, in which both sides of the body were equally disturbed; but from the fourth to the sixth day there was a partial return of consciousness, which once more gradually disappeared. The right arm became permanently rigid, while automatic movements took place of the left arm, and of both legs. On the seventh day deglutition became difficult; on the ninth impossible; and on the tenth day from the first occurrence of coma death took place.

The cerebral convolutions were flattened, the arachnoid on the convexity of the brain was granular, and much fluid escaped on removing the brain.

There was abundant effusion of blood beneath the arachnoid at the base of the brain, covering both surfaces of the medulla oblongata, reaching as far forward as the optic commissure, and downwards, though slightly, on the posterior surface of the cord, as far as the fourth cervical vertebra. There was a softened tubercle of the size of a pea at the posterior part of the right hemisphere of the cerebellum, and there were five masses of tubercle in the substance of the left optic thalamus, but no softening nor other appreciable change in the cerebral substance about the deposit. Tubercle, but to no considerable extent, was present in the lungs and bronchial glands.

In this instance, irregular, almost choreic, movements were for some months the only evidence of the grave cerebral disease which, advancing all the time, at length destroyed the patient. But even they might serve to excite suspicion. They were all the time limited to one side of the body, while chorea seldom continues so restricted, though one side only may be at first attacked. The drawing of the head towards the left side, and afterwards the paralysis, even though temporary, of the right portio dura, pointed to some abiding irritation of the brain, and served to raise the suspicion that the symptoms were of a graver import than at first seemed likely, if one looked merely to the apparently good health and the perfect happiness of the little child.—*Med. Times and Gazette, April* 20, 1861, *p.* 411.

MISCELLANEOUS SUBJECTS.

141.—ON THE USE OF ALCOHOLIC STIMULANTS IN HOSPITAL PRACTICE.

By Dr. W. T. Gairdner, Physician to the Royal Infirmary, Edinburgh.

[Dr. Gairdner seems to have taken an accurate account of the quantity of alcohol which in each case he prescribed. Before adding up the whole, he suspected that at any rate he had not been increasing the quantity, but to his surprise he found that there had been a nearly uniform increase, especially in the female wards. We think that Dr. Gairdner's view of the use of alcohol is the true one, and opposed to what is commonly understood of Dr. Todd's views. Wines and alcohol "are *stimulants* and *tonics*, *i.e.*, *medicines*, and not *food*, properly so called;" and to administer them in the early periods of acute disease requires the greatest caution. Dr. Gairdner says]

My own habitual practice has been to give stimulants, if at all, only in very moderate quantities along with the food, and, in general, as an aid to the digestion of food,—the only exceptions being in the case of persons largely and habitually dependent upon stimulants from old and formed habits, and in a comparatively small number of acute cases for a very few days, sometimes only for a few hours, to help the system over a dangerous crisis, or to co-operate with other needful remedies, such as antimonials in pulmonary inflammation. I feel quite assured that there have been no such facts in my experience of alcoholic stimulants as are mentioned in Dr. Todd's last volume—*e.g.*. brandy, at the rate of six drachms every hour, given to a girl of 17 years of age in rheumatic inflammation (case of Jane Cook); or, in another case (Sarah Butcher, age and habits not stated), a pint a day of brandy for a month together in pyæmic inflammation. Such facts are, I cannot help thinking, the indications of great excess, if not of an entirely wrong direction, in the use of these powerful remedies; which, to be powerful for good and not for evil, must be maintained strictly within the limits of their medicinal action, and given, not *as being food* in themselves, but rather as *adjuvants to food—i.e.*, as aids to the gastric digestion, and stimulants of the nervous system and circulation. I quite agree, however, with Dr. Todd in thinking that when stimulants really act beneficially in acute disease, they diminish the frequency of the pulse and restrain the tendency to delirium, while improving the appetite and producing an amendment in all the general symptoms. On the other hand, I cannot but demur to his inference that delirium and other bad symptoms, even if increased under small doses, are to be kept down by giving much larger quantities. To regard flushing of the face and increased feverishness, for example, as not a contra-indication, but a reason for increased administration (Lecture viii., p. 269), is opposed entirely to the practice I have followed; for under such circumstances, or even when stimulants have not been obviously followed by reasonably good effects after a cautious trial of small quantities, I have nearly always abandoned them at once as being unsuitable remedies, at least for the time; and, accordingly, it has never occurred to me to have to "sluice the head well with cold water," or to use any of the other means recommended in Dr. Todd's fourteenth lecture, in order to distinguish "the coma of alcohol" from "the coma of disease" in cases of accidental over stimulation.

Let me add, that another opinion prominently put forward in Dr. Todd's book appears to me to demand qualifications, viz., that "it is far more dan-

gerous to life to diminish or withdraw alcohol than to give too much." So far from having had constantly before me the fear of sacrificing life by diminishing or withdrawing a habitual allowance of stimulants, I have made it part of my regular practice to do so in most cases of persons accustomed to the use of ardent spirits in excess, and especially in many cases of delirium tremens, or of other acute diseases modified by alcoholic excesses; and though not committed to the treatment of any disease entirely without stimulants, I can entirely corroborate the remarkable statements made by Dr. Peddie in his very important memoir on delirium tremens, viz., that the suppression of the habitual allowance is not, *per se*, dangerous in most cases; but on the contrary, extremely conducive to the cure. In regard to other diseases, I believe that much more mischief is done by the routine administration of stimulants than could possibly result even from their entire suppression in hospital practice. In the case of young persons affected with fever, indeed, it has been almost made matter of demonstration by statistical data, that the stimulating practice pursued on Dr. Todd's plan, was the opposite of useful in the saving of life. Nor can it be doubted that in pneumonia, as well as in fever, Dr. Todd's practice as recorded by himself, was less successful than that of many practitioners who are less liberal in the administration of alcoholic stimulants. It is quite true that comparisons cannot always, or indeed often, be drawn with accuracy from limited numbers of cases in different spheres of observation; nevertheless, I feel well assured that the recorded mortality from the cases of pneumonia or fever occurring under Dr. Todd's care in King's College Hospital, would be regarded as excessive in the Edinburgh Royal Infirmary. On this subject it is sufficient for the present purpose to refer to the "Five Years' Hospital Experience of Pneumonia," published in the Edinburgh Medical Journal' for March 1860; and to the 'Clinical Notes on Fever,' in the same Journal for July 1859, although neither of these papers were compiled with a view to the present inquiry: nor would it be fair to Dr. Todd to assume that his cases were parallel to those referred to in these papers.

Average Daily Consumption of Alcoholic Stimulants per Patient during Five successive Years, in the Royal Infirmary, Wards 4, 15, *and* 16.

	1856.	1857.	1858.	1859.	1860.
GENERAL WARD, Males—					
Wines (ounces).....	0.158	0.465	0.710	0.928	0.739
Spirits (ounces).....	0.056	0.312	0.287	0.184	0.454
Malt liquors (pints).....	0.039	0.040	0.025	0.053	0.058
GENERAL WARD, Females—					
Wines (ounces).....	0.446	0.534	0.799	1.498	1.200
Spirits (ounces).....	0.295	0.312	0.223	0.164	0.510
Malt liquors (pints).....	0.064	0.069	0.048	0.061	0.048
FEVER WARD, Females—					
Wines (ounces).....	0.715	1.256	1.734	1.725	1.140
Spirits (ounces).....	0.069	0.083	0.346	0.052	0.135
Malt liquors (pints).....	0.023	0.029	0.135	0.069	0.027

I may, in conclusion, be permitted to refer to the 'Edinburgh Medical Journal' for February 1858. The review of the arguments at p. 735, *et seq.*, in regard to the medical uses of alcohol, is so expressive of the views I have all along held on this subject, that I am well satisfied now simply to lay claim to the authorship of this anonymous paper as evidence that the opinions now put forward had been carefully considered long before this paper

was thought of. The recent progress in the physiology of the subject, and especially the researches of Lallemand, Perrin, and Puroy in France, and of Dr. Smith of London, appear to me to be in general confirmatory of the ideas expressed in that review; although, no doubt, some of the questions in controversy may still for a long time remain open to discussion on points of detail.

The object of this paper will be served if it shall be the means of procuring more accurate records than hitherto of the actual expenditure of alcoholic stimulants in hospital practice. Considering the vast moral issues involved in this question, and considering also the important economic interest which the governors of our public charities have in keeping within reasonable bounds the administration of stimulants, it is surely not too much to suggest that in every hospital in this country monthly returns should be made, exhibiting, as in the Edinburgh Royal Infirmary, the aggregate expenditure in each ward, and also a calculated average of the amount supplied to each individual patient. By such averages, physicians would be insensibly guided to the truth; and the results of various practice would, when carefully compared, supply data hitherto wanting for the settlement of a great many scientific questions connected with alcoholic stimulants.—*Edinburgh Medical Journal, May* 1861, *p.* 961.

142.—ON THE ACTION AND USES OF ALCOHOL.

By Dr. Daniel Hooper, B.A.

How do alcoholic liquors act on the human body—as foods or as poisons?—and is their regular and moderate use beneficial or injurious? These are questions to which our most eminent physicians and physiologists give contradictory answers, and the intelligent and reading classes appeal to them in justification of their habits of indulgence or abstinence; they take or avoid alcoholic liquors systematically, and on principle, and the practice in either case is based upon a scientific truth or a scientific error. It is therefore of the highest importance, both for the sake of the public health and of science, that those to whom we naturally look for guidance in these matters should come to some definite and determined conclusions on the alcohol question. What can be more deplorable than the fact that the medical profession is divided into two distinct schools on this subject—the one represented by Dr. Carpenter, and the other by Dr. Todd, Dr. Chambers, &c.? By one school alcoholic drinks are regarded as *food*, by the other as *poison;* by one as a regular, daily, and useful, if not necessary, part of man's sustenance; by the other as powerful medicinal or poisonous agents, only useful, like those of the apothecary, under certain rare and peculiar circumstances. One school would place them in the same category as salt, sugar, butter, and tea, as articles of ordinary domestic consumption; the other would range them on shelves, in company with laudanum, sal volatile, and chloroform. These facts prove that a deep-rooted fundamental creed as to the action of alcohol upon the human tissues lies at the bottom of, and gives its peculiar tinge or complexion to each of these schools; and until this action of alcohol upon the tissues and vital functions is finally settled and agreed upon, it is impossible that the teaching of men of science and the practice of the public can be, as in the case of meat and bread, unanimous and satisfactory.

It is extremely difficult to determine, with accuracy and precision, the mode in which any agent affects the bodily tissues and functions, but experience and experiment, induction and deduction, chemistry and physiology, enable us, in many cases, to arrive at the truth, or an approximation to it, and, consequently, there is at the present day a tolerably constant and unanimous belief respecting the *modus operandi* of a considerable number of dietetical and medicinal agents, and it is much to be lamented that two totally opposite and contradictory beliefs should exist on a subject of such

vital importance to the public as that of the action of alcoholic liquors upon the human body. I do not pretend to be able to settle this great and difficult question, but I shall endeavour, by quotations from high authorities, as well as by observations and reasonings of my own, to exhibit fully and clearly its present state, and thus to render its solution easier and less distant.

Alcohol appears to seek out and fix upon *nervous* matter and to act directly and specially upon it, just as other agents localize themselves in particular organs. Dr. Todd regards alcohol as a *food*—Dr. Carpenter as a *poison*—to nervous matter; both, however, agree that it acts upon the nerve-cell and fibre *directly*, and upon the encephalon almost to the exclusion of the spinal cord. "So far as it influences the nervous system," says Dr. Todd, "the action of alcohol is that of a *stimulant*—an unfortunate term, indicating a distinction without a difference; other forms of food are likewise stimulant, but as they do not act directly and quickly upon the nervous system, their exciting properties are not so apparent. In like manner, alcohol possesses its stimulating property, because it is a form of aliment appropriate to the *direct nourishment* of the nervous system, and to its preservation; its special adaptation to this system gives it an immediate exciting power superior to any other kind of food." He further says, that alcohol, even when taken in excess, does not produce inflammation of any organ, but that its bad effects are shown in the nervous system; it damages the nutrition of the nervous matter, poisons the nerve-fibre and nerve-cell, and produces anæmia of the brain. Waste of nervous matter is indicated by tremblings and impaired mental power, and these symptoms may be caused by mental anxiety, fatigue, or sexual excesses, with or without the use of alcohol. Todd contends that the moderate and proper use of alcohol repairs and invigorates the nervous system; in short, he regards it as "*its appropriate pabulum*," or food; if given beyond what is required in the treatment of disease, he says it will be exhaled and perceptible in the breath; not so if the quantity be proportioned to the wants of the system. Dr. Marcet, in a book, entitled 'Chronic Alcoholism,' has very well described the effects of the habitual and *excessive* use of alcohol. The symptoms, he says, may appear during the indulgence, or long after the discontinuance of the bad habit, and are these: Headache, vertigo, unsteady gait, tremors, muscæ volitantes, ringing in the ears, deficiency of what the French call "*aplomb*," nervousness, sleeplessness, nightmares, frightful dreams, weakness in the loins, hips, and knees, inability for exercise, and weakness of intellect and memory. The same or similar symptoms may arise from other causes besides alcohol; such as mental anxiety, excessive intellectual toil, sexual excesses, inordinate smoking, &c., all which, in common with alcoholic excesses, exhaust or *waste* the nervous matter. Dr. Todd, as I said before, regards alcohol as the proper pabulum of the nervous, in the same sense as albumen is the appropriate pabulum of the muscular, tissue. Carpenter, on the other hand, endeavours to show that alcohol is essentially *de*structive, and not *con*structive, of nervous matter; he contends that it only stimulates the brain and nerves as the spur does the horse, and that this stimulation wastes and destroys it, so that in the end, and on the whole, it will be in a worse condition than before, and will require time and repose in order that it may be fed and repaired by blood containing what he considers to be its appropriate pabulum—viz., phosphorized fats, albumen, &c. If every act of the mind, every thought and emotion, wears away some portion of nervous matter, just as every muscular contraction destroys a particle of muscular fibre, it is evident this must, in both cases, be restored in *some* way. Now Carpenter contends that alcohol cannot, and does not, restore nervous matter, but that it only stimulates the wasted and jaded brain and nerves to further efforts—that is, in fact, it acts upon them as the whip or spur does upon the jaded horse, making them work at the expense of still further wear and tear; so that alcohol, on this view, is a sort of suicidal instrument to the nervous system, goading it on to its own destruction! What, then, can and does repair satisfactorily waste? Dr. Todd would answer, "Above all things, *alcohol*." Carpenter would reply, "Certainly *not alcohol*, but rest, sleep, and fresh blood containing the proper

pabulum of nervous matter—fats, albumen, &c." No doubt there are many agents which all the world admits *do* repair nervous waste; such are tea, coffee, fresh air, recreations, sleep, and good food, about which there exists no sort of doubt or question; but good reasons ought to be given for excluding alcohol from the category. Now, of tea, coffee, exercise, study, sleep, &c., we may affirm, that used within certain limits, they stimulate, strengthen, nourish, and repair the nervous tissue; and that beyond those limits, they weaken, depress, and waste it. May this not be asserted, also, of alcohol? Dr. Carpenter argues that alcohol cannot ultimately benefit nervous matter, because it is incapable of regenerating it—i.e., of becoming its material pabulum, or food; but no one questions the benefit of tea, coffee, moderate study, sleep, and recreations; and yet we have no reason to suppose that they, *per se* and directly, contribute the most minute particle of matter to the brain and nerves.

In studying the physiological action of alcohol upon the human body, we must never forget that it is one of that large class of agents whose influence varies, not simply in amount, but in kind or quality, according to the quantity administered; so that the effects of a large dose will be, not a mere *multiple* of those of a small one, but of a totally different character. In some few cases, as those of lying or stealing for instance, quantitative difference does not produce qualitative difference; but in the majority of cases it does. A certain temperature produces ice—a higher one, steam; a certain weight bends a spring—a heavier one breaks it; a short mountain walk invigorates the body—a long one weakens it; a few hours' study may *in*nervate the brain—a few hours more will *e*nervate it. And may not, also, a certain amount of alcohol, tea, coffee, &c., strengthen the nervous system, and a larger one weaken it? Or is alcohol mischievous in *all* proportions, whilst tea, coffee, study, &c., are not so? Cause must be shown why alcohol is to be excluded from the class of agents which do good in moderation, and harm in excess.

Is alcohol a *food* or a *poison?* This question is still an open one; the end and aim of all food is *force:* food is finally converted into force, which may be regarded as its true definition, and almost as its equivalent, convertible, and equipollent term. In this sense, alcohol, tea, coffee, study, exercise, oxygen, may all be regarded as food, for they all give force, although they probably do not, directly and *per se*, furnish any material pabulum to the brain and nerves. Alcohol, however, *may* possibly do so, (as we shall see in another place,) and may, therefore, be regarded as a more real food than the others. Moderate exercise of mind and body is a generator of force, and the indirect means of imparting growth and strength to the brain and muscles. But alcohol is more really and strictly a food than any such agent as exercise; for all material food is either plastic (tissue making) or respiratory (heat-making), and alcohol is a most excellent respiratory or calorific food, for it is far more digestible, and far quicker in its action than starch, fats, and sugar, and is at once absorbed by the vessels on the walls of the stomach; consequently, where time is an object, as in cases of fainting, or of collapse from accidents, alcohol possesses a manifest advantage over the more solid and slowly-acting hydro-carbons. Even Carpenter admits the rapidity of its action; but he objects to its employment except in cases of emergency. He says—"Alcohol is the quickest in its action of all the hydro-carbons, but others would be equally and more permanently efficacious if only time were given them to act. In some exceptional cases this *time* cannot be given, and then alcohol is indicated." He also says, "Alcohol, by presenting itself first for combustion in the lungs, prevents the other carbonaceous matter of the blood (supplied from food and other sources) from being burnt off the lungs; these, consequently, are thrown upon the skin, liver, and kidneys, which organs are very likely to suffer in the performance of this extra duty." All writers agree with Moleschott, that "wine saves the tissues from being burnt, by offering itself as fuel;" and the most recent experiments of the eminent physiological chemists of Germany have completely established this truth—that alcohol (in common with tea and some

other agents), by preventing the waste of the tissues is, if not a real and material pabulum, at least an equivalent to it, a diminution of expenditure being, of course, tantamount to an increase of income. It is objected that alcohol is only a *temporary* stimulus; that the force generated by it is only *temporary*. But this is not a valid objection, since *all* stimulus, *all* force is temporary; food, fresh air, exercise, are all stimuli, or generators of force, but are temporary in their action. Life is only possible under incessant stimulus. Tea and coffee are called "agreeable and refreshing" stimuli; why should the stimulus of alcohol be called noxious? What is there *peculiar* in the alcoholic stimulus that demarcates it from all the others? If a moderate quantity of tea be taken, the effect is agreeable and refreshing; but if taken in excess the effect is disagreeable and enervating. We may say the same precisely of a mountain walk; neither of these stimuli leaves the body in a worse condition (but, on the contrary, in a better) at the expiration of five or six hours than it would have been without it. Dr. Chambers applies this test (which seems to me a very fair one) to alcohol, and contends that if an individual finds himself better able to perform all the duties of life during the five or six hours' interval of his meals with than without alcoholic stimulants, then they are *good for him;* at the end of this interval, if alcoholic liquors agree with him, he will feel more cheerful and vigorous than he would have felt if he had not taken them. In the case of poisons, or *alcoholic excesses*, this average interval of five or six hours would be one of misery, and before its expiration the poison or the stimulus would demand imperatively either a remedy or a repetition.

All physiologists agree, that every mental effort wears away a portion of nervous matter, just as every muscular effort destroys and removes a particle of muscular fibre. This being so, the lost matter must, in both cases, be restored by the blood, unless a totally different law obtains in the two structures, which is improbable. Now, we know that the muscular tissue is repaired by the albuminous matters of the food existing in the blood. What part of the food, then, is it which, entering the blood, repairs, restores, or builds up the effete nervous matter? Phosphorus, oils, and fat constitute a large proportion of the nervous structures; and alcohol, being a hydro-carbon, is chemically allied to these components of nerve-cell and fibre. But what is its precise action upon them? Is it a real pabulum? Does it nourish them, directly and materially, and build them up in the same sense as albumen does the muscular tissue? Or does it merely affect them after the manner of study, or cheerful amusement, without imparting anything to them of a real and substantial character? Or, lastly, does it simply quicken the circulation, and so send a larger amount of blood to the brain and nerves in a given time? These and similar questions have yet to be answered before the subject of alcohol can be well understood.

My own observation and reflection have led me to believe that alcoholic drinks are highly useful, if not necessary, articles of regular daily consumption, for vast numbers of persons; but that their kind and amount must be determined by age, sex, constitution, mode of life, and other circumstances. I believe they are more necessary for those whose avocations involve head-work, anxiety, and wear and tear of brain, than for such as lead a comparatively animal life, or one of mere bodily labour. And I think it will be found that the degree of *refinement* of the alcoholic liquor required is in tolerably exact ratio to the expenditure of brain-power. The agricultural labourer, for example, is satisfied with ginger-beer, or very poor home-brewed beer; the working classes of London with porter; clerks and shopkeepers with bitter ale; and barristers, judges, and members of Parliament with wine. In fact, we find a gradation of brain-work corresponding pretty exactly to that of the refinement and alcoholic power of the liquor habitually and instinctively made use of. On the continent, also, we see illustrations of the same fact—the strength and refinement of the wines consumed gradually rising with the exaltation of the brain-work of the consumers. Nor is this owing, as might be supposed, entirely to difference of rank or pecuniary resources; for every man finds the same fact illustrated and cor-

roborated in his own experience. We all find, when on our tours in Switzerland or the Highlands, where we enjoy pure air, good food, and rest and recreation of brain; when, in short, we are living rather an animal than an intellectual life, we care nothing for, and do not require any sort of alcoholic liquor; whereas, when engaged in our professions or business in London, in the midst of bad air, noise, hurry, bustle, competition, and excitement, we are conscious of an unmistakable craving for a certain amount of alcohol with our daily food; the reason being that, in one case, we are doing everything to refresh and fortify, and in the other to exhaust and wear out the nervous system. This fact goes far to prove that alcohol, in some particular but as yet unexplained way, *does* repair nervous tissue.

In estimating the value of alcohol the experience and testimony of healthy persons who use it habitually, and in moderation, ought to be taken into account; also the fact that in all ages, and in every corner of the globe, man has discovered a method of preparing it. There are persons who do very well without alcohol; but this is no proof that it is useless to others. There are country districts where the labourers are healthy and strong without meat, and with beer almost as weak as water; but does it follow that the same fare would suit the London lawyer, barrister, judge, or member of Parliament? No, the two cases are totally different. Men whose labour resembles that of horses may and do live, like horses, upon corn and water; but those who are calculating, thinking, and reasoning twelve hours out of the twenty-four require a more refined sort of food and drink, A ploughboy will look fat and rosy upon his bread and cabbage and hard pudding and water; whilst a Gladstone will require, beside these, good animal food, tea, coffee, and an alcoholic liquor of great purity and refinement. If the brain work of a London clerk demands a supply of Bass's ale, that of the working statesman will require something approaching œnanthic æther!

Two arguments used by total abstainers require a short notice. They maintain that alcoholic liquors cannot afford any real and permanent benefit because they contain little or nothing of a *solid* nature (as proved by evaporation to dryness). But if this proves the worthlessness of wine, so does it of tea and coffee! The fact is, experience has proved that all these agents in spite of their unsubstantial nature, do refresh the wearied brain and nerves, and impart new life and health and spirits. Exercise, fresh air, recreations, study, tea, coffee, and cigar smoke, are all devoid of solidity; but the argument that they are *therefore* capable of imparting anything to the human body is still more so. On the contrary, we know that exercise does add bulk and weight and substance to the muscles; that fresh air does redden and enrich the blood; that recreation and study do nourish the brain and nerves; that tea and coffee and alcohol do, at any rate, prevent waste of the tissues (and probably also directly nourish the nervous system); and that moderate smoking, by soothing and calming the over-busy and excited brain, prevents its exhaustion and waste; in short, some of the least material agents have the most real, powerful and beneficial influence upon the human body. Again, teetotalers contend that in the case of alcohol it is impossible to define moderation and excess, since what is moderation to one man is excess to another, and *vice versâ*; but this is equally true of salt, sugar, tea, coffee, and many other things, moderation and excess in which they regard as tolerably well defined by common consent. The truth is, there is a certain recognised standard quantity of alcohol, salt, sugar, tea, coffee, &c., which all men agree to call *moderate*, and the difficulty is not greater than the case of alcohol than of any other article of daily consumption. The man who eats a leg of mutton at a meal, or consumes a pound of salt, or drinks a gallon of beer per diem, is looked upon by the public as a monstrosity, an exception, a wonder! whilst he whose daily consumption is one-sixteenth of these several articles is regarded as an ordinary individual—a type of the masses; in short, the excessive and the moderate man are as well known and as easily recognised as are any of the types and their deviations in the animal and vegetable world. It is idle and absurd to pretend that the boundary line between moderation and excess is indefinable. I believe every man knows where it is

and when he has overstepped it, even although from long habit and blunted sensibility the transgression may have little effect upon him. The soldiers' rations and the diet-lists of our great hospitals are so many proofs that *there is a standard* in these matters well understood, and that public institutions, in their dietetical arrangements, do not contemplate or provide for monsters who eat a leg of mutton, and drink three gallons of beer per diem!—*Lancet, May 25, 1861, p. 507.*

143.—ON VERATRUM VIRIDE.

By Dr. Otterson.

The veratrum viride is not the white or European hellebore, furnished by the mountainous districts of the continent; but is the American hellebore, and is found in swamps and low grounds from Maine to Georgia; it is on this account sometimes designated swamp hellebore. In the United States Pharmacopœia and Dispensatory it is called veratrum viride, and by that name it is now familiarly recognised.

This article has never been used in the regular practice to any extent, although Prof. Tully, of Yale College, New Haven, had been in the habit of prescribing it in his practice, and recommending it in his lectures many years ago. In the August number of the 'American Journal of the Medical Sciences,' 1835, Dr. Charles Osgood, of Providence R. I., published an account of his investigations and experiments with it, his attention having been called to it by Prof. Tully; still it received little or no attention from the profession at large, until the papers of Dr. Wm. C. Norwood, of Cokesbury, S. C., appeared about the year 1850.

That gentleman had been using it for several years, and his papers on the subject aroused the attention of the profession to what many practitioners, and myself among the number, considered a remedy of great importance as well as power. The best mode of prescribing the veratrum viride is in tincture; and as Dr. Norwood's preparation is now in the market, for the sake of uniformity, and while it is comparatively a new remedy, I should recommend that it be always prescribed. Its effect upon the system are generally prompt, certain, and of such power as not to be entirely void of danger. In its operation as well as in its nativity, the veratrum viride differs from the ver. alba., in that it does *not*, like the alb., produce catharsis, which is the principal operation of the latter; on the contrary, it is quite promptly emetic, and not cathartic at all. But its chief action, and that which gives to it its remedial value, is its wonderful control over the circulatory and nervous systems. It possesses in a pre-eminent degree the power of calming nervous excitement, and reducing the force and frequency of the heart's contractions.

It is indicated in all that class of sthenic diseases where prompt and persistent sedatives are required; and has the advantage over the lancet, in that its effects can be continued by slight repetitions, without reducing the vital powers, as would the same repetition of the lancet. It is preferable to digitalis, in that it is more prompt and certain, and has not yet been found to possess that cumulative property which constitutes one of the dangers of, and objections to, digitalis. It is preferable to antimony, in that it does not produce such violent enteritic irritation and griping diarrhœa, while as a sedative it is considered by many superior; and as an *expectorant*, fully equal to the antimony; its diaphoretic properties are about the same; in its local effects, it somewhat resembles this drug, as, when applied to the skin, it produces heat and irritation, though not such an eruption as will the antimony. The same may be said of its effects upon the alimentary canal. Hence it is contra-indicated when any gastric or enteritic irritation exists. I will now in as few words as possible, introduce this new friend to a place in the society of those old and long-tried ones, upon whose virtues we have been so long accustomed to rely. Nor have I any doubt that the addition will be found both pleasant and profitable.

I would recommend Norwood's tincture to be prescribed in preference to Tilden's, or any other fluid extracts, because I think it likely to be more uniform in strength, and hence in its effects.

It is adapted to all diseases requiring the action of the heart and arteries to be controlled, except, as above stated, those attended by gastro-enteritic irritation.

In the latter stages of disease where the rapid action of the heart is consequent upon the progress of the disease, it should not be employed; such as the latter stages of fevers, when there are nervous symptoms and excessive cardiac action, attended with much prostration.

I think the doses recommended by Dr. Norwood, and some others, too large to commence with, particularly by those unaccustomed to prescribing it, and would rather advise them to commence with half the quantity, carefully observing its effects until they become familiar with them, and their own experience will be their most reliable counsellor.

I have used it myself in pneumonia, pleuritis, acute rheumatism, inflammatory croup, iritis, puerperal peritonitis, and nervous palpitation of the heart, and have occasionally added a few drops to expectorant mixtures, where the cough was produced or attended by bronchial irritation, which seems to be extended to the minute vesicles and the expectoration difficult and albuminous in its character; and, I must say, I consider it a most excellent addition; in its administration I have seldom commenced a case with more than three to four drops, repeating it in two hours, and then adding by one drop at each repetition, or diminishing the quantities, and lengthening or shortening the interval as circumstances might demand. I have generally found a much less quantity to bring down the pulse as low as I required, than that mentioned by Norwood, and some others. When emesis is once produced, the pulse generally comes down sufficiently, the skin softens, or a profuse perspiration, breaks out, and the great point is attained. This, as before stated, is easily continued by the occasional repetition of the remedy in very much diminished doses, varying of course, as with all other medicines, according to the susceptibility of the patient. When emesis is once produced, I find two or three drops every hour or second hour control the symptoms which indicated its administration.

Reference has been made to the *power* of the veratrum viride. It sometimes occurs that its operation is active beyond anticipation, and excessive emesis and prostration, even it may be to an alarming degree, are produced. This may be immediately counteracted by the administration of stimulants; a little weak brandy and water with a very few drops of laudanum in it, or a few spoonsful of camph. julep with laudanum answer the purpose. I have never seen a case requiring carb. ammon., although it is advised. Sinapisms to the stomach are useful addenda to the treatment. In excessive emesis anodynes seem to be the antidote, and laudanum or syrup morphia are always at hand.

I have made this imperfect and disjointed sketch, as at first stated, in order to elicit the experience of such as may have prescribed this remedy, and with a view to bring it to the attention of those who have not. Not that I consider our pharmacopœias deficient in the number of remedies they present; by no means; and yet I think new remedies of undoubted virtues should find there a place; notwithstanding they may be new to the great majority of our members. I presume the administration of this is new. At the South it is in extensive use, and highly prized, and by those who pretend to prescribe vegetable remedies only; it is at once in combination, their lancet, their digitalis, and their antimony; with them it is *tria juncta in uno.*—*American Medical Monthly Review, March*, 1861, *p.* 185.

144.—ON THE GALVANO-CAUSTIC APPARATUS.

By Dr. Thomas James Walker, Birmingham.

The galvano-caustic apparatus is extensively used in Vienna for the removal of morbid growths, as well as for other purposes. Owing to the absence, during the greater part of my stay last year in the Austrian metropolis, of Dr. Adolph Zsigmondy, *Primararzt* in the Hospital, who is the great advocate of the galvano-caustic mode of operating, I saw less of it than I should otherwise have done.

In the skin wards for the treatment of lupus, I several times witnessed its employment as a caustic by Professor Hebra. To all the parts affected, the instrument was applied at a white heat; thus, in a case of lupus of the ear, the porcelain cauteriser was deliberately applied to the whole surface of that organ. During the operation, which was necessarily a slow one, the size of the instrument being about that of an ordinary lead-pencil, the patient, a nervous female, continued shrieking violently. In another case, almost cured, the small pointed platinum cauteriser was applied to the numerous points where new centres of the inveterate inflammation were appearing upon the healed parts. During this process, the patient, a boy, scarcely uttered a groan; but it is hard to say whether this depended on absence of suffering, or on the fortitude produced by the prospect of one or two *ten-kreutzer* pieces which the professor gave him for his good behaviour, when the operation was completed. Similar courage, or freedom from pain, was manifested by a young man in another case, where the cautery was applied round the edges of the several large patches of lupus non exedens, which existed about his body.

In Dr. Zsigmondy's practice, I had an opportunity of seeing the employment of this instrument, as a cautery to various ulcers of the leg. The galvano-caustic instrument in these cases, where the surface to be treated was large, did not avail for the completion of the operation; the aid of the ordinary actual cautery being called in, on account of the much larger heated surface which the iron heated in a charcoal fire affords, as compared with the small portion of platinum or porcelain which can be heated by the galvanic current. The first application of the glowing cautery to these parts appeared exquisitely painful, there being of course no particular virtue in the heat produced by electricity which should prevent its causing pain; but, in comparing the actual cautery as usually heated with that heated by the continuous voltaic current, as regards the pain caused by their application, we find that the latter, possessing in itself the source of heat, is not cooled by the tissues of the body, consequently it quickly and completely destroys their sensibility and vitality, producing only a momentary pain; while the former, however massive it may be made in order that it may retain its heat, is, on its first application to the tissues, so cooled by them, that it requires a longer application in order completely to destroy the structures with which it is in contact, and may in many places only half burn them, causing therefore pain of some duration. This I would advance as a general rule. To the eschar left where the instrument had been once applied in these cases of ulcer, it might immediately afterwards be reapplied without producing any pain. In pointing out what I conceive to be comparative merits of the galvanic cautery, I do not wish it to be supposed that I am advocating what appears to me an unnecessarily harsh and cruel treatment of a disease which does not require the use of the cautery at all. It may be that in gangrenous ulcers of the leg, and also in cases of hospital gangrene, many English surgeons would be inclined to adopt the treatment by the galvanic cautery; but for varicose and indolent ulcers, none of us, I think, would employ it.

In a case of large epulis growing from the outer side of the gum opposite the left wisdom tooth of the lower jaw, I saw the application of the cutting-noose (*Schneide-schlinge*), an instrument which consists essentially of a platinum-wire ligature heated to a white heat by the galvanic battery, and

capable of being gradually tightened by means of a screw. In this case, the wire noose being thrown round the tumour, and drawn so as to surround its base closely, the galvanic circle was closed, and the wire, becoming white hot, was slowly tightened; it gradually passed through the tissues until the tumour dropped into the mouth; to the part of the gum where it had been attached, the porcelain cautery was applied, and the operation was complete. As to the amount of pain caused by the removal of the growth, I am unable to form an opinion; for, although no chloroform was administered, the patient had fortified himself for the operation by drinking a quantity of wine sufficient to make him noisy and talkative. Very little coughing was caused by the smoke which the burning of the tissues produced in considerable quantity, the patient evidently respiring chiefly by the nostrils; the bleeding was very slight indeed, the patient spitting out once or twice only a little burnt blood and saliva. The wooden spatulas which were employed to protect the tongue and the sides of the mouth did not quite answer their purpose, as the tubes containing the platinum wire, becoming heated during the operation, blistered both the tongue and the cheek. In other cases which I saw, the mode of proceeding was so much the same, that I will not detail them.

The galvano-caustic mode of operating has been brought to so much higher perfection in Germany than here or in France, and of the plan pursued by Mr. Marshall and other advocates of the proceeding in England so little is generally known, that I shall describe at some length the instruments employed by Dr. Zsigmondy in Vienna, which are almost the same as those employed by Professor Middledorpf of Breslau, differing only in certain small points, which are, however, very important in their effect in improving the working of the apparatus. The essential part of the instrument is, of course, the galvanic battery; it is necessary that this be a constant one and very powerful. After many attempts to avail themselves of simpler and less expensive forms, the surgeons and instrument-makers have decided in favour of a Grove's battery as the most useful.

The communication between the battery and the instrument to be used must be made by means of wires sufficiently large to offer no opposition to the voltaic current, which must arrive in a full stream at the platinum wire which is to be heated; and these wires must at the same time be as light and flexible as possible, in order that they may interfere but little with the movements of the operator's hand. In Zsigmondy's apparatus, these conducting wires are formed thus. A bunch of capillary copper wires are carefully bound round with silk, so as to constitute a compound wire of about the twentieth of an inch in thickness; and from six to eight of these are again bound together to form a thicker wire. When the platinum wire of the instrument to be employed is very fine, so full a current is not required to heat it, and accordingly a lighter wire than that described may be used.

The instruments employed in these galvano-caustic operations are the various forms of cautery and the cutting noose, to most of which I have had occasion to allude. The various cauteries, as well as the cutting noose, are instruments in which a fine platinum wire, by being placed in communication with the two conducting wires, may be made the means of closing the galvanic circuit; these wires being made as good conductors as possible, and the far smaller platinum wire being a much worse conductor, the full stream of electricity meets here with such resistance as causes it to heat the wire to whiteness. The several varieties of cautery are formed on the same principle. They consist essentially of a wooden handle, through which run two copper wires, connected by their further extremities with the ends of the platinum wire of which the instrument is formed; while by the other extremities they may, by means of a screw, be made continuous with the conducting wires of the battery. The copper wires in the handle are at least four times as large as the platinum wire of which the cautery is composed. One of them is cut across, and its two portions can be easily connected or disconnected by pushing a slide on the handle upwards or downwards, with the thumb of the hand holding the instrument; so that the combining disk

being in its place and all ready for the operation, we still see the instrument cool and inactive, until the slide being pushed forward, the platinum wire begins to glow and rapidly rises to a white heat, at which it remains, when all the cells are in use and the wire is thin, even while in contact with the tissues to be acted upon. So intense is the heat that, unless the instrument be applied immediately to the tissues, so as to cool it somewhat, the wire will sometimes melt, high as is the melting-point of platinum.

The cauteries used are various, according to the platinum portion of the instrument. In the *Galvanokauter*, the platinum wire, one twenty-fifth of an inch thick, is flattened out into a form of blade with a beaked point; this is used as a cutting instrument, or its beak-shaped extremity is a fine cautery. In the porcelain cautery the platinum wire is spirally twisted round a small porcelain capsule, about the third of an inch in diameter; to this the heat developed in the wire is communicated, so that a much larger cauterising surface is obtained than could be got by the wire alone. Other forms of cautery for burning through urethral strictures, for passing down the lacrymal duct, for the destruction of the nerve-pulp of the tooth, &c., are also sold with the battery and other parts of this apparatus.

The cutting noose *(Schneideschlinge)* consists of a handle, from which projects forward a framework in which an ivory screw is fixed; round this the two ends of the platinum wire are wound, and when it is turned the wire is shortened. From the front of the frame pass forward two metallic tubes to contain the platinum wire; and these are isolated from one another by the ivory support; at the junction of these tubes with the frame between them and the handle are two screws, by which the platinum wire at this situation is made continuous with the conducting wires from the battery. The noose of platinum wire, which is usually about the fiftieth of an inch thick, being thrown round the part to be removed, the combining disk is placed on the battery, and the course of the galvanic force must be by the conducting wire to the screw at one side of the base of the double cannula; and, the communication of the wires below being prevented by a portion of ivory projecting round the middle of the screw mentioned, up the wire in the tube of the same side, round the loop, and down the other cannula to the screw at the other side of the base, and so back to the battery. Not only the projecting loop of platinum wire becomes heated, but also to a certain extent that within the tubes; and it is on this account necessary, in operating with the instrument at the bottom of the buccal, vaginal, or other cavities, to protect the neighbouring parts from contact with the cannula or with the fluids which flow from the seat of operation very much heated; for unless this be done they will be burned, as in the case of epulis above described.—*British Med. Journal, April* 20, 1861, *p.* 409.

145.—ON THE INFLUENCE OF OZONE ON ORGANIC SUBSTANCES.

By Dr. T. K. Hornidge.

[The great differences in ozone from ordinary oxygen are all referable to its more energetic oxidising power. It is possible, and indeed probable, that ozone plays an important part in the chemico-vital changes of the body. The great source of ozone is unquestionably the electrical action unceasingly going on in nature.]

The ordinary action of ozone is one of energetic oxidation. In spite of the small proportion of ozone present, iodine or chlorine is readily set free from its combinations; silver leaf is converted into oxide of silver; even mercury is superficially oxidised; the white oxide of lead is converted into the brown peroxide; and certain noxious gases, as sulphuretted hydrogen, are completely destroyed, as may be readily shown by suspending in ozone a paper black with sulphide of lead: it is rapidly bleached, by conversion into the sulphate. Ozone, therefore, is the great natural scavenger of the air. Hence it is always most abundant where there is least impurity in the air.

In mountainous regions, and by the sea, with a sea-wind, it is abundant; whereas near large towns, when the wind has passed over the town, it is wanting. Nevertheless, the relation of the presence and abundance of ozone to the presence of certain impurities in the air, especially in relation to the presence of the *materies morbi* of epidemics is enveloped hitherto in difficulties and uncertainty; due partly, I think, to the insufficiency of the ordinary test of ozone, which may be affected by many gases necessarily present in the air of large towns (as sulphurous or nitrous acids.)—*Med. Times and Gazette, April* 6, 1861, *p.* 357.

146.—FERRI CARBONAS EFFERVESCENS; A NEW AND ELEGANT FORM OF CHALYBEATE.

By Dr. Thomas Skinner, Obstetric Physician to the Liverpool Dispensaries.

Believing that the protocarbonate of iron, even in a solid state, is one of the best preparations we can administer in the thousand and one diseased conditions in which chalybeates prove useful, and that its present officinal preparations are incapable of preserving it from decomposition for any reasonable length of time, I have long thought it a desideratum to obtain it nascent, and in a soluble form, at the time of ingestion. I am now happy to state that after a great deal of trouble and experimenting, both by myself and by various experienced chemists, I have at last succeeded in obtaining the protocarbonate in the permanent and elegant form of an effervescing granular powder.

Formula and Process of Preparation.—The effervescing carbonate of iron is made as follows:

℞ Acidi tartarici ℥ iij; sodæ bicarbonatis ℥ v; ferri sulphatis ʒ x; pulveris sacchari ℥ j ʒ vj; acidi citrici ʒ ij.

1. Mix the sulphate of iron with the sugar and part of the tartaric acid. 2. Mix the citric acid with the remainder of the tartaric acid and the bicarbonate of soda. 3. Add the mixtures, and thoroughly incorporate them by sifting. 4. The whole is now to be thrown into a metallic pan set in a water bath; in a few minutes it will separate, when it should be rapidly stirred until granules are formed. If preferred, it may then be flavoured with oil of lemon; hitherto, however, the preparation has been without it.

Physical and other Properties.—When the above is carefully prepared, it has all the appearance of the popular and well known granular effervescent citrate of magnesia, with the addition of a slight yellowish green tint. Every drachm and a half contains ten grains of sulphate of iron, which, with a complement of bicarbonate of soda, is certain to produce, in a state of solution, four grains of nascent protocarbonate of iron. At the same time, there is developed a tartrate with a little citrate and sulphate of soda, which is, if anything, an advantage, as they act the part of a very gentle saline aperient, obviating the usual astringent effect of preparations of iron, as well as the too frequent constipation attending cases requiring chalybeates, particularly amongst females. The taste of it depends very much upon the amount of dilution. When taken in the dose and manner hereinafter recommended, the taste is that of a mild sparkling, and refreshing chalybeate.

After the effervescence subsides a perfectly clear light-green solution remains, which, if allowed to stand for some time, becomes of a deeper green colour on the surface, gradually increasing from above downwards, and floating like a cloud upon the upper stratum of the liquid. This appearance was at first mistaken for oxidation, but the more direct explanation seems to be that it is the carbonate of iron which was retained in solution by excess of carbonic acid gas; that, as the excess escapes from the surface, the carbonate separates from the solution in the form of a fine cloud, and becomes ultimately precipitated in the form of an impalpable powder. So far as permanency is concerned, the preparation has stood the test of several months, and it now remains as good as the day when it was made.

Doses, Uses, Administration, &c.—The dose is a teaspoonful, more or less (about a drachm or a drachm and a half), twice or thrice a day, in half a tumbler or more of water, an hour after, between meals, or upon an empty stomach, as is found most suitable. Dilution, within reasonable bounds, increases the tolerance of the remedy and favours its therapeutic action. It may be drank during the action of effervescence, but it seems to me preferable immediately after it subsides. When a prolonged course of iron is required this preparation will never take the place of the protosulphate, the iodide, or the sesqui-chloride; but where a moderate course of a few days, or a week or two, is necessary, it will be well borne by the stomach. It is not only well borne, but it seems to produce a much more manifest chalybeate effect within a given time and in a smaller dose, than any other preparation of iron with which I have had experience.

In facial or other forms of neuralgia, arising from anæmia or other cause relievable by iron, and particularly if the bowels are at all torpid, a few doses often act like a specific. It ought, however, to be continued after the pain disappears, so as to entirely remove the condition upon which the neuralgia depends. I may remark that the quantity of iron and the aperient effect might be doubled if required; the present proportions, however, have been found by experience to be the best for ordinary purposes.

The pharmaceutist who has prepared this effervescent chalybeate for me, and who deserves great credit for the perfection to which he has brought it, is Mr. Banner, chemist, 4, Hardman-street, Liverpool.

Let me add, in conclusion, that in prescribing chalybeates in general, particularly for those who are fastidious about the colour of their teeth, I am in the habit of ordering at the same time the following wash, to be used with a tooth-brush morning and evening, or after each dose:—

℞. Potassæ quadroxalatis 3 ss; aquæ rosæ ℥ vj. Solve.

After the use of this the mouth should be rinsed with cold or tepid water. —*British Med. Journal, June* 15, 1861, *p.* 627.

147.—ON THE CHRONOMETRY OF LIFE AND DISEASE.

By Arthur de Noe Walker, Esq., Paris.

That a child born in the seventh month is more likely to live than one born in the eighth, is assured as a fact throughout Europe. When I attended lectures, and heard this stated *ex cathedrâ*, the reason I adduced was, that a fœtus of seven months old, although immature, has at all events attained to the first stage of *relative* perfection; and that, therefore, the various forces and functions of its organism, although imperfect, are nevertheless, as a co-relative whole, in sufficient harmony with themselves to carry on the work required for further development. A child born in the eighth month is no longer in this *relatively* favourable condition; for after the seventh month a new series of progressive changes commence, perfected at the end of eight more hebdomads. Now, a child born while this new progressive stage is going on comes into the world in circumstances intrinsically less favourable than one born in the seventh month, because a child born in the eighth has passed out of a relatively harmonious development into an imperfectly co-ordinate relation of parts. That which is imperfect, but whose imperfections harmonize, is better than one who, although more advanced toward ultimate perfection, is called upon to contend with the external world with an inharmonious adaptation of parts.

Septenary periods are especially remarkable among the Ovipara. I believe many of them sit on their eggs for seven days, or during some multiple of seven; a question immediately connected with the chronometry of life, beautifully seen even in the vegetable kingdom, wherein wonderful order is obtained in the midst of excessive complication of parts and functions. In some individuals, for example, as noted by Mr. Paget and others, each stamen at different periods bows down to the ovary, impregnates it, and then

resumes its original posture, and makes way for another, whose organic development opportunely enables it to fulfil its destiny. It would be easy to ascertain if some particular part or parts of the chick are relatively perfected at the end of certain septenary periods. Human hair is apt to fall in greater quantity about the last septenary period of each lunar month, and to flourish again at the end of the second week, as I have more than once verified in my own person. Liebig states that wine in bottles undergoes a slight fermentation at the end of every month. It seems generally true that "to *every* thing there is a season, and a time to *every purpose*, under the sun;" and perhaps the chronometry of life is also subordinate to septenary or to other fixed periods.

The moral and physical phenomena observed in a healthy subject at the third septennial is particularly striking. Men at that age who are in the army and navy evince excessive daring and enterprise; while all assume a certain confident walk and tone, betokening intolerance of correction or contradiction in argument. We sometimes observe enormous energy displayed in furtherance of some favourite pursuit or chosen career. Advice, dogmatically given, is rejected with resentment; but sound counsel, offered in a proper and kind way, is gratefully received and seldom forgotten. This part of the subject reminds me of Burns's song—

"O for ane-and-twenty, Tam!"

I observe, however, that men at this age are apt to fail, if subject to great and prolonged privations and hardships; nevertheless, a regiment composed of equal proportions of young men and old soldiers is the best fighting material. At the end of the fourth septennial, a great change takes place. The mind sobers a good deal; the respiratory function is slower and less energetic, with a marked tendency to increase of bulk and adipose tissue. During the two following septennials a man is endowed with those qualities which render him most fitted to fulfil his individual responsibilities, and to give a permanent and useful character to his social position, inasmuch as he then possesses great mental and bodily strength, guided and governed by a mind free from the recklessnes of twenty-one.

The chronometry of disease, although easily studied, has not been sufficiently investigated. I have lately turned my attention to this subject, but my observations are as yet too incomplete to be trustworthy. That septenary periods are often remarkable in the course of acute disease, is known to many nurses of common experience. In small-pox and after vaccination the papulæ and vesicles are matured on the seventh day, and visibly so on the eighth—a day in which a new phase commences in all cases where the seventh completes an antecedent one. Has any one ever observed whether relapses are liable to occur at stated periods? If so, perhaps some analogy might be traced between diseases liable to relapse at the same fixed periods.

While changes on the seventh day, or some multiple of seven, are generally unfavourable, it is, I believe, equally true that we may observe hopeful symptoms of amendment at fixed periods, varying according to the nature of the morbid state. I have myself remarked, in some fatal cases of diphtheria a decided amelioration on the fifth day, increased tendency to death on the seventh, and, if the patient survived the first hebdomad, death ensued during or about the twenty-fourth hour of the fourteenth day after an antecedent amendment *perceptible* on the twelfth. I have never been able to trace any kind of septenary change after the twenty-first day.

Mr. Rice's remarks on the general question of sympathy are sound. Sympathy and antipathy stay society to an almost inconceivable extent, and, like instinct, are independent of reason, merit, or prejudice. But observation will prove the truth of Mr. Rice's assertion, that our antipathy or sympathy for a person is due to individual organisation, and has, therefore, like all other phenomena, a material basis. His remarks, also, on "reflex motion" commend themselves. Moral reflex action—a totally distinct thing from example—exercises immense influence over mankind, especially in civilized cities, or where large civilized masses consort together.

Moral reflex action is nothing more, however, than sympathy induced by more active means than is the case in what Mr. Rice calls "hidden sympathy," *apparently* quite passive. The former however, operates very powerfully, either for good or for evil on the great mass of the people; a fact, perhaps, so general, that it seems to have escaped the notice of most statesmen in Europe, save in Austria and Russia, where due attention to this subject is a great help to despotic governments.

With reference, however, to Mr. Rice's remarks, I will observe that the present Lord Radstock has propounded the following theory: He believes that when a public speaker or preacher, for example, influences his audience to an unusual and often unhealthy extent, and, as the saying is, "carries them with him" such sympathy, or moral reflex action, is due to the speaker's ability or power of exciting in each individual before him that particular part of the main nervous centre peculiarly excited in himself while addressing his audience; or, in other words, every person present has the same identical part of his brain aroused or excited. This explains why, as I have myself seen, a whole division of an army may be overtaken by a frightful panic, developed in the first instance in half a-dozen men, or in a single advanced sentry. In the same way we may explain why a single hysterical patient may propagate hysteria to the inmates of a whole ward. St. Vitus's dance also is propagated in the same way, by one person so diseased putting the affected part of his own nervous system *en rapport* with others; and thus it is, I believe, that a few persons have infected whole villages. I have observed, and I believe, verified this theory at "Revival" meetings; and if so, the material origin or medium of similar reflex actions is satisfactorily explained.

I have always thought the phenomena connected with panics very extraordinary. Troops that have faced death every day, and ready to do so oftener if required, will fly across the country "panic-struck." Monetary panics are not unfrequent, and "a run" on a savings bank sometimes takes place without any real or apparent cause. An "invasion panic" may be induced by the public press in forty-eight hours. Panics of a more reasonable nature are apt to pervade whole masses, from slight shocks of an earthquake, or from a sudden outbreak of an epidemic. Liability to panic-terrors being universally prevalent among all people, the ancient sacerdotal craft attributed the cause to a god, and assigned the attribute to universal Pan.

Sometimes, and in particular circumstances, the results of these reflexed influences are singularly permanent. I verify this in the person of a near relative of mine. A few years after her marriage, while out riding with her husband, his horse reared, fell over him, and killed him. Ten years after, being still a young woman, she married again, and bore children; but all the males, and in one case painfully so, unmistakably resemble her first husband. A renowned English physician, one of the most trustworthy observers that ever lived, known and quoted in all the schools in Europe, declared that convalescence in old men is much abbreviated, and the whole organism strengthened, by consorting, and even sleeping, with very young persons of the same sex.

The majority, of course, only smile at all this. Those whose only aim in life is to "get on in the world" declare these questions useless, *even if true;* while a smaller number, who have already "got on," don't care about it. There is, however, a very small minority who love their noble profession for its own sake and for the sake of others; and for them I close these remarks with a quotation from the works of the most learned man that ever sat in Parliament: "We would introduce into primary philosophy a real and solid inquiry into these transcendentals or adventitious conditions of beings, according to the laws of nature, *not of speech.*" (Bacon.)—*Medical Circular, July* 24, 1861, *p.* 65.

148.—ON INTERNAL OBSTRUCTIONS BY BANDS.

By JOHN GAY, Esq.

(Abstract of a paper read before the Medical Society of London).

The records of cases in which the abdomen has been cut into for the purpose of relieving internal obstruction, upon the presumption that the symptoms were due not only to a physical, but to a removable agent, show that the difficulty of distinguishing its exact nature and seat in each case has not been overcome. I need not refer to the recorded case of Manoury, Dupuytren, Monod, Hilton, Druitt, Avery, Fergusson, Erichsen, Recamier, Curling, Luke, Holt, and my own. In some of these the seat of stricture could not be discovered; in others the stricture was discovered to be occasioned by irremediable or irremovable disease; whilst in a third class the state of the parts was anticipated, but relief was too long deferred. It is true that in a few cases, such as those recorded by Fuschius, Caryoche, Dr. Wilson, Dr. Manlove, and Schacht, success followed these operations; but, on the whole, the best surgeons have deemed them speculative; and a lull in this species of abdominal surgery chides us for the neglect of those investigations which may render our knowledge more exact, and our treatment consequently more useful.

Of the various causes of internal obstruction which come under our notice I have chosen the "band" for present consideration. And here I must premise that by the term "band," I mean a structure distinct from ordinary adhesions, and having its origin, texture, and relations, characteristics peculiarly its own. As these bands are generally limited in each individual—seldom exceeding one, and never, as far as I have been able to discover, more than two.—I shall take the liberty of applying to it the generic term "solitary," in contradistinction to the filamentous bands that are often met with in the abdominal cavity, and which are only a variety of the common plastic exudations of its serous membrane. From these it differs, 1st. in size; 2ndly, in the fact that it is rarely if ever attached but at its extremities. The former are attached throughout their length to that which they embrace or connect —the latter *never*, their only attachments being *terminal*. 3rdly, in their having a distinct glistening surface, as though they possessed, in common with the abdominal viscera, a peritoneal investment. 4thly, in the fact that they are (I believe) frequently found to be *tubular, when they stretch from one hollow viscus to another*, and form a channel of communication between them. And 5thly, inasmuch as they have always some definite length. At the time of their formation, the parts connected by these "bands" appear to have been in *abnormal relation* to each other, whilst in simple plastic exudations the parts appear not to have been displaced.

Let us now look more narrowly into the ordinary conditions and relations of the "solitary" band. It has mostly *two* attachments—viz., *terminal*,—and sometimes, though rarely, it has but *one*, being free to float amongst the abdominal viscera.

The classification given in the former part of the paper shows that of the total number of bands, 27 were attached by one or both ends to the mesentery; that they may be formed upon and have their terminal attachments to that structure; but that when attached by one end only to it, their opposite extremities have either an intestinal attachment as was in the case in *twelve* instances; are attached to some contiguous organ, as in three instances: or it may be *free*, as in one. If we take another view, we shall find that when attached on one side to the bowel, the band becomes attached on the opposite besides the mesentery, either to another portion of the intestine, as in ten instances (including in these the vermiform appendage of the cœcum, which was the point of attachment in five cases)—or to a neighboring viscus, as in three—or to the parietal peritoneum, as in two.

The general inference from these facts is, I think, that whatever the terminal attachment of these bands, one attachment at least is always to some *one* of those abdominal structures of viscera, which are very prone to be-

come, at some period of life, the seat of serious and destructive diseases; and that, in all probability, these bands differ in their origin from those abnormal products with which they have been allied, inasmuch as they are the productions of the gravest disorders to which the abdominal viscera and their appendages are liable. And the view just enunciated becomes, I think, strengthened by an examination of the comparative liability of different abdominal structures to become the seat of bands, and the seat of destructive disease! A band of varying length—in some instances over 17 inches—may seem at first sight a very unlikely provision for the care of a wound or an ulcer through the intestine walls; but the improbability fades away before such facts as I am now about to lay before you.

1st. Traumatic injuries of the bowels are frequently remedied by the formation of a band; tying the bowels, at the seat of injury, to the tissue which happened coincidently to be in contact with it.

2nd. Morbid specimens abound in which the connection of a band with an intestinal cicatrix is shown.

These affections arise—

I. From impactions of hard bodies—such as gall-stones, plum-stones, &c. &c.—in the bowels or certain diverticula, especially in the vermiform appendage.

II. From the ordinary destructive disease of the abdomen,—mesenteric tabes, idiopathic typhlitis, typhus, catarrhal and dysenteric diarrhœa, &c.

III. From sloughing of portions of the intestinal cylinder, in consequence of injury done by external strangulation, as through hernia.

3rd. The retention of a viscus, in abnormal position, for a *longer or shorter* period (for it is impossible to assign any *proximately* fixed time for the formation of bands of this class), will give rise to the production of a band, in all probability, in consequence of direct or indirect injury during the period of dislocation. The most common instances of this kind of "band" are those in which a portion of perviated bowel becomes attached to the *sac*, in which it has been allowed to remain unreduced for some time. Such bands not unfrequently cause the reappearance of the hernia after repeated *reductions*, and might always be suspected when it assumes so *obstinate* a character.

But to proceed. *These bands*, it must be remembered, do not always constitute the whole of the immediately constricting agent, *bridle*, or *ring*; they may not constitute even any part of it, although that agent could not have existed as such without them. Thus a twisted and elongated *mesentery* may constrict through the adhesion of its *bowel* to some other structure. The omentum might also constitute the constricting agent through a like attachment, as in cases in which it has thus been tied to the edge of the femoral ring. Even a portion of intestine, or its diverticulum, might, in a similar manner, become the immediate *agent* in the act of constriction. In one case a band so attached a knuckle of intestine, that it (the knuckle) formed a ring, and strangulated another portion of intestine that happened to pass through it.

The evidences of the existence of a band may be divided into those derived from 1st, antecedent history; 2nd, mode of attack; sensatorial, 4th, physical, and, 5th, functional indications.

(Here follows an *analysis* of these several evidences, derived from a large number of collected cases.)

Perhaps this survey of the means of diagnosis is somewhat *discouraging*. It amounts to this:

1st. The history of the individual should point to some antecedent disease or injury equal to the production of a "solitary band."

2nd. Pain does *not of necessity* immediately follow on the constriction of a portion of intestine by a band, although a portion of higher intestine be constricted, and that tightly. If it *is* coincident with the *first* symptoms, it is *local*, and arises from a physical injury to the bowel; but if not, it comes on sooner or later, as the consequence of *over-distension*, or of *peritonitis* and its results, or of *both* combined. That it is not always, but *often paroxysmal*.

3rd. That distension is an early and prevailing sign. When the constriction involves the higher bowel, it is generally confined to a limited area around the seat of injury; when it is lower it is more *general* from the commencement. Dulness is a sign of uncertain value; but in general it is found gradually to concentrate itself, together with greater distension, towards and around the seat of constriction. Vomiting is a prevailing symptom, and especially characteristic of complete obstruction when it becomes fæcal.

4th. The suddenness of the attack, and its not being preceded or accompanied by any indications of any alteration in the prevailing state of the patient's health.

The practical conclusion at which I arrive from the foregoing remarks is this: viz., that our present means of diagnosing internal obstruction by *bands* are imperfect, and that, consequently, many cases will escape the most careful scrutiny to ascertain their exact cause. On the other hand, the inductions from the confessedly feeble analysis of the cases which I have collected together, and thus cursorily examined, encourage and even authorise the surgeon, in a case presenting a certain combination of features or evidences, rather to advise abdominal section than allow the patient to sink without the chance that such a procedure might afford.

1st. An *antecedent abdominal affection* of such *severity* as to lead the surgeon to believe that it might have been attended with some destructive process.

2nd. The attack occurring *suddenly*, without any previous visible deterioration of the patient's health.

3rd. *Pain first localised*, then tenderness over a large area.

4th. Distension, with, *first*, general dulness, and subsequently a concentration of the dulness and tension towards the original seat of pain.

5th. Vomiting, especially if it speedily becomes fæcal.

I believe no such conjunction of symptoms as these can arise without some *sudden* alteration in the relation of parts within the abdominal cavity; and that in such cases a *bridle* will be found to be, either directly or indirectly, the means of constricting a portion of intestine. As soon as the last of this series—viz., fæcal vomiting—has set in, the surgeon is justified in proceeding to explore the abdomen. Should a band or bridle be found, some caution must be observed in its treatment. It should be divided; but in case it unites two portions of a hollow viscus, there is a probability of its being tubular; consequently, the two ends should be twisted or tied to insure of their not allowing the escape of the visceral contents.—*Medical Circular*, *April* 3, 1861, *p.* 235.

149.—CRINOLINE ACCIDENTS.

By Dr. Odling. F.R.S., Guy's Hospital.

The various means proposed for rendering textile fabrics non-inflammable were carefully investigated a short time back by two well-known chemists, Messrs. Versmann and Oppenheim. An account of their experiments was read at the Aberdeen meeting of the British Association in 1859, and was afterwards published in the 'Journal of the Society of Arts,' and in a separate form by Trübner and Co., of Paternoster-row.

They showed that linen and cotton goods dried after immersion in a solution of one or other of several salts possessed the property of non-inflammability, and that the best results were obtained with a solution of sulphate of ammonia or of tungstate of soda, neither of which liquids produced any injurious effect upon the tissue or colour of the fabric. The tungstate of soda solution was found most applicable to laundry purposes, on account of its not interfering in any way with the process of ironing.

Muslins, &c., steeped in a 7 per cent. solution of sulphate of ammonia, or a 20 per cent. solution of tungstate of soda, and then dried, may be held in the flame of a candle or gas lamp without taking fire. That portion of the stuff in contact with the light becomes charred and destroyed, but it does

not inflame, and consequently the burning state does not spread to the rest of the material.

I am induced to send you these remarks through the mention of my name by Dr. Gull at the recent inquest on the death of Mrs. Brodhurst, as reported in the 'Times.'—*Times, July* 29, 1861.

150.—EXPERIMENTS RELATING TO THE DIURETIC ACTION OF COLCHICUM.

By Dr. WILLIAM A. HAMMOND, Professor of Anatomy and Physiology in the University of Maryland.

In the Proceedings of the Academy of Natural Sciences of Philadelphia for November, 1858, I gave the results of a series of investigations relative to the diuretic properties of digitalis, juniper, squill, and colchicum, by which it was shown that the latter alone possesses the power of increasing the amount of organic matter eliminated by the kidneys. From this circumstance the argument was adduced that this substance, of all those experimented with, was the only one that could be regarded as a true depurator of the blood.

The results obtained by earlier investigators cannot be regarded as satisfactory, owing to the faulty manner in which their analyses were made. The urine was concentrated by heat, and thus a large quantity of its organic matter underwent decomposition.

Since the publication of my experiments, Dr. Garrod of London has studied the physiological action of colchicum; but led away by his theory of the nature of gout, he limited his researches mainly to the termination of its influence over the excretion of uric acid, which, as is well known, forms but a small proportion of the total amount of organic matter excreted by the kidneys. As the result of his investigations, he announced that colchicum does not increase the quantity of uric acid contained in the urine, and that it is not by any action on the kidneys that the remedy in question exerts its curative influence in gout. His result, as relates to the uric acid, does not, so far as I know, conflict with mine, as I did not separately determine the quantity of this substance present; but his conclusion that colchicum is not a diuretic in the true sense of the term, is certainly not borne out by his own experiments, and is directly at variance with those which I performed.

It was, therefore, obviously necessary that additional investigations should be instituted, and I accordingly undertook the task of furnishing further contributions to the subject. Before proceeding to detail these, I desire to call attention to the valuable memoir of Prof. Austin Flint, in the number of this journal for November, 1860, entitled, 'Clinical Researches on the Action of Diuretic Remedies.' In this essay, in addition to much other valuable matter, the conclusion to which I have arrived relative to the action of colchicum is confirmed; Professor Flint finding it to produce a marked increase in the amount of solid matter eliminated by the kidneys, without, however, increasing the quantity of water of the urine.

The investigations to which the present paper relates consisted of experiments upon adult males, in a good condition of health. In all cases, the officinal tincture of the seeds of the *colchicum autumnale* was given.

The determinations made were the following:—1st, the quantity of urine; 2nd, its specific gravity; 3rd, the total amount of solid matter; 4th, the quantity of inorganic matter; 5th, the quantity of organic matter; 6th, the amount of uric acid.

The quantity of urine was determined in cubic centimetres.

The specific gravity was ascertained by means of the specific gravity bottle and a delicate balance.

The total amount of solid matter is given in grammes, and was determined in the following manner:—Ten cubic centimetres of the urine were evaporated to as complete dryness as possible *in vacuo* over sulphuric acid, and the residue accurately weighed. By simple proportion the amount of solids in the whole quantity of urine was easily ascertained.

Although it is impossible to get rid of all the water by this process, the quantity remaining is extremely small, and the results obtained are far more accurate than those obtained by evaporating to dryness in the water-bath, as generally practised. No matter how carefully this latter process is conducted, the loss of urea by decomposition is always an important item, and involves far more serious errors than the imperfect desiccation by the former process.

For the determination of the amount of organic and inorganic matter separately, the solid residue obtained as above was mixed with ten or fifteen drops of moderately strong nitric acid, and gently heated till the mass was well dried. The heat was then gradually raised till all the carbon was consumed, and the mass in consequence became white. It was then cooled *in vacuo* over sulphuric acid, and weighed. The inorganic matter was thus determined, and the loss showed the proportion of organic substance.

The quantity of uric acid was determined by adding chlorohydric acid to a known volume of urine.

The first experiments were instituted upon myself. In three days immediately preceding their commencement, the average quantity of urine for each day was 1425 cubic centimetres, of specific gravity 1021·73. The average amount of solid matter was 64·28 grammes; of which 30·18 were inorganic, and 34·10 organic substance. The average amount of uric acid excreted for each period of twenty-four hours was 0·77 gramme.

During the experiments with the colchicum, my manner of living was not materially altered from that of the three days above referred to; *i. e.*, I ate the same food and took the same amount of exercise, and endeavoured to make all the collateral circumstances the same, so as to ascertain as nearly as possible the exact effect produced by the colchicum.

First Day.—On this day I took one fluid drachm of the tincture three times—at 8 a. m., 2 p. m., and 10 p. m. The total quantity of urine excreted was 1685 cubic centimetres, of which the specific gravity was 1021·50. The total amount of solids was 70·15 grammes, of which 30·90 were represented by inorganic, and 39·25 by organic matter. The quantity of uric acid was 0·81 gramme.

Second Day.—One and a half fluid drachms of the tincture were taken, as on the previous day. Quantity of urine, 1720 cubic centi metres; specific gravity, 1020·87; total solids, 75·29 grammes; inorganic solids, 32·44 grammes; organic solids, 42·85 grammes; urio acid, 0·69 gramme.

Third Day.—Same quantity of colchicum taken as on previous day. Quantity of urine, 1784 cubic centimetres; specific gravity, 1022·57; total solids, 80·13 grammes; inorganic solids, 35·11 grammes; organic solids, 45 03 grammes; uric acid, 0.82 gramme.

Fourth Day.—On this day the quantity of colchicum was reduced to half a fluid drachm, taken as before. Quantity of urine, 1540 cubic centimetres; specific gravity, 1023·17; total solids, 69·23 grammes; inorganic solids, 31·09; organic solids, 38·14 grammes; uric acid, 0·78 gramme.

Fifth Day.—On this day the quantity of colchicum was increased to one and a half fluid drachms of the tincture before mentioned. Quantity of urine, 1698 cubic centimetres; specific gravity, 1023·68; total solids, 76·14 grammes; inorganic solids, 32·26 grammes; organic solids, 42·88 grammes; uric acid, 0·76 gramme. On this day there was some derangement of the general health, manifested by increased heat of skin, fever, and severe abdominal pains. There was also a little diarrhœa. The experiments were, therefore discontinued.

From an examination of the results obtained by the foregoing investigations, the effect of the colchicum upon the urinary excretion cannot fail to be perceived. The conclusions which I think may be formed are—1st, That the colchicum increases the quantity of urine. 2nd, That it increases the total amount of solid matter eliminated. 3rd, That this increase is mainly due to an augmentation of the organic matter. 4th, That the amount of uric acid does not appear to be affected.

These conclusions are rendered much more probable from the fact that on

the fourth day, when the quantity of the tincture of colchicum taken was reduced one-third, the effect upon the urine was less decidedly marked; and that when, on the fifth day, it was again augmented to a drachm and a half, the urinary excretion was materially increased in quantity, and the solids, the organic especially, remarkably raised in amount. The relation of cause and effect would therefore appear to exist; and accordingly it would be contrary to the principles of sound reasoning to assert that the change in the composition of the urine was accidental. It is doubtless true that the urine changes greatly from day to day, and even from hour to hour; but this fact is due to the other fact, that we are constantly varying our food, exercise, &c. When, however, as in the investigations cited in this paper, these circumstances are fixed, and only one difference exists between the ordinary mode of living and that practised during the continuance of the experiments, we are fairly justified in attributing any change in the urine or in any other excretion to the influence produced by that difference.

In the next series of experiments the effect is just as directly shown, though, for reasons beyond my control, they were not continued as long as were desirable.

The subject of these experiments was a young man 23 years of age, and weighing about 140 lbs. Before taking the colchicum I examined his urine whilst he was taking a fixed quantity of food and exercise, he being at the time an attendant in the hospital under my charge. As the results of these examinations for three consecutive days I obtained the following as the averages for each day:—Quantity of urine, 989 cubic centimetres; specific gravity, 1020·14; total solids, 51·20 grammes; inorganic solids, 22·45; organic solids, 28·75; uric acid, 0·47 gramme.

First Day.—On this day one drachm of the tincture of colchicum was taken three times. The effect upon the urine was as follows:—Quantity, 1021 cubic centimetres; specific gravity, 1024·18; total solids, 63·25 grammes; inorganic solids, 23·57; organic solids, 40·68 grammes; uric acid, 0·59 gramme.

Second Day.—One and a half drachms of the tincture were taken three times, as previously. Quantity of urine, 875 cubic centimetres; specific gravity, 1026·11; total solids, 60·25 grammes; inorganic solids, 20·38 grammes; organic solids, 39·87 grammes; uric acid, 0·51 gramme.

On this day diarrhœa was produced. This was quite of a severe character, and in consequence the colchicum was not further continued.

The remarkable effect of the colchicum in increasing the amount of organic matter excreted is, however, very decidedly shown. This increase is so great as to render the probability of its being accidental extremely small, and we cannot do otherwise than regard it as being directly due to the influence of the colchicum.

The details of the third case in which the colchicum was given have been unfortunately mislaid. I am, however, enabled to state with certainty that the same well-marked effect over the amount of organic matter excreted by the kidneys was exerted as in the cases the particulars of which have been given in full. The experiments were continued for six days with variable quantities of the tincture.

What are we to infer from these investigations? It appears to me that the conclusion must be admitted that colchicum is a true depurator of the blood, and hence we have an explanation of its good effects in those blood diseases, gout and rheumatism.

It is seen that no constant effect was produced upon the quantity of uric acid eliminated, and hence these experiments do not conflict with those of Dr. Garrod. We are not, however, bound to admit that the presence of uric acid in the blood in increased amount, during a paroxysm of gout or rheumatism, is the cause of that paroxysm; and, consequently, because colchicum does not increase the quantity of this substance found in the urine, we are not to suppose that the remedy in question does not exert its influence through the kidneys.—*American Medical Monthly.—Glasgow Medical Journal, July* 1861, *p.* 247.

151.—THE HOT BATH; ITS PHYSIOLOGY, USE, AND ABUSE.

ESPECIALLY IN CONNEXION WITH THE TREATMENT OF DROWNING.

By CHARLES HUNTER, Esq., late House Surgeon to St. George's Hospital.

"From these facts it appears that warmth causes a greater exertion of the living powers than cold, and that an animal in a weakly state may be obliged by it to exert a quantity of the action of life, sufficient to destroy the powers themselves, ... The first (artificial respiration) in many cases may succeed alone; but the second (heat) must, I think, always fail where the powers of life are considerably weakened."—JOHN HUNTER *on Proposals for the recovery of Persons apparently Drowned*, "Animal Economy," 2nd Edit., 4to, pp. 188, 189.

"Therefore in every case of vital suspension, the *primary object* is to institute *artificial respiration* till the *natural breathing* can be re-established. . . . . During this, if we attempt to raise the heat suddenly to the natural standard, we exhaust the feeble remains of life.

"The most efficacious method of restoring heat, then, is to renew the generating power by renewing respiration."—Dr. A. FOTHERGILL *on Drowning, in* "A new Inquiry into the Suspension of Vital Action," 1794, pp. 112, 184-5.

"On this account, and particularly as it is removing the cause of death, we cannot hesitate one moment in pronouncing the restoration of the action of the lungs to be of the very first importance to recover the apparently dead."—CHARLES KITE, "An Essay on the Recovery of the Apparently Dead," 1788 p. 138.

"Thus we have traced the connexion of the symptoms, and the consequent cessation of the heart, to the obstruction of respiration, as to their proper cause."—EDMUND GOODWIN, "On the Connexion of Life with Respiration, Submersion," &c., 1788, p. 98.

"Death from drowning is similar to death from strangulation, and the want of the due oxygenation or decarbonization of the blood is the sole cause of the animal's destruction."—BRODIE'S Lectures on Pathology," 1846. p. 86.

"It is surely not less essential to the progress of science and our art, to remove error than to establish truth. . . . . So that the warm bath is not only positively injurious by *poisoning*, but negatively, by excluding the de-poisoning process."—MARSHALL HALL, On Drowning," &c., 1857, pp. 76, 78.

[Mr. Hunter after citing the above quotations goes on to observe that the arrest of cardiac action is *not* the cause of death. Dr. Waters of Liverpool has shown by experiments that cardiac action goes on for an average period of nineteen minutes, dating from the commencement of asphyxia. The following inference may be drawn].

That the longer cardiac pulsations continue, after the lungs have ceased to act and purify the blood, the more effectually is the system being narcotized by the non-eliminated carbonic acid in the blood.

It would be well for the chief surgeon of the Royal Humane Society to ponder on these important truths: that the lungs do cease the first to act; and that *the longer the heart acts*, either naturally or *by hot bath stimulation, before the respiration is restored*, the more surely is the system being *poisoned* by carbonic acid narcotism. But they may possibly say—for they cannot argue the point physiologically—that the body is quite cold, the animal heat deficient; and that for these reasons, as well as to restore the heart's action, do they recommend the hot bath. There does, at first sight, seem some reason to justify the hot bath, not for the sake of cardiac stimulation, but to warm the body and retain what animal heat there still may be; but when the cause of the loss of animal heat is considered, I believe that true, sound physiology would discard the hot bath as a means of adding warmth to the body, and for retaining animal heat.

Indications for treatment.—What should be the rational treatment for a person with suspended animation, whose very symptom is traceable to deprivation of air and cessation of lung action?

"If a poor creature," says Marshall Hall, "be perishing for want of food, we cautiously administer food. If a man be in like manner perishing for want of air, should we not administer air? Is not this simple and reasonable? And, in the case of drowning, is not the want of air the first condition to which we should bring succour, and the want of temperature the second or third? And should we not first administer to the first want?"

Air is, then, the first want, and the power to breathe it the second.

What can be more clear or reasonable than the following admirable passage from Copland:—

"The indications are, firstly to remove the patient as soon as possible from the causes which ocassioned the asphyxied state and secondly, to restore the functions of the respiration, and *through it the circulation*. The restoration of the functions of the respiration is to be attempted by various means

calculated, in the first place, to dislodge the impure air contained in the lungs: secondly, to replace it with pure air; thirdly, to excite remaining vitality of the nerves and muscles; and, fourthly, to restore the circulation by measures calculated to return the blood from the lungs to the left side of the heart."

Dr. Richardson's first conclusion from his numerous experiments on resuscitation is in accordance with the views of Hunter, Fothergill, &c. "In restoring animation," he says, "artificial respiration *should always be first resorted to*, in the hope that there may be still some remaining action in the heart."

To attempt at once, in suspended animation, to restore the respiration, is, then, the unanimous opinion of the highest authorities on the subject of drowning.

To attempt to renew the circulation in the first place is to act contrary to sound sense and true physiology. But by *first restoring the respiration*, we physiologically, *and in the proper order*, nullify the various evils shown in the above arrangement to be produced by submersion: we cause air to enter the lungs; carbonic acid to be exhaled; oxygen to be received; the expansive motion of the lungs to be resumed—the muscular respiratory action also; the blood to circulate through the lungs, and if it did still circulate, to be circulated in a purer state.

Then as to the heart.—The establishment of the respiration unloads its right side of the excess of black blood accumulating there, and causes the left side to receive accessions of blood *gradually purer in quality*.

The system generally becomes more vitalized or gradually decarbonized. The brain and cord gradually resume their functions, consciousness and reflex action returning; a fresh and healthy impulse is given to the whole capillary system, in which "an arrest of blood change" had progressed to a greater or less extent; and by the removal of this stagnation of impure blood, one great (if not the) cause of the loss of animal heat is also removed.

By thus first restoring artificial respiration, we remove the *narcotism* affecting all parts of the system, or, as Sir B. Brodie expresses it "the want of the due oxygenation or decarbonization of the blood, the sole cause of the animal's destruction."

"Then, in the case of drowning, we should," with Marshall Hall, "administer air first, and warmth in the second place. But may not the warmth administered without air do great and absolute injury?"

Who can now imagine the drowned individual—whose every symptom is recognised by John Hunter, Brodie, Marshall Hall, and all other chief writers on the subject, to be traceable to the deprivation of air whilst in the water, and when out of it to the want either of the power to respire, or to impure blood from want of air—receiving benefit by the continuous hot bath? No plea can now be urged, unless contrary to all physiology, in favour of the hot bath, save for the warmth that it furnishes; and this warmth can be more rationally applied, when it is required, by other and more handy means.

The experiments previously related, in which I was kindly assisted by Mr. Tomlinson, Mr. Braine, and other gentlemen, showed the "fatal tendency" of the hot bath: how that the lungs of animals drowned in hot water were more congested than of those drowned in cold; how the normal effects of a hot bath were to stimulate cardiac action for the first twenty minutes at least; and the still more important point that, whilst the heat was increasing the cardiac power, it was at the same time acting as a depresser of respiratory action, as shown by the breathing becoming slower, more feeble, and irregular, with occasional deep efforts to give relief.

In reviewing these facts, I have no hesitation in asserting that for an individual with suspended animation from drowning, whose respiration is not yet restored, the speedy employment of the continuous hot bath would be *wrong*, both theoretically and practically, tending all the more certainly and quickly to cause death:—

1st. By increasing the congestion of the lungs;

2nd. By arresting lung action in more ways than one, to be hereafter detailed; and

3rd. By circulating poisonous blood in the system.

In these results I am remarkably confirmed by Dr. Waters, of Liverpool, in his 'Researches on Asphyxia'—a paper read before the Royal Medical and Chirurgical Society subsequently to the appearance in the 'Lancet' of the first portion of this article. And these conclusions, of which I hope shortly to show still further proof, are all in accordance with the views of the nature of drowning held by the ablest writers on the subject.

The hot bath, as the Royal Humane Society have used it of late years, for the treatment of drowning, is, then, an error, and a serious one.—*Lancet, Aug.* 10, 1861, *p.* 132.

152.—RULES FOR GUIDANCE IN CHLOROFORM ACCIDENTS.

By Dr. KIDD.

I. Young patients, up to fifteen, and females bear chloroform best; there seems a general "tolerance" of chloroform in large surgical operations, probably from the hemorrhage keeping the blood moving and preventing engorgement of the cavities of the heart; in females the reflex system is more active than in males.

II. Anæsthesia under chloroform is not at all of the nature of imperfect oxygenation of the blood. But this imperfect oxygenation may be superadded and run parallel with anæsthesia, leading to fainting fits after administration; this is shown also by the plunging or violent efforts of the patient, from black blood in the muscles: even fresh concentrated vapour, from fresh additions to the inhaler, increases these muscular efforts, and always suggest the necessity of more air to the patient.

III. Where simple syncope is feared from fright, it is well to postpone the operation for a week, feeding the patient during this interval on meat and wine; the latter strengthens the muscles of the heart; it is well to bring such a patient under the chloroform, too, in a quiet ward rather than in the excitement of the operating-theatre.

IV. A passive, fixed, starting forward of the eyes, sudden contraction of the pupil, probably from spasm of the fifth nerve, and sudden change of the countenance, are the first signs of danger; the pulse may remain unchanged; chloroform should be kept warm (100° F.); this prevents coughing, which is also a dangerous symptom.

V. Simple syncope is much more dangerous than apnœa; it occurs, however, less frequently.

VI. The operations where nearly all the accidents have occurred have been those for removal of toe-nails, dead phalanges, tooth-drawing, strabismus, &c., operations on testis, reduction of dislocations,—all more or less connected with tendinous tissues.

VII. The right side of the heart, in apnœa cases, continues to beat long after the pulse stops. Probably while the iris of the patient contracts on approach of light we have a fair chance of waking up the entire heart; the right side of the heart will beat twelve or sixteen hours after the pulse at the wrist. Faradisation of the phrenic nerve or spine will set the heart in full action in animals through the renewed action of the respiratory muscles. Pulling out the tongue by forceps, so common hitherto in accidents, is a mistake; irritation of the nerves of deglutition stops the diaphragm.

VIII. Of 125 deaths, 54 occurred immediately before operation; 42 during small operations (syncope?); 25 from ether. The number of males in all returns seems to be exactly double that of females, though chloroform has been used in at least 30,000 cases of midwifery, and syncope is common in females. This leads to a suspicion that the deaths from chloroform are like railway accidents in the management of cases.

IX. Vomiting, as in cataract cases, is best prevented by an aloetic purgative beforehand an hour or two; the stomach also to be empty at the time of operating, and the chloroform pushed well to deep anæsthesia.

X. The "cardiac syncope" of Snow is partly a post-mortem change. These are all cases of apnœa, or where the mischief arises essentially from fixture of the diaphragm. The heart, in fact, pulsates longer than the lung will receive or return the blood—hence the congestion, increased also by the efforts made at resuscitation by the Surgeon himself.

XI. As to resuscitation, the means adopted at first should be as gentle as possible: a candle-wick gone out is to be blown in, not smothered by the *nimia diligentia*. Fanning the patient with cold fresh air is first in importance; the hands dabbed with cold water, which has a powerful influence in exciting the brachial and heart nerves. Too much cold water is not advisable. The patient should be brought at once into the open air, if summer, or into a very warm room, if winter; turned on his right side; the soles of the feet and interior of the ear tickled with a pen; his left arm held up, smacked with a wet towel, and that axilla and side of chest dashed with cold water; then artificial respiration tried by "up and down" pressure rather than "rotations." Two or three needles next stuck, where the omohyoid lies at the outer edge of the sterno-mastoid, so as to hit off, if possible, the phrenic nerve; then the *moist* pole of a Faradisation apparatus tried over the part, or along the spine; the other pole inserted under the floating ribs.

XII. It is probable, in the milder cases, the previous measures will succeed, especially in the instances of apnœa; but if, after five minutes' trial, little progress is made, then tracheotomy is to be done, and a small tube (a No. 10 india-rubber catheter) passed, and air blown gently into the lungs (a large tube causes emphysema). Some ammonia and warm water should be thrown into the rectum. In syncope cases, transfusion into a vein, of warm water with a little soda in it may wake up the heart. In apnœa (asphyxia as regards the heart, the right cavities gorged), opening a vein is useful. These measures should be continued for at least four to six hours, the body kept warm by hot blankets in a semi-recumbent position, the battery, gases, and impure air of a crowd of students rigidly excluded; pure oxygen gas also to be avoided.—*Medical Times and Gazette, October* 5, 1861, *p.* 362.

153.—ON THE TREATMENT OF GOUT.

By M. Trousseau.

Lithontriptic lixivia and alkaline preparations were in high repute for gravel and gout as early as the preceding century; and even the waters of Carlsbad were recommended by Van Swieten. Nor can you be ignorant with what frenzy people have, in these times, pushed the use of the mineral waters of Vals, Vichy, and Carlsbad. My opinion is, that there does not exist in the world a more dangerous medication. I have, for my own part, certainly seen more than five hundred gouty persons who had been at Vichy, and had in consequence found themselves in wretched plight; and I do not know if, by way of compensation, my memory can furnish me with some isolated cases attended with real amelioration. Waters so strongly impregnated with alkali are inconsiderately prescribed by physicians, and are stupidly taken by patients. The danger thus incurred is often irremediable. M. Prunelle, who has for a long time practised medicine with much success at Vichy, was the first to point out the deplorable consequences of treating gout with alkalies *intus et extra*. But if, on the contrary, you have recourse to waters that are feebly mineralised—those, for instance, of Pongues, Contrexéville, Plombières, Spa, or Wiesbaden, not only will you never see the occurrence of untoward symptoms, but you will remark in the great majority of cases a sensible amendment. When you have gravel as well as gout, Contrexéville and Pongues will give you therapeutic results of great value. The physician who really at this moment has the largest practice at Vichy thinks it is true that the waters of these celebrated springs are useful to the gouty, but in a very restricted measure; and hence he recommends their use for ten or twelve days only at a time. Alkaline saturation, in fact, appears

to him a phenomenal expression of great gravity, and capable of occasioning a fatal result by unexpectedly provoking the appearance of atonic and visceral gout; and how many such examples has he not witnessed?

Peruvian bark was considered of such efficacy by Held, that he said, "Uno verbo, cortex peruvianus in podagra diviunm est remedium." In our 'Treatise on Therapeutics,' M. Pidoux and myself have said, "It cannot be denied that, at the beginning of an attack of acute gout, sulphate of quinine, given in large doses, dissipates the pains and abridges the fits as certainly, at least, as those pernicious drugs known under a variety of names; but it is not necessary to grow old in the profession in order to understand, from sad examples, how carefully you should respect attacks of acute gout, and how great are the dangers which they incur who seek for sudden relief, so constantly purchased at a terrible price!

"But if in acute regular gout we proscribe Peruvian bark, as we proscribe other remedies, vaunted in the same circumstances, the case is altogether different when gout becomes vague and visceral, characterised by asthma, dyspepsia, disturbances of innervation—disorders of respiration, digestion, and circulation. Then it becomes useful from time to time to give Peruvian bark, especially when the patient, after abnormal gout, has fallen into a state of confirmed cachexia." The swelling, pain, and engorgement of the joints —the arthritic lesions and deformities—as well as, in a word, all the consequences of gout—yield readily enough to the action of the waters of Aix, in Savoy; but do not send thither a patient still labouring under gouty attacks, for you might in that way occasion some formidable visceral disorder. I do not, then, recommend a season at Aix, in Savoy, but to persons who *have had* gout, and now bear its indelible stigmata; while persons who still have the disease I direct to Wiesbaden, Pongues, Contrexéville, Plombières, or Spa.

Gouty persons, in order to protect themselves from humidity and atmospheric changes, have an evil habit of covering themselves immoderately with flannel. They would do better gradually to accustom their bodies to resist cold, and get habituated, as summer sets in, to washing in cold water; then, every morning on leaving bed, for a few minutes to wrapping in a wet sheet, the temperature of which should after a time be more and more reduced. The simple and ingenious method of the Silesian peasant has furnished Boissière with some favourable results; and it has been well spoken of by M. Fleury. Hydrotherapæia, in fact, restores the cutaneous functions, prevents renal disorders, modifies consecutive symptoms, and communicates to the system a physiological impulse altogether new. Scudamore pretends that he could cite a multitude of examples to prove that the disposition to gout "has been effectually combated by having recourse in time to good advice, and by following a suitable mode of life and regular habits." We may say, in reference to this subject, that the prophylaxis of gout in a great measure consists in proportioning the quantity and quality of food and drink to the usual expenditure of the system, and to maintain the healthy condition of the renal, intestinal, pulmonary, and cutaneous excretions. [We think M. Trousseau might have added with advantage the Hippocratic rule, which to every one, without exception, enjoins *exercise* in proportion to the quantity of food taken.—Tr.] M. Boissière, moreover, has insisted much on corporeal exercise, and recommends to his patients walking, rubbing the *parquet* of their apartments, digging their gardens, sawing wood, &c., and dissuades the sons and descendants of the gouty from entering on any sedentary profession.

The gouty should avoid a diet consisting chiefly of meat, since the flesh of animals contains, in an equal volume, more albuminoid substances than vegetable aliment. Not only are the gouty liable to consume meat in too great quantity, but meats contain but few substances of an indigestible nature, and leave but a small amount of residue. Hence the peristaltic action of the intestine is not so much excited, and a state of habitual constipation may in this way be established. This fact did not escape the attention of Ambrose Paré; for he says, "the rich are tormented with gout more frequently than the poor, because they do not work, and they eat much and

have diverse meats at all their repasts, and they drink in a similar manner and to excess, and are too much addicted to the game of draughts." Fresh vegetables, herbs, green legumes, and the red and watery fruits, as Boissière has well remarked, give rise to citrates, malates, and organic acids, which, in the system, become changed into alkaline carbonates, the effect of which is to render the blood and urine less acid. The ultimate transformation of animal matters, on the contrary, tends to a result altogether the reverse, and acidifies in a greater degree both the blood and the urine.

Should the physician deem it necessary to commence with immediately changing his patient's mode of life, he must, if he would avoid a check, or an aggravation of the disease, endeavour to accomplish gradually that change, especially should the gouty subject be old or enfeebled through his gouty diathesis. As muscular exercise, daily carried to the extent of causing slight fatigue, may, if gradual, be favourable; so harrassing the patient with violent efforts suddenly undertaken after a sedentary life and long-continued muscular inactivity, may rapidly produce a fatal result. When it is once thoroughly understood that nothing should be attempted with precipitation, nothing exaggerated, then may your patient immediately commence with corporeal exercise.

Such is the picture of gout I proposed to lay before you. Like every chronic affection, it perverts the nature of the whole man; "homo integer deinceps nova inende refingendus." Physicians who listen to me, apply yourselves, then, to get a thorough knowledge of the disease.—*Medical Circular, June* 26, 1861, *p.* 451.

154.—*Formula for the Administration of Chloroform, Ether, Turpentine, Camphor, or Essential Oils.*—M. Vée recommends the following as a valuable means for the equable administration of these medicinal substances:—Chloroform (or any of the above) 4 parts (or less); oil of sweet almonds, 15 parts; powdered gum arabic, 10 parts; water, 100 parts; syrup, 25 parts. The chloroform is to be dissolved in the oil, and rapidly emulsified, to prevent evaporation. *Syrup of Chloroform* may be well prepared by the following formula:—10 parts by weight of chloroform are to be dissolved in 60 of oil of sweet almonds, 40 parts of gum added, and an emulsion formed with 350 parts of water. In this cold emulsion, placed in a close vessel, 540 parts of sugar are to be dissolved. It is a very stable syrup, rendering water white on admixture with it, and it contains exactly one-hundredth of its weight of chloroform.—*L'Union Medicale; and Med. Times and Gazette, June* 8, 1861, *p.* 607.

155.—ESSENCE OF BEEF.

By Dr. Druitt, London.

[Essence of beef is one of the old devices of cookery, and has been in use for a very long period.[

When meat, chopped up, and put into a closed vessel (such as an earthen jar, with a cover, sealed up with flour paste, and tied up in a cloth), is boiled or baked for an hour or more, it undergoes separation into three portions,—fat, solid fibre. and liquid essence. The last is easily strained off, and the fat separated by blotting-paper. It is, when nicely made, a clear, amber liquid, of a very meaty, aromatic smell and taste; but, as will readily be imagined, the quantity obtainable from a given weight of meat varies infinitely as the quality of the meat and the proportion of pure muscle, in contradistinction to sinew, varies. When made from old meat, it is darker; when from young meat, lighter; and, in this latter case, becomes gelatinous when cold. Almost one-fourth of its weight is yielded by ordinary meat.

Evaporated to dryness, it yields about one-sixteenth of solid residue; but this varies likewise.

The object of this preparation, in contradistinction to common soup or concentrated beef-tea, &c., is, to administer the purely stimulating elements of meat in a separate form. If all the soluble constituents of meat be extracted by boiling water, a gelatinous extract is obtained, far better adapted for most cases, whether in health or sickness; for the gelatine of meat has a very high nutritive value. But there are cases in which we wish to stimulate, and in which the stomach loathes a quantity of gluey matter, and for such cases this essence is adapted.

The object of my bringing it under the notice of the Obstetrical society was to advocate the use of it as a partial substitute for brandy, in the treatment of the consequences of hemorrhage, and other states of exhaustion incidental to the puerperal state. It will not supply the place of brandy during the early and urgent stages of exhaustion from loss of blood, though even then it is the best auxiliary, but it does very largely supply the place of an alcoholic stimulant in the treatment of the after stages, when the brain is suffering from anæmia, and when the patient complains of headaches, giddiness, and despondency, particularly in the early part of the day. It is equally valuable in states of mental depression arising from other causes, such as grief, mental shock, &c., in which the usual resort is to wine or brandy. It may be taken as a stimulant after breakfast, at the time of the day when nervous feelings are most distressing, and when alcoholic stimulants are most pernicious and most likely, if used at all, to lay the foundation of injurious excess. Out of the vast number of drunkards, there are a few whose intemperance has followed the use of wine for the relief of mental depression caused by anæmia. Bodily weakness alone does not tempt to drinking, except when it is attended with mental despondency; but with that combination it does tempt most strongly. But the essence of beef, freed from the grosser gelatinous particles, is directly absorbed, and exhilarates the brain specifically, causing the same phenomena in slight degree as brandy does. It is in cases such as this, as a substitute for brandy, and as a means of preventing habits of drinking, and of curing them, that I more particularly recommend its use to my Professional brethren. One tablespoonful taken early in the forenoon, with or without a little bit of crust and a mouthful of wine, gives the brain a something that relieves it from nervousness and vapours. It should be given, not as food at meals, but in the guise of medicine, between them.

In dyspepsia, likewise, it is a capital remedy, and one infinitely preferable to pepsine, as I venture to think. There are very many cases of general debility and emaciation in which a gelatinous soup is preferable; but in the cases in which a nutritive stimulant without bulk is required, as in that hopeless disease called "old age," your readers may find that this liquid is a valuable addition to their stock of remedies. It may, if required, be flavoured with a little dill water or camphor, and administered out of a medicine bottle.

[Mr. JARDINE MURRAY, of Brighton, considers that Dr. Druitt has been forestalled in the recognition of the value of this material, and in introducing it to the notice of the profession. Dr. Christison, in 1855, not only showed how it may be prepared, but how it may be preserved. The following is Dr. Christison's method:]

Cylindrical cases of tinned iron are filled each with six pounds and a-half of beef; and the lid is soldered on, but with a hole of about half-an-inch in diameter in the middle of it. Two trays of such cases are shoved into iron retorts, analogous in form to retorts for gas-making, but double-cased, so that steam may be introduced into the interstice around. They are thus subjected to a heat of 220° under steam pressure, for about three hours, by which the beef is partially cooked, and being thus also made to contract strongly on itself, squeezes out a portion of its juice, amounting to a few ounces from each tin. The tins are then drawn, the juice is poured out, and the meat, with certain additions, is subjected to the preservative process. The juice, after being cooled and entirely freed of fat, is put into small four-ounce tin cases. Each of these has a small

aperture at one end, which is secured with solder, after the juice is poured in. The tins are then subjected, on trays, to a temperature of 220° in a muriate of lime bath. On being removed, the solderer rapidly touches with his iron the solder on the top, which giving way allows steam to rush out forcibly and carry with it the air in the upper part of the interior. By the time he has thus swiftly passed over sixteen or twenty tins, the first is ready for being re-soldered by a similar dexterous application of his iron, which then in succession as quickly secures the whole open and steaming apertures. The process of heating in the bath, tapping, and re-soldering, is then repeated a second time, to make sure of the thorough expulsion of every particle of air.—*Medical Times and Gazette, June* 1, *and May* 25, 1861, *pp.* 587, 560.

A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES; SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY).

AFFECTIONS OF THE SYSTEM GENERALLY.

ACUTE RHEUMATISM.—In the treatment of acute rheumatism by means of large doses of alkalies, care must be taken that the bowels be well opened. If this is not the case, large quantities of alkali are retained in the system, and sometimes produce unpleasant effects. (Prof. Reid, p. 26).

Give twenty-five to forty drops of the tincture of the *Actea racemosa* three times a day. It may be obtained of Messrs. Duncan and Flockhart, Edinburgh. It is almost uniformly successful in both acute and subacute rheumatism. (Dr. F. R. Mac Donald, p. 25.)

GOUT.—The waters of Vichy, Vals, and Carlbad, do more harm than good in gout. Waters so *strongly* impregnated with alkali are inconsiderately prescribed in the present day. But the waters which are *feebly* alkaline do much good; these are to be found at Pongues, Contrexéville, Plombières, Spa, and Wisbaden. When your patient has gravel as well as gout, send him to Contrexéville or Pongues. Alkaline saturation provokes the appearance of atonic and visceral gout. It is better to accustom the surface to cold, by means of bathing or the cold douche, than to keep it too warm by clothing. Also avoid too much animal food, and take a fair amount of exercise. (M. Trousseau, p. 314).

In the course of the twenty-four hours give two or three of the following pills for three or four successive days. Quinæ sulphatis, gr. xxij; ext. seminis colchici, gr. viij; ext. digitalis, gr. iv. M. divide in pilulas decem. (M. Trousseau, p. 27).

INTERMITTENT FEVER.—You may often succeed as well, and even better, by giving arsenious acid in these cases, than by the use of quinine. Give it as follows: Arsenite of potash, and compound tincture of cardamons, of each half a drachm; gum mucilage, three drachms; camphor mixture or water, half-an-ounce; mix. To be given every second hour for four or five times, the last dose to anticipate the expected paroxysm at least two hours. Even to a baby nine months old ℥xx. of the liq. potassæ arsenitis may be given in ten hours, repeating the dose next day if necessary. (Mr. J. Turner, p. 25).

MALARIOUS FEVER.—Mix a purgative with quinine in the treatment of these cases, as follows: Resin of jalap, calomel, of each eight grains; quinine and rhubarb of each four grains; mix well together and make into pills. Dose from ten to twenty grains. This medicine brings away a great quantity of black bile almost like blood. Sometimes you have to give a dessert-spoonful of epsom salts, when the above does not operate sufficiently. Quinine must next be given till the *ears ring*. The above doses are for strong adults. (Dr. Livingstone, p. 17).

SCARLET FEVER.—Use the sesquicarbonate of ammonia as a gargle, as follows: Sesquicarbonate of ammonia, two drachms; water six ounces. Gargle the throat with this every four hours. Also give it internally in five or ten grain doses, according to age, three times a day. (Dr. J. McNab, p. 24).

AFFECTIONS OF THE NERVOUS SYSTEM.

DELIRIUM TREMENS.—In a case related by Dr. Carey, of Guernsey, opium aggravated the symptoms; whilst half an ounce of tincture of digitalis with an equal quantity of gin, caused the patient to fall into a deep sleep, which continued without intermission for twelve hours. He awoke quite rational, and in two days was quite well. The digitalis did not produce its alleged results, diarrhœa and vomiting, nor were the kidneys stimulated to increased action. The influence of the drug on the pulse was not such as to offer any barrier to its repetition. (Dr. F. E. Carey, p. 37).

The large doses of tinct. digitalis, half-ounce doses, at long intervals of twenty-two or twenty-four hours, may be given safely. The pulse falls, but acquires power and steadiness, and the tremor subsides. It is best to confine its use to young and robust subjects, whose strength has not been broken-down by prolonged habits of intemperance, and to cases of excessive spirit drinking. (Dr. T. B. Peacock, p. 39).

AFFECTIONS OF THE CIRCULATORY SYSTEM.

APOPLEXY.—The first effect of an apoplectic seizure is a violent shock to the nervous system. The skin becomes damp and cold and pale, the pulse and heart beat quickly and irregularly. If the patient be bled at this time fatal syncope might ensue, but after a time the heart recovers its power; if this take place we must bleed to prevent a return of the cerebral hemmorrhage. In some forms of apoplexy these spmptoms do not occur—as in that form depending on destructive softening of the cerebral substance. In such a case bleeding would aggravate the symptoms materially. But in all cases we may safely be guided by the pulse; if it be sharp and hard at the wrist it will be sharp and hard in the brain. (Dr. T. K. Chambers, p. 41).

VARICOSE VEINS.—By applying repeated blisters over varicose veins of the leg, you cause deposition of fibrine in the surrounding areolar structures, and partial obliteration of the enlarged vein. The vein of course ceases to be varicose, and any ulcers present heal. (Mr. Ure, p. 99.)

Use Mr. Wood's pair of needles, which are of the thickness of an ordinary hare-lip pin, and bent at right angles, about an inch from the head, forming, at the bend, a loop for the reception of the point of the fellow needle. The head, or blunt extremity, is also formed into a loop to serve for the purpose of retention by a strip of lint or plaster. The point of one needle is straight and spear-shaped, as in the hare-lip pin; that of its fellow is curved as in the ordinary surgical needle for sewing up a wound. The method of application is as follows:—The skin over the vein being pinched up and raised from the vein by the finger and thumb, the straight needle is passed close under the skin, between it and the vein. The finger and thumb are then adjusted so as to raise the vein from the fascia a little on one side of the needle. The curved needle is then passed through the same punoture in the skin as the former one, but in an opposite direction and under the vein, which is thus included between the needles. The needles are then locked on to each other by passing the points of each through the loop at the bend of the other. The punctures are then protected from pressure by the interposition of a piece of lint or wash-leather, and the needles pushed in so that the loops are close to, but do not press upon the punctures. The points of the needles are then cut off with pliers. The bent head of one needle is then turned upon its fellow until it lies on the limb in the same direction. This will be found to twist the shafts across each other in such a manner as to compress laterally the included vein, and at the same time slightly to twist it. The needles are then held firm in their places by a strip of lint twisted figure-of-eight-wise, and a strip or two of sticking-plaster, care being taken to prevent all chafing. They are left in a week or ten

days, until so much effusion has taken place as permanently to obstruct the current in the vein, and by progressive absorption from *acupressure on both sides* to cause complete obliteration. In the many cases treated by Mr. Wood in this manner, there has been no suppuration whatever in the track of the needles, when care has been taken to prevent chafing or irritation. A drop or two of serum, forming a sugar-like crust, is the only discharge. (Mr. J. Wood, p. 103.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ASPHYXIA.—*Effects of the Hot Bath.*—Thirteen animals, as nearly as possible the same age and strength, were drowned, that is, were submerged until respiration had entirely ceased. Seven were placed in the warm bath, on removal from the cold water, and the other six left to themselves. Of the latter, four recovered; of the former, only one. With regard to the morbid appearances in cases of death of the animals put into the hot bath, compared with those of the animals not so treated: "in the animals put into the bath, the lungs were much more congested, more full of blood; they were firmer in substance and specifically heavier than those of the animals not so treated. Both sides of the heart were loaded with blood. In some instances the blood was coagulated in the vessels of the lungs, the systemic veins, and the cavities of the heart. The blood was generally less fluid than in the animals not put into the bath, and coagulated more rapidly when removed from the vessels. In no instance did the bath produce a respiratory effort or any movement whatever on the part of the animal; it seemed, further, to shorten the duration of the heart's action." In those animals which had died after being placed in the hot bath, the lungs were dark and firm, and almost liver-like in appearance. In some instances portions sank in water. These experiments seem quite sufficient to show the injurious influence of the hot bath, both when asphyxia is complete, and when recovery is commencing. "In the former case, the bath momentarily increases the circulation; but respiration being in abeyance, the lungs become loaded with blood, and the left side of the heart distended. In the latter case, respiration being imperfect, engorgement of the lungs takes place, and subsequent arrest of the heart's action." The prolonged use of the hot bath in asphyxia is not only inefficacious but dangerous, and its temporary use is attended with no direct benefit. Probably, by far the most efficacious means of producing respiratory movements is the alternate dashing of hot and cold water on the body. It is better to omit all artificial treatment whatever, than to use the warm bath when respiration is going on freely. (Dr. A. T. H. Waters, p. 280.)

HOOPING COUGH.—The common clover hay (*Trifolium in fæno*) given as an infusion, ℥ ij. to Oj., or as a syrup, has a wonderful effect in relieving hooping cough. The remedy is purely empirical, but it nevertheless cures most cases in a few days. (Mr. M. Foster, p. 49.)

PHTHISIS.—*Chlorate of Potash and Sugar.*—For some years the author's practice has been to combine the administration of from one to three or four ounces of the chlorate per week, with the use of three or four pounds of sugar during the same period. If the patient has a dislike for sweets, he substitutes sugar of milk. Some of his cases, from a state of debility and emaciation have grown fat and strong. (Dr. J. Turle, p. 48.)

SPASMODIC ASTHMA.—It is a common practice in China, for asthmatics to smoke arsenic, and with the greatest relief to the disease. A very interesting case is related in which a French lady did the same in consequence of hearing the above. She commenced with a quarter of a grain, swallowing the smoke. From being in a state of constant breathlessness and suffering, unable to lie down or make the slightest exertion, she is now able to go about like other persons, and is rarely threatened with an attack oftener than once in three or four months, and that is at once checked by smoking arsenic with a very small quantity of belladonna or stramonium in the do

I have mentioned. She now uses, instead of a cigarette, a small red pipe about five inches long. (Dr. F. G. Julius, p. 50.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

DIARRHŒA.—Give the following: Compound infusion of gentian, eight ounces; tincture of opium, a drachm to a drachm and a half; nitric acid, twenty minims: one ounce to be taken after every loose stool. (Dr. P. J. Hynes, p. 59.)

DIPHTHERIA.—The best application to the throat which I know of, is Beaufoy's solution of chlorinated soda. A full-bodied camel's hair-brush is the best instrument for applying it, and to ensure its full efficacy the throat should be thoroughly mopped with it as many as three or four times a day. The general treatment I adopt is a supporting diet and a pretty liberal use of alcoholic liquors. The next favourite medicine in such cases is the tincture of sesquichloride of iron in considerable doses. (Dr. W. Budd, p. 56.)

DYSENTERY.—*Tropical.*—The treatment now usually adopted in the Military hospitals in India is the following. On admission a sinapism is applied to the epigastrium, and ℥xxx. of tincture of opium are given, for the purpose of rendering the stomach more tolerant of the remedy. An hour afterwards ʒj.,—ʒiss. of ipecacuan is given in powder; it is better to give it about half an hour after the tr. opii, but by waiting for a whole hour the medicine is retained longer and produces a more powerful effect. As may be supposed, considerable nausea is the result, but vomiting does not generally come on until at least an hour afterwards, sometimes two hours. During this nausea copious perspiration is produced over the whole body; the pulse becomes more full and soft, and less frequent; the countenance loses the expression indicative of suffering, so marked in acute dysentery; tenesmus and abdominal pain immediately cease, and the patient has no more stools for twelve and sometimes twenty-four hours. (Dr. R. W. Cunningham, p. 57.)

HEMORRHOIDS.—*External.*—When the swelling outside the anus is owing to the rupture of a dilated vein, incise it, and allow the coagulum to escape. When the swelling is owing to enlarged and inflamed portions of skin, excise it with the knife or scissors, and do not depend on the ligature. (Mr. J. Lane, p. 114.)

Internal Hemorrhoids.—In internal piles three modes of treatment may be adopted—excision, the ligature, and nitric acid. Do not employ excision, as the bleeding is sometimes fatal, and you cannot always *get at* the bleeding vessels. The ligature in internal piles, therefore, is much safer, although not so rapidly successful. Mr. Salmon's operation is a kind of compromise between excision and ligature. It consists of a separation of the hemorrhoidal tumour from the subjacent parts, for about the lower three-fourths of its extent, leaving it attached by the remaining upper fourth, which is then included in a ligature. The tumour is drawn down by means of a hook with four prongs, and the division of the lower part of the tumour is made with scissors. These structures are always supplied by vessels which descend *from above*, close beneath the mucous membrane, and the trunks being necessarily included when the upper part of the tumour is tied, hemorrhage is avoided. In nearly all internal piles there is a swollen and everted portion of skin outside the anus. Do not cut away too much of this, else you will produce stricture. Remove only all the irregularities, and all the *pendulous* portions of the skin but do not meddle with the circular fold which surrounds the anus. To remove too much skin would certainly end in severe contraction of the anus, but it is not so if you remove *mucous membrane* only. *Nitric acid* is very useful when there is a very *vascular condition of the mucous membrane* of the rectum without large and distinct tumours. (Mr. J. Lane, p. 114.)

HERNIA.—*Radical cure of.*—The following is the method of operating now

practised in most cases of scrotal hernia by Mr. Wood. An incision directed obliquely downwards and outwards is first made through the skin of the scrotum over the middle of the hernial sac, with a small narrow-bladed knife. The edges of the skin are separated from the subjacent fascia by carrying the blade of the knife flatwise under the skin to the extent of three-quarters of an inch from the margin of the incision. The denuded fascia being invaginated by the fore-finger of the right hand into the inguinal canal, carrying the sac before it, the lower border of the internal oblique is felt for, and raised upon the finger. The curved hernia needle is then carried along the finger at its thumb side (*i.e.*, towards the patient's linea alba) and passed through the conjoined tendon from behind forwards till the point of the needle is seen to raise the skin of the abdomen about one and a-half inches above the pubes. The skin of the groin is drawn inwards as far as possible, before the needle is pushed through it. A piece of stout copper wire silvered, and tapered off at both ends to a flexible degree of thinness is attached by one end to the eye of the needle, and drawn back with it through the scrotal incision, and then detached and left in the wound. The invaginating finger is then carried outwards behind Poupart's ligament, or external pillar of the ring, as high up as possible. The aponeurotic structures are lifted upon the finger, and the needle passed through them on the finger along its opposite border (*i.e.*, that nearest the ilium of the patient). The point of the needle is carried through the same aperture in the skin of the groin as in the first instance. The other end of the wire is next fixed to the needle, drawn back with it into the scrotum, and also detached. The sac of the hernia, at the scrotal incision, is then pinched up from the cord behind by the thumb and forefinger of the left hand, and the needle passed across it in front of the cord, in the same manner as in passing a needle for the cure of varicocele, but within the limits of the scrotal incision. The inner extremity of the wire (*i.e.*, that which included the conjoined tendon) is then fixed to the eye of the needle, and drawn through the sac across the front of the cord. The two ends of the wire are now drawn down till the loop or bend is close to the upper puncture, the loop being held by the forefinger of the assistant. The ends of the wire are twisted firmly down upon the fascia or sac in the lower puncture, and cut off at a point about five inches distant from the skin. By traction upon the upper loop the twisted ends of the wire are then drawn up into the inguinal canal, invaginating the sac and fascia, and leaving, when drawn tight, about two inches protruding from the lower puncture. The upper loop of wire is next twisted firmly down into the puncture in the skin of the groin until the sides of the canal are drawn firmly together. The lower or cut ends of the wire are then bent into a small hook, and curved upwards, so as to meet the upper or loop end curved downwards to meet it in a corresponding degree. The hook is fixed into the upper loop, a pad of lint inserted between the wire and the skin, and a spica bandage and compress put over all, so as to exercise firm pressure upon the posterior wall of the canal. The spermatic cord is left behind the wire with a small portion of the neck of the sac covering it, this part of the sac being obliterated by the pressure of the wire backwards upon it. (Mr. J. Wood, p. 106.)

New Operation for Radical Cure of.—Having invaginated the scrotum upon the finger, transfix the scrotum at the apex of the invaginated portion with a long, strong, curved needle, fixed firmly in a handle and armed with a silver wire. The needle must be carried through the internal column and the skin of the abdomen, one end of the wire seized and the needle retracted. Again pass the needle through the external column, and, by gliding the skin upon the needle, pass the needle out at the same opening in the skin of the abdomen as before; retain the other end of the wire and withdraw the needle completely. The finger must now be withdrawn, and the two ends of the wire drawn tight and twisted, so as to bring the columns into close apposition. Draw the twisted wire firmly outwards, and clip off as close as possible to the skin, so that when the traction is discontinued the ends of the wire are completely concealed by the integuments, which heal over them.

The wire remains permanent; it is soon imbedded and isolated in a deposit of lymph. It is essential to success that the loop encircle the columns of the ring near their points of attachment to the pelvis. This operation is very easily and quickly performed, and is very safe and successful. Moreover, the patient is only withdrawn from his ordinary occupations for five or six days at the furthest. (Prof. Chisholm, p. 107.)

PROLAPSUS ANI.—Sometimes the greatest relief may be obtained by combining the use of nitric acid with subsequent removal of transverse folds of integument from around the margin of the anus. Proceed as follows: Give an injection, get the prolapsed portion well down, and apply the nitric acid to the prolapsed portion. In two or three days repeat the application if necessary. In two or three days more remove from around the anus the thickened folds of skin and mucous membrane, by making a few separate incisions, at *right angles to the orifice, dipping the points of the scissors well inside the sphincter.* If the mucous membrane protrude again after this, apply the nitric acid again after a time, and even a fourth or fifth time, if necessary; but it is better *not* to use the nitric acid after the external incisions have been made, if you can avoid it. By and by use a bougie, else there will be contraction of the anus. This operation will not answer where there is much indurated and thickened membrane protruded, or when conjoined with large hemorrhoidal tumours of a blue colour; but it will answer when the mucous membrane is not indurated, however vascular and voluminous it may be. (Mr. H. Smith, p. 117.)

RECTUM.—*Flat Ulcers of.*—These exist just within the anus. Keep the bowels gently open, and apply an ointment composed of a drachm of the nitric oxide of mercury to an ounce of lard. (Mr. W. Coulson, p. 122.)

AFFECTIONS OF THE BONES AND JOINTS, ETC.

AMPUTATIONS.—The rectangular operation of Mr. Teale has many advantages. When you operate on the leg be careful to avoid wounding the anterior tibial artery at the base of the flap; if you do this you make the flap more liable to slough. "First mark out the anterior flap by carrying the two lateral incisions through the integuments only, and the transverse incision through all the structures down to the bone. The aponeurosis covering the muscles in front of the leg should next be divided throughout the whole length of the flap, close to the edge of the tibia on one side, and of the fibula on the other. This being done, there is little need for the further use of the knife, as the finger is almost sufficient to detach the mass of muscles upwards from the interosseous ligament, more especially in the situation of the artery, a few touches of the knife being only required laterally to separate the muscles from the bones." The stumps made in this way are very superior and bear pressure much better than in cases of the circular and double-flap operations. This is a point of the greatest importance to the future comfort of the patient, as in many, if not all other kinds of amputation of the leg and thigh the patient has to support himself by an apparatus resting either on the head of the tibia, or the tuberosity of the ischium. (Mr. Teale, p. 70.)

DISLOCATION OF THE SHOULDER.—To reduce this dislocation the shoulder should be steadied firmly by two stout rollers, one passing over the clavicle and under the chair on which the patient is sat, the other round the chest and under the axilla of the injured side, the ends being secured firmly to the wall or some unyielding substance. The extending force should be applied from the wrist of the injured side; the tendon of the biceps being thus rendered tense, assists in restoring the bone to its place; moreover a much longer leverage is thus obtained. The operator must place his foot on the edge of the chair, and use his knee as the fulcrum. (Dr. N. R. Smith, p. 82.)

DISLOCATION OF THE KNEE-JOINT.—It is a great mistake, when, in cases of inflammation of the knee-joint you see contraction taking place, not to

correct it at once. This very flexion is in itself the cause of the necessity for amputation in many cases, and must be most carefully guarded against. (Mr. J. Hilton, p. 74.)

FRACTURE OF RADIUS.—Remember that nearly all fractures of this bone are near the wrist. When the arm is placed midway between pronation and supination, any pad applied in front, and covered with a splint, will exercise its chief pressure along the centre of the fore-arm, and will not be sufficiently oblique to restore or maintain fully the concavity of the radius. Therefore use a splint which has attached to its radial border a piece of wood, so bevelled that it fills up and fits accurately the natural concavity of the radius. The fracture renders the palmar substance of the radius convex instead of being concave. The surface of the splint is also convex, and covered with tow. The convexities of the splint and radius are mutually brought into apposition. The breadth of the splint at its lower end should not exceed that of the fore-arm at the wrist: it should not project so much internally as to be on a level with the inner border of the fore-arm. On the radial side it should project beyond the radius, and the bevelled piece for filling up the concavity of the radius should be attached half-an-inch at least internal to that border. By this arrangement the ulnar side of the fore-arm sustains the pressure of the bandage or straps. The reverse is the case on the radial side. (Dr. A. Gordon, p. 86.)

HOUSEMAID'S KNEE.—Pass through the tumour a thickish thread, which allow to remain in. This sets up inflammatory action, known by a little redness around the entrance of the thread, and the swelling either subsides altogether, or, what is more common, an abscess forms, which is opened, and the cavity becomes obliterated. This treatment is only suitable after all acute symptoms have some time subsided, and best of all to the hard and indurated bursæ. In some cases the walls of the cyst have become so thick and solid that no plan of treatment short of actual removal will prove of any avail. (Mr. F. C. Skey, p. 79.)

LOOSE CARTILAGES IN THE KNEE-JOINT.—To remove these loose cartilages adopt the following plan. Fix the cartilage firmly at the lower side of the joint, and entrust this to an assistant; next make a subcutaneous separation of the cellular tissue with a long tenotomy knife, two inches in length. From this puncture direct the knife upon the cartilage and freely incise the synovial membrane; then slide the cartilage along the track prepared for it, and fix it at the end of its track with a pad of lint, strips of plaster, and bandage. Apply a straight splint at the back of the limb, and place it at an angle of forty-five degrees. This will keep the parts at perfect rest. (Mr. W. J. Square, p. 75.)

PIROGOFF'S OPERATION.—It is a decided improvement in Pirogoff's operation to saw through the calcaneum from below upwards, previously to disarticulating the joint. (Prof. Fergusson, p. 82.)

AFFECTIONS OF THE URINARY ORGANS.

ALBUMINOUS URINE.—It is well known that in some specimens nitric acid occasions a cloudiness from the precipitation of uric acid or urates quite independently of albumen. If the test be applied in the following manner it is easy to distinguish between them. Let the test-tube containing the urine be held in an inclined position, and the acid allowed to trickle slowly down its sides; the heavier liquid will sink through the urine, and form a distinct layer at the bottom of the tube. If albumen be present, a cloudy zone will be produced *immediately above and in contact with* the nitric acid; but if no albumen exists in it, but an excessive proportion of urates, a cloudy zone will appear *at the surface of the liquid*, while the lower part, that in contact with the nitric acid, will remain perfectly clear. (Dr. L. Beale, p. 63.)

BLADDER.—*Irritable States of.*—Buchu is a favorite medicine with many

practitioners in some affections of the bladder and kidneys, but Mr. Thompson thinks that it is often excelled by the root of the grass called *Triticum repens*. In vesical irritability from affections of the prostate and bladder, calculus, stricture, &c., it is a valuable medicine. Infuse one ounce of the dried and cut stems in one pint of boiling water for an hour. Give from twelve ounces to one pint in the day, in divided doses. (Mr. H. Thompson, p. 142).

Diabetic Urine.—*How to ascertain the quantity of Sugar easily.*—Take the specific gravity of the urine before and after fermentation, and from the loss of density occasioned by the conversion of sugar into carbonic acid and alcohol, calculate the amount of sugar destroyed. "The analysis of a diabetic urine by the clinical method is thus performed:—About four ounces of the urine are poured into a twelve-ounce phial, and a lump of German yeast, of the size of a small walnut or so, is added. The phial is then loosely corked, or covered with a slip of glass, and placed on the mantel-piece, or other warm place to ferment. Three or four ounces more of the same urine are likewise put up tightly corked in a *companion phial*, without any yeast, and placed beside the *fermenting phial*. Fermentation soon begins, and proceeds with such rapidity, that in twenty-four hours not only is the process completed, but the froth and scum are dissipated from the surface, and the urine, though still cloudy, is in a fit state for the second step in the process,—namely, taking the densities. It is advisable, however, to remove the phials from the mantel-piece a couple of hours beforehand, so that their contents may cool down to the general temperature of the room. The specific gravity of the two products is then separately observed in the usual way,—by pouring the urine into a cylindrical glass vessel, and dropping in the urinometer. The fermented urine will be found to have suffered a very great diminunition of density, ranging from thirty to forty degrees—more or less, according to the amount of sugar destroyed. This diminution of density holds such proportion to the sugar originally present in the urine, that for each grain of sugar per fluid ounce, there is a fall of one degree of density; or, in other words, *for every degree of density lost, you may count one grain per ounce of sugar in the urine.* The following example will serve to illustrate the proceeding:—

Density of a diabetic urine before fermentation	1040	degrees.
Density after fermentation	1002	...
Density lost by fermentation	38	...

These 38 degrees of 'density lost' indicate that the urine contained exactly 38 grains of sugar per fluid ounce, or 740 grains per imperial pint. In ten minutes—five minutes one morning and five minutes the next—an accurate quantitative analysis can thus be made. Indeed, the preliminary part of the operation may be left to the patient himself or his attendant. There is no skill required to set aside a few ounces of urine, in a twelve-ounce bottle, with a lump of German yeast, and to place beside it a 'companion phial' of the same urine without yeast. The medical attendant need only perform the second part of the operation—the taking of the densities; so that, five minutes a-day, he may keep an exact register of the diurnal separation of sugar by a diabetic patient," (Dr. W. Roberts, p. 64).

New Test for Saccharine Urine.—The following test is stated to be of great delicacy and extreme facility of application. Upon a clean slip of tinned iron, place one or two drops of the suspected material, and hold it over a spirit-lamp: the fluid will speedily evaporate, leaving, if the process be arrested at that point, scarcely a trace upon the metallic surface. Continue the application of heat; in a few moments after the dessication is complete, a spot of an inch or so in diameter, over which the drop had spread with the first ebullition, will gradually assume a rich reddish-brown colour, with a brilliant lustre, as if coated with a film of varnish or Japan lacquer. A strong heat produces a darker colour, but the lustre continues till the heat becomes sufficiently intense to decompose the substances. (p. 67).

LITHOTOMY.—In young subjects it is necessary to cut well into the membranous part of the urethra. If this precaution be not observed, the prostate is liable to be pushed on the end of the finger into the bladder, thus inverting the order of things. Operators often, when they have reached the groove of the staff in the membranous part of the urethra, as urine escapes, think they have reached the bladder. "No such thing: the membranous part of the urethra, somewhat like a syphon, is generally if not always, filled with urine —the groove of the staff acting as a conductor of the urine. If you do not cut fully onwards into the prostate, so that you can easily pass your finger into the bladder, a complication will assuredly happen. I consider the lateral operation as good as any other: other modes are introduced—I shall abide by the lateral cut. For simplicity of procedure, safety in execution, freedom of action, and ample space of opening—these circumstances all recommend the lateral operation to me before all the other modes, especially in children. I use the scoop in children in preference to forceps; but small forceps may be used on some occasions." (Prof. Fergusson, p. 140).

PHYMOSIS.—The prepuce being put upon the stretch, divide it directly backward for about a quarter or a half an inch. The glans will be exposed, and the adherent mucous membrane of the fore-skin may be separated. By means of a blunt pointed pair of scissors, such portions of redundant skin and mucous membrane may be cut away, as will allow the penis and glans to remain exposed, and the cut edges of the skin and mucous membrane may be brought together with fine sutures. (Mr. P. C. Price, p. 139).

RENAL DROPSY.—*Succeeding Scarlet Fever.*—When the urine is albuminous after scarlet fever, and the limbs dropsical, give the following draught to a child six years old: Benzoate of ammonia, five grains; spirit of nitrous ether, fifteen minims; syrup of tolu, one drachm; camphor mixture, one ounce; to be taken three times a day, and an occasional purge of the compound jalap powder. (Dr. J. Taylor, p. 24.)

STRICTURE OF THE URETHRA.—It seems that out of every hundred cases in which external division of stricture has been performed, six have proved fatal. Considering that the disease itself is not mortal, this should deter us from employing external division, except absolutely compelled to it. A stricture, not of traumatic origin, rarely becomes impenetrable to a skilfully-plied bougie or catheter. The most frequent causes of failure are the inopportune employment of the catheter, and impatience in its use. There is not the objection to the simple internal division, recommended by Mr. Thompson, which applies to the external division of Syme. (Mr. J. Gay, p. 124.)

Simple dilatation is safe, efficacious, and *dilatory*. Incision of the stricture by a sheathed knife has long been performed, but fell into disuse, and Mr. Syme substituted his external incision. The danger of internal incision is from hemorrhage, infiltration of urine, or pyæmia: these will be more likely to be caused by many modern methods of internal incision, such as using a blade which is protruded from, and at a considerable angle with, its sheath, after it has passed beyond the stricture, and which is then pressed on the floor of the urethra and slowly and steadily withdrawn outwards. This will often cut the sound as well as the diseased structure, and cause much mischief. Can we therefore have an instrument which will *only* cut the diseased, consolidated, and contracted structure? This can be done either by using a knife so *blunt* that it will only penetrate the part much pressed upon, or one so *sharp* that it will cut by pressure alone, or without any forward or backward motion. For this purpose use Dr. Arnott's instrument described at page 136. (Dr. J. Arnott, p. 134.)

In some of the most obstinate and most discouraging cases, you may pass an instrument and *split up* the stricture at once, with the greatest relief and a fair degree of safety. The instrument consists of two grooved blades fixed in a divided handle, and containing between them a wire welded to their points, and on this wire a tube (which when introduced between the blades corresponds to the natural capacity of the urethra,) is quickly passed, and thus ruptures or splits the obstruction. The following description in Mr. Holt's own words, will best explain the process: "The permeability of the

canal having been once satisfactorily ascertained, the size of the meatus of the urethra is to be gauged by passing into it a sound that will conveniently fit, and the number of the sound so used is to be the number of the tube to be passed: this it is important to ascertain, so that the urethra may not be stretched beyond its natural limits, for while the urethra of one person will admit a No. 14, another will not admit more than No. 9. The dilator having been previously well oiled, is to be introduced with the handle somewhat over the patient's left hip, and by keeping the convex portion gently pressing against the under part of the urethra, the point will glide along the upper portion until it is fairly beyond the triangular ligament, when, by bringing the handle to a right angle with the body, and gradually depressing it—but not so much as in the passage of an ordinary catheter—it will usually slip into the bladder; in fact, the same proceeding is to be adopted as in introducing a lithrotite for the purpose of crushing a calculus. Having reached the bladder, the dilator should be gently rotated, to prove that it is fairly within that viscus, and being thus assured, the surgeon is now to place the point of the tube he had previously selected upon the wire, between the blades, and thrust it quickly onwards to the end. The stricture being now fairly split, the dilator should be rotated to still further separate the tops of the rent, and be then withdrawn, a catheter corresponding to the number of the tube being substituted for the purpose of removing the urine. The catheter is now to be taken out, and the patient sent to bed, with directions to take, every four hours, for the first day and night, a mixture containing in each dose two grains of quinine and ten minims of the tincture of opium." (Mr. B. Holt, p. 126.)

VARICOCELE.—Cases of pendulous testicle, as they present themselves in practice, may be divided into three classes: 1. Those in which the skin and subjacent tissues are relaxed; 2. Those in which the veins alone are enlarged; and 3. Those in which both skin and veins are relaxed and enlarged. The first may be remedied by the removal of a portion of the integuments only; the process of cicatrization braces up the surrounding parts, and the evil is thus remedied. In the second class of cases the obliteration of the veins is requisite, and this may be done by subcutaneous section, without any wound in the skin beyond that which is required to admit the thinnest knife. But in the third class, where the skin is much relaxed, the simple obliteration of the veins will not remedy the pendulous condition of the testes. In such instances it is necessary to remove a portion of the skin together with the subjacent enlarged veins. The best mode of operating is the following: Having introduced a thin needle between the vas deferens and the enlarged veins, a figure-of-8 ligature is passed over the ends of the needle, and the bunch of veins is compressed. The same proceeding is to be repeated at the lower part of the scrotum. In each case a piece of thin leather is placed under the thread to prevent any pain that the pressure on the skin might occasion. A considerable extent of the skin of the scrotum and of the enlarged veins beneath it are thus included between the two needles, and the circulation in these parts in a great measure obstructed. A thin knife is then introduced about midway between the two needles, and between the vas deferens and the enlarged veins, and directed upwards, so as to divide the integument near the upper needle. A second incision is made to pass downward, so as to divide the skin near the lower needle. A wedge-shaped portion of tissue—skin, enlarged veins, and all—is thus removed, and a very considerable interval left. Some extremities of divided veins, and some cellular tissue may be removed with scissors, and a clean surface left. Although such an extensive surface is exposed in so vascular a part, yet no hemorrhage follows; this being, in fact, completely controlled by the acupressure of the needles. The needles are now drawn together and maintained in position, and thus approximate the edges of the wound, which may then be dressed by a piece of dry lint, and the patient sent to bed. (Mr. H. Lee, p. 140.)

Pass, by means of a needle, a double silver wire between the vas deferens and the veins, and by the same apertures in the opposite direction,

behind all the veins, another wire. By engaging one free end of the wire through the loop of the other one, and doing the same on both sides, the veins may be compressed by traction on the wire. This traction must be kept up constantly by means of an elastic watch-spring, bent in the shape of a horse-shoe. In ten days' time, the wires may be cut off as short as possible, and the skin will heal over the remaining portion, from which no inconvenience is ever felt. (Mr. R. Davies, p. 141.)

AFFECTIONS OF THE SKIN, ETC.

CICATRICES FROM BURNS.—The causes of the unfavourable results so frequently experienced after operations for the cure of cicatrices from burns, is probably owing to too little attention being bestowed upon the subsequent process of extension or stretching. It is probable that where assisted by a cutting operation, and careful and continued extension of the cicatrix is maintained, much more successful results will follow. It is in some cases a good plan to begin with extension, carry it on perseveringly for some time, and then, if necessary, to employ subcutaneous section of the most resisting bands of the cicatrix. Finally, if this fail, the more severe operation of dividing the cicatrix may be resorted to. The best and perhaps the only instrument which will answer the purpose of keeping a cicatrix on the neck fully extended, consists of a shoulder-piece of steel, fitted closely and firmly over the shoulder before being hardened, and retained in its place by two straps passing round the chest. A firm cushion is placed against the lower jaw, and fixed there by two straps passing over the top of the head; the shoulder-piece and the cushion are connected by two steel slips, which can be lengthened at pleasure by means of screws: thus, any amount of extension can be applied at will. This apparatus must be taken off every night, and be re-applied during the day. Some of the most resisting bands may be divided subcutaneously. Where after a fair trial of this for many months, no improvement is perceptible, make an incision both above and below the cicatrix, close to it, but through the healthy skin: the cicatrix may then be dissected from its attachment sufficiently to admit of extension to the desired extent. It must be remembered that much perseverance in the use of extension is necessary to success, but success often follows ultimately. (Dr. J. K. Barton, p. 151.)

CYSTIC TUMOURS.—Many cystic tumours are formed by the obliteration of some natural outlet of a gland: of this nature are sebaceous tumours. In these cases look for the obstructed outlet, usually a small black pore, remove the obstruction by pressure or the introduction of a pin or needle; squeeze out the contents of the tumour, and then destroy and cause exfoliation of the cyst. For this purpose a piece of chloride of zinc paste may be passed into the interior. The cyst will usually mortify, and will either come away in small pieces, or it may by a little manipulation be removed by the forceps. (M. A. Courty, p. 147.)

ECZEMA.—We can always cure eczema, however long it may have continued, though when the disease depends upon internal causes there may be considerable difficulty. In these latter cases remedies given internally may be required, as mercury or arsenic; but in the great bulk of cases external measures only are requisite. When the disease arises from excess of secretion, as in the axilla, or between the buttocks, starch mixed with oxide of zinc is an excellent application. Oxide of zinc (ʒj to ℥j of lard), sulphate or acetate of zinc, alum (ʒj to lbj of water), red or white precipitate (6 to 12 grains to ʒij of lard), are of good service in acute eczema, or in chronic when there is but slight infiltration, and the disease prevails only over a limited extent. When, however, there is considerable infiltration in chronic eczema these means do not suffice, and then the almost imdispensable *schmierseife*, by reason of its slightly caustic action, becomes the most pre-

ferable remedy. Its mode of employment varies according to the degree and extent of the eczema. When there is but slight infiltration, a rubbing with it once or twice a day is sufficient; but when the infiltration is more considerable, a more frequent application is necessary, and even epithems composed of it may be required. When we have thoroughly rubbed the skin with the soap, until excoriations and red points have been produced, the surface should be washed and cold applications laid on until next rubbing. This procedure must be continued as long as the moisture and itching and the infiltration of the skin continue, and until the frictions no longer give rise to heat and excoriations of the skin. The soap must then be replaced by cold applications, and the treatment terminated by the employment of tar. In many chronic cases, with great infiltration, it may be necessary to employ a stronger form of potash than the *schmierseife* soap, viz., caustic potash, one drachm, dissolved in two drachms of water. A pencil of charpie is dipped in this and well rubbed into the diseased parts for some minutes. The part must be well washed, and cold applications constantly applied. One or two such cauterizations usually suffice. (Prof. Hebra, p. 159.)

GLANDS.—*External Lymphatic.*—When suppuration takes place, followed by induration, scars and uneven surfaces, apply a lotion composed of one drachm of iodide of potassium one drachm of carbonate of potash, one ounce of spirits of wine, and eleven ounces of water. This should be kept constantly applied. Mr. Price substitutes the iodide of ammonium for the iodide of potassium. Sir Astley Cooper used the chloride of ammonium. Sometimes the scar is so disagreeable that it is necessary to destroy it. For this purpose caustics may be used, as chloride of zinc and potassa fusa. Potassa is the best, as the subsequent scar is more shining and smooth. Sometimes the knife is required; the puckered cicatrix may adhere to the subjacent parts, and you may then divide these adhesions subcutaneously by introducing a fine knife at a little distance, and wiping it along under the skin, so as to accomplish what you want. You must then draw the cicatrix away from the divided bands, so as to prevent adhesion again. (Mr. P. C. Price, p. 143).

SMALL-POX.—*Pitting from.*—The best method of preventing the pitting which occurs so frequently in cases of small-pox, is to apply the linimentum aquæ calcis on cotton wool. Pour the liniment on a plate, and dip in it masses of cotton wool, answering to the size and shape of the parts to be covered. Apply the wool so as to completely cover the face and neck, leaving apertures for the eyes, nostrils and mouth. This dressing may be allowed to remain until convalescence; and if it becomes accidentally detached at any part it should be immediately renewed. (Dr. J. Bell, p. 150).

SCABIES.—Itch may be cured most rapidly by benzine. Every part of the body must be well sponged with it; one or two applications will be sufficient. (Mr. J. J. Godfrey, p. 157).

VENEREAL AFFECTIONS.

GONORRHŒA.—When the discharge has existed a few days only, the following is the practice usually adopted amongst soldiers by the author. A blister six inches by four is placed very high up towards the anterior and inner aspect of each thigh, and is safely confined in its place by a broad slip of adhesive plaster at each end. For convenience the blisters are generally directed to be applied at night, and no harm has ever come of it, and usually they are well risen by the morning. The patient is then ordered to take every four hours an ounce of the following mixture: Sulphate of magnesia two ounces; carbonate of magnesia, four drachms; potassa-tartrate of antimony, two grains; tincture of hyoscyamus, two drachms; peppermint-water, to sixteen ounces. He is placed on spoon diet, with rice pudding for dinner, and a pint of imperial drink should he be thirsty. During the day he is directed to inject now and then a syringeful of cold or lukewarm wa-

ter, according to the temperature and season of the year. The blistered surfaces to be dressed with lint dipped in castor oil (a favourite dressing with the men). At first there is often a little aggravation of the symptoms but by the third morning the discharge is much diminished in quantity, the scalding in micturition is less, and the patient feels better. In another day the symptoms have nearly disappeared. The saline purgative must be continued through the case, but in diminished quantities, and the patient must continue to drink freely of mucilaginous drinks; and rest and low diet must be rigidly enforced. Ordinary recent cases are curable in four days by this treatment. Old obstinate cases of gleet seldom fail in being completely relieved by blisters; and in instances where the first application is insufficient, a second set of blisters will prove completely effectual. Occasionally a weak nitrate of silver injection is of service. The complication of swelled testicles in no way invalidates the success of the treatment by blisters. (Mr. H. C. Miles, p. 166).

As an injection use three times a day a solution of pernitrate of mercury made by dissolving half-a-mimin of the liquor hydrargyri pernitrate in one ounce of water. (Mr. B. Childs, p. 166).

Dr. Irvin seldom resorts to any other remedy in the treatment of urethral inflammation than chlorate of potash. His method of using it is as follows:—One drachm of the salt dissolved in eight ounces of water, of which an injection is given every hour for twelve hours, at the end of which the discharge has become changed and diminished, allowing the remedy to be gradually discontinued until the second or third day when the disease will be generally found to have ceased. (Dr. Irvin, U.S., p. 170).

Mix twenty parts of well-washed bismuth (if not well washed, the acid which remains may give rise to irritation,) in 200 parts of distilled water, causing as much to be taken up as possible. Let this be used as an injection, a local emollient bath being first employed. This is not so useful in chronic as in *acute* cases. (M. Caby, Dr. Moulson, p. 166.)

Psoriasis.—*Mercury and Arsenic.*—Mercury is a never-failing remedy in cases of syphilitic psoriasis, but has no power at all over cases of ordinary psoriasis. Arsenic is almost a specific in cases of non-syphilitic psoriasis, whilst it is of no use in cases of syphilitic origin. By far the best way of employing mercury in cases of syphilitic psoriasis is the use of calomel vapour baths. Let the patient heat to redness half a common brick, and place this in a common pan, the bottom of which contains a little boiling water. The calomel (20 grains) must be placed in a thin layer upon the surface of the brick, and the whole placed beneath a cane-bottomed chair, on which the patient must sit enveloped in a blanket, drawn tight around the neck, but covering the body loosely. The hot brick has the effect of keeping the water boiling, at the same time as it volatilizes the calomel. (Dr. M'Call Anderson, p. 170.)

AFFECTIONS OF THE EYE AND EAR.

Black Eye.—The discoloration may be got rid of in a few hours by applying the washed and scraped root of the "convallaria," or "Solomon's Seal." A tingling sensation is produced at first. The convallaria is readily found. (Anon, p. 173.)

Eustachian Catheter.—*How to pass.*—There is only one fixed rule, namely, to keep the catheter on the floor of the nasal cavity; all other rules are liable to numerous exceptions. In general the beak of the catheter is directed downwards, and towards the septum, as the catheter passes through the nasal passage. Sometimes, however, it must be directed upwards; and in others the floor of the nasal cavity passes obliquely outwards and downwards, and is as it were hollowed out; in this case the catheter must be kept far from the septum. If any resistance is met with, rotate the instrument gently, but employ no force. Having passed the catheter through the nose,

turn the beak of the instrument downwards, if it has not already in its transit been held in this direction, which in many cases it may be. When the posterior wall of the pharynx has been reached, two ways are open of bringing the catheter into the mouth of the Eustachian tube. Either its beak is turned immediately outwards on the posterior wall of the pharynx, whereby the instrument is brought into Rosenmüller's cavity, therefore behind the projection of the tube, and is now drawn gently, feeling our way, straight forwards over the prominence of the orifice; and so soon as it is felt that it has glided over the latter, it is gently pressed into the mouth of the tube; or the beak of the catheter is drawn, directed downwards, from the posterior wall of the pharynx directly forwards, back to the velum palati (in so doing it is well to press the beak with somewhat greater force still more downwards, in order to be sure of hitting on the velum, and not by more gentle manipulation to run the risk of returning into the nose); having arrived there, the beak is turned outwards and somewhat upwards, and pushed *a little* forwards, whereby it passes into the mouth of the Eustachian tube. The latter operation is often still further facilitated by causing the patient to make the movement of swallowing at the moment when the quarter-turn is made, and the catheter is thus usually involuntarily thrown into the mouth of the tube. (Dr. Voltolini, p. 173.)

EYELIDS.—*Adhesions of.*—When the eyelids adhere to any part of the eye-ball, so as to interfere with vision, or to render the movements of the eye-ball difficult, observe the same principle in dividing these adhesions and transplanting healthy tissue as you do on the skin, when you wish to remedy the cicatrices from burns, &c. Dissect the eyelid from the eye-ball, so as to allow the eye to move freely; fill up the raw surface by one or more flaps of conjunctiva from the parts adjacent (only use the conjunctiva for this, and do not include the subconjunctival fascia); and keep the parts together by sutures. (Mr. T. Pridgin Teale, p. 172.)

PRESBYOPIA.—A very rapid increase of presbyopia is one of the premonitory symptoms of glaucoma; but suspicions should be aroused, and we should not fail to examine as to the presence of other premonitory symptoms of glaucoma. (Mr. J. S. Wells, p. 173.)

MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN.

AMENORRHŒA.—If local stimulation be required in the uterus to induce the menstrual flow, in cases of amenorrhœa, the best means at our disposal is the introduction of a small galvanic intra-uterine pessary. This instrument consists of a small copper bulb, from the middle of which rises a stem, made half of copper and half of zinc, and two and one-third inches in length. The stem of this instrument must be introduced, so that the upper surface of the bulb is felt to be in apposition with the lips of the cervix. The instrument may be introduced by the fingers alone, but it is so made that a staff may be fitted into the lower end of it, if necessary, to facilitate its introduction. In the majority of cases the instrument thus introduced will remain of itself *in situ*. After it has been worn for some days, the zinc portion is encrusted with saline matter, and this facilitates its remaining in position; it should not however be allowed to accumulate much. If necessary, a small gutta-percha pessary may be introduced below the intra-uterine pessary, for the purpose of supporting it and preventing its slipping out. (Dr. J. Y. Simpson, p. 271.)

One reason why *electricity* is not more used in the treatment of amenorrhœa, is, that it cannot well be effectually applied externally. In young and unmarried women, however, in whom we are led to supect a torpid state of the vaso-motor nerves of the ovaries and uterus, and also in such cases where the catamenia have been suppressed in consequence of emotion and cold, electricity externally applied is a most valuable emmenagogue. Fara-

disation of the skin of the soles of the feet by means of wire brushes, or of the abdominal parietes by means of moistened conductors; and finally, the application of one moistened conductor to the nape of the neck, and of another over the os pubis, are the most effectual methods in cases of this kind. Perseverance is just as necessary with electricity as with most other remedies. (Dr. Althaus, p. 270.)

ATROPHY OF CHILDREN FROM DIRT-EATING.—Cases now and then occur in which children are ill from some cause apparently unaccountable, until it is discovered that they have a habit of eating dirt, such as earth, cinders, chalk, rags, gravel, lime, in fact anything the child can lay its hands on. The symptoms produced are anæmia, tumid unhealthy abdomen, a downcast and forbidding look, palpitation, and others of a similar class. The use of enemata, along with the internal administration of calomel or grey powder, forms the most suitable treatment for ordinary cases. Dr. Corrigan recommends calomel in repeated doses, followed by castor oil and turpentine. When the offending matters are removed we must endeavour to improve the character of the secretions generally, and for this purpose, the alterative powders mentioned, with the continued use of tepid baths, will be found most efficacious. In the convalescent stage the syrupus ferri iodidi, vinum ferri, or other chalybeate, as indicated. Of course the most careful watching, as regards diet, and to prevent the vicious propensity being indulged in, is indispensable. (Dr. W. Moore, p. 282.)

CEREBRAL SYMPTOMS IN CHILDREN.—Whenever, in young children, the tuberculous cachexia is much developed, cerebral disease is liable to come on extremely insidiously. In some cases there are scarcely any symptoms at all for a long time after actual disease has commenced, and in others they are so slight as not to awaken suspicion until too late. At the post-mortem examination, the amount of disease is often very small, and yet it has been sufficient to destroy life, (Dr. C. West, p. 123.)

DIPHTHERITIC ULCER OF THE OS UTERI.—Diphtheritic ulcer of the os uteri is a very rare, but at the same time, very intractable disorder. It is mentioned even by but few medical writers. It has at first sight considerable resemblance to syphilitic ulceration. It commences with redness and pain; and after a short time there is a crop of small, whitish, smooth, and shining patches of different and irregular forms. These patches are extremely adherent to the cervix uteri, and even friction with lint fails to remove them. At the end of a few days, however, they fall off spontaneously, leaving an ulcer. This ulcer, by some recent French writers of eminence, is said to heal with the greatest facility; this, however, arises from their confounding an aphthous with a diphtheritic ulcer. There is, before the diphtheritic patch has fallen off, a certain slightly rough and dry sensation communicated to the finger, contrasting forcibly with the otherwise polished surface of the os; this is very peculiar, if not diagnostic. With the speculum the os is seen to gape, and its sides to be deeply ulcerated, the surface of the sore being coated with a whitish membrane. Here and there the membrane is perhaps removed, and a raw vascular bleeding surface uncovered. This disease is most commonly met with at or about the close of the functional life of the uterus. In the treatment of this affection carefully avoid the use of nitrate of silver; it invariably aggravates the symptoms much. Neither the tincture of iodine nor the potassa cum calce produce this effect, and these form our best means of curing this most tedious complaint. The actual cautery applied by the electric wire is very useful. The wire must be maintained at a white heat by a powerful Grove's battery, and tightly drawn over the ulcer. It is useless applying any of the weaker caustics. But an exclusively local treatment is by no means successful. There is a deep-lying fault in the patient's constitution, and the effort must be made to rectify that co-existingly with the absolutely indispensable use of local means. Iron, quinine, and chlorate of potash present themselves as the remedies for the restoration of the general health; and an occasional course of cod-liver oil will be useful, if the digestive powers can be got up to the point of its easy assimilation. (Mr. R. Ellis, p. 250.)

DYSMENORRHŒA.—*From Mechanical Obstruction.*—The canal may be first dilated by a small plug, and then some cutting-instrument must be used. For this purpose Dr. Coghlan has invented an instrument about eight and a half inches long, including the handle, and slightly curved. The point is blunt; and, at the distance of a quarter of an inch from the extremity, the instrument swells out into two cutting blades, about three-eighths of an inch wide—these again subsiding into the stem. The advantage of the instrument is, that, guided by the probe point, we are sure with ordinary care to have our incision right into the uterine cavity, and perfectly central, and by using instruments of different widths, we can have our incision of the exact extent we desire. Having made the incision, take a small leaden tube, made by rolling a piece of sheet-lead, pass it into the cervix, and, by means of a pair of long thin dressing forceps introduced into its interior, dilate it to the full extent of the incision. (Dr. J. Coghlan, p. 275.)

FIRST STAGE OF LABOUR.—When we know from previous experience that a patient will probably have a tedious and protracted first stage of labour from non-dilatation of the os uteri, it is a good plan to give small doses of belladonna for a fortnight before the termination of gestation. An eighth of a grain of extract may be given three times a day combined with a very small amount of antimony. (Dr. B. F. Barker, p. 223.)

FORCEPS.—*Application of.*—When the head has entered the pelvis so that an ear can be felt easily, and labour is going on briskly the child becomes in danger if delayed too long, and if there be some impediment: therefore use the forceps. If you do not know the exact position of the head, feel for an ear, fix the forceps over the ears and simply make traction, until the head has somewhat advanced. Then, if possible, gently turn the head round at the same time that traction is being applied, so as to bring the face into the hollow of the sacrum. Sometimes you can best do this by making it turn towards the right side, sometimes towards the left. First, always try to turn it towards the right if the face be towards that side, opposite the symphysis pubis, or if even slightly to the left, as the rectum on the left side diminishes the capacity of the pelvis and offers an obstacle to the free version of the head in that direction. When the face is towards the left acetabulum, or more decidedly still to the left, first try to turn the face to the left into the hollow of the sacrum, but should you fail, try to turn the face quite round to the right, so as to reach the hollow of the sacrum in this circuitous way. (Dr. G. Hamilton, p. 205.)

There are some cases especially liable to happen in primiparæ, where, from the duration of the labour and size of the fœtal head, the latter becomes much elongated, so much so that the ordinary forceps is quite inapplicable, the blades not being long enough and too sharply curved. For such cases an instrument should be used the blade of which is 8 inches long (instead of 6½ or 7 inches), the curve that of a circle of 14 inches (instead of 10 or 11 inches), and identical in other respects with the ordinary short forceps. (Dr. G. Hewitt, p. 208.)

It is not a fair representation of the application of the forceps to show it as embracing the head with perfect symmetry. The grasp is oblique, one blade lying on the right brow or temple, the other on the occiput, or behind the line of the transverse diameter. This arises from the long forceps being introduced in the transverse diameter of the pelvis, and not with reference to the child's head. This is an important rule to follow. In some cases, with the head low in the pelvis, it may be possible to apply the short forceps with a blade on either parietal bone; but it is a bad practice. The pelvis should be the guide to the position of the forceps, not the head. (Dr. R. Barnes, p. 209.)

HYSTERIA.—There are cases in which an hysterical tendency is evidenced less by the simulation of disease which does not exist, than by the exaggeration of the symptoms of some affection which has an actual existence. In these cases, it is the most successful plan not to treat the nervous affection, but to continue to direct the treatment principally against the original affection. Thus, in a case of syphilis, with hysterical contraction of the elbow-

joint, treatment directed so as to cure or relieve the syphilis, cured the hysteria. (Mr. W. Coulson, p. 265.)

OVARIOTOMY.—In operating for ovariotomy, do not close the wound perfectly around the peduncle, so that any discharge may find vent. Sometimes a little peritonitis sets in, and the serum formed is highly poisonous, so much so, that a most minute scratch of the finger touched with it causes severe symptoms. (Mr. T. S. Wells, p. 263.)

PERINEUM.—*Support of in Labour.*—The chief uses in supporting the perineum are, to relieve the irritation caused by the descent of the child, to diminish congestion of the part, and to counteract the too powerful contractions of the womb, when the perineum is not ready for the head to pass. It is doubtful whether it does any good to press the head forwards towards the pubic arch. Forcing the head over a perineum unprepared, is just as likely to cause laceration as drawing it back, or leaving it to itself unsupported. (Dr. E. W. Murphy, p. 201.)

Laceration of the Perineum.—Why does laceration of the perineum occur in one case and not in another? One great reason for this diversity is the position and length of the perineum, and its relation to the pubes and coccyx. The pubic margin of the perineum is nearer the symphysis pubis in one female than in another; and upon this depends whether it is out of the axis of the outlet or not. In one case it is scarcely carried forward at all by the presenting part of the child's head, whilst in another it is carried forwards by the head, and distended from three to five inches; in the latter cases laceration can hardly be prevented. Much may be done by guiding the head forward in the direction of the abdomen of the mother; if we do not prevent it, we may at any rate greatly lessen the rent. (Dr. J. Gray, p. 200.)

Operation for Lacerated Perineum.—Use the needle suture of Dr. Aveling, made by Messrs. Cocker Brothers, Sheffield, and sold by Mr. Aitken, cutler, York. Place the ruptured parts together and secure them with two needle sutures; bend the wire a little before introducing the needle point; pass each end through a smooth flat splint of wood, and fix them by means of pierced duck shot. Do not approximate the parts too tightly, and chiefly attend to approximating the deep parts rather than the superficial. The parts can be approximated more tightly in a short time, if necessary. (Dr. T. Skinner, p. 203.)

It is very essential to success that a sufficient quantity of the mucous membrane be removed from the vagina, to secure its more perfect contraction. There is a firmer and more secure apposition of the parts when silver hare-lip pins are used instead of the ordinary quilled suture. (Dr. D. L. Roberts, p. 243.)

PREMATURE LABOUR.—To bring on premature labour by injecting water into the uterus is effectual, chiefly in proportion as it separates the membranes from the uterus, but it is a sloppy and often disagreeable method, although generally safe enough. Never inject air or carbonic acid gas, as the open veins may take in the air and cause serious mischief, and even death. One of the best ways is to introduce a flexible male catheter, to separate a portion of the membrane at some little distance up the inside of the womb, taking care as much as possible to avoid the situation of the placenta; then withdraw the stilet and leave the flexible portion of the catheter in the cavity till labour comes on. Labour will commence in about twelve hours, but if the instrument perforate the membranes the labour will perhaps be retarded for twenty or thirty hours. (Dr. J. Y. Simpson, p. 178.)

The use of *caoutchouc dilators* for inducing premature labour is spoken very highly of. A caoutchouc bag is introduced partially within the os, and then filled with water; the pressure so produced acts uniformly and gently. As a means of overcoming rigidity, it is superior in efficiency to bleeding, antimony, or stretching by the hand. (Dr. R. Barnes, p. 180.)

Statistics show that a much greater number of children are saved when premature labour is induced by separation of the membranes by means of injecting tepid water, than when the membranes are punctured. Thus 88

per cent. were saved by Dr. Hamilton, who separated the membranes but did not puncture them, and only 45 per cent. by Dr. Merriman, and still less by Dr. Lee, who punctured the membranes. (Dr. W. O. Priestly, p. 181.)

The plan usually pursued by Dr. Davis when desirous of inducing premature labour, is to introduce a sponge-tent within the os, for the purpose of dilating it sufficiently to admit the finger. Sometimes it is unnecessary to use the tent. Introduce the finger slightly within the os, and pass it round inside so as to dislodge the mucous plug and separate the membranes; subsequently, a piece of soft sponge previously oiled, and as large as can be passed without much difficulty, must be introduced. This plan will answer in most cases. (Dr. H. Davis, p. 182.)

PLACENTA PRÆVIA.—Contraction of the womb is the condition upon which the arrest of flooding depends, and the great object of all interference in the management of cases of placenta prævia is, to secure this saving contraction of the uterine muscles. In ordinary cases of placenta prævia it may suffice, frequently, to simply detach with one finger passed through the cervix all that portion of the placenta which adheres to the cervical zone of the uterus. It must not, however, be supposed that the separation of the placenta, either partially or wholly from the uterus, can of itself stop the flooding. The hemorrhage ceases because the muscular fibres of the part become more contracted, and are able to close the open mouths of the vessels. If this proceeding does nôt suffice, tap the amniotic sac, and drain off the liquor amnii. If the uterus still remain flaccid, and hemorrhage continue, or, where the patient is much exhausted, so that effective muscular contraction cannot be roused, artificial aid is called for to accelerate delivery. If the cervix be freely expanded, the child may be immediately turned by two fingers of the left hand passed through the cervix, tilting the head forwards over the pubes, the other hand externally pushing the breech backwards and downwards until a knee or foot is reached. But if the os is undilated and rigid, we must dilate by the caoutchouc dilator. A suitable one is introduced into the os and cervix, and is then distended with air or water, so as to put the cervix gently on the stretch. Besides acting as a dilator, this elastic sac presses on the mouths of the bleeding vessels and arrests the hemorrhage. Sometimes this measure suffices, until the natural powers terminate the case favourably without further help; if not, we may turn and deliver when the size of the os admits; and the hemorrhage being stopped we can wait for this. Sometimes the placenta is found attached only by a small portion, it is then better to remove it entirely. (Dr. R. Barnes, p. 188.)

The old practice in cases of placenta prævia, and with many men even yet, the usual mode of practice is, to wait till the os is sufficiently dilated and then to turn the child. The mortality when this practice is pursued alone is very considerable, being to the mother one in every three cases. The mortality is very much reduced (to one in forty-four) if, as early as possible in the case the finger be introduced in the os, and the placenta separated as far as it can be reached. If the detachment is completely and properly effected, it will rarely fail to stop the hemorrhage. This mode of practice was first suggested and practiced by Mr. Kinder Wood, of the Manchester Lying-in Hospital, and was subsequently taken up and advocated by Prof. Simpson, of Edinburgh. It is supposed by many that the separation of the placenta is certainly fatal to the child, whereas only one in five are born dead, whilst when the hemorrhage is allowed to proceed, at least half the children are born dead. (Dr. C. Clay, p. 193.)

URETHRAL IRRITATION IN FEMALES.—There are cases of urethral irritation in females, characterised by a constant desire to void the urine, yet each attempt, instead of bringing relief, only aggravating the sufferings. The urine is not more acid than usual; it is clouded with mucus. The urethral mucous membrane appears very red and congested, and on the passage of a catheter is found to be exquisitely tender. The bladder itself does not seem to be affected. The best treatment of these cases is to give constant small doses of liquor potassæ, well diluted with water. Also, every day, af-

ter washing out the bladder with warm water, to inject a syringeful of solution of nitrate of silver (five grains to the ounce), and allow it to come away of itself. Rest in the recumbent posture must be enjoined. (Mr. J. Hunter, p. 265).

Uterine Hemorrhage.—One of the ordinary vulcanized india-rubber balls may have attached to it a small brass tube, in the way described at page 228, so as to form a pessary, which will either act as a pessary or to restrain hemorrhage. The simple admission of air through the brass tube is sufficient to dilate the ball, which may be introduced into the vagina in a state of collapsion. (Dr. G. Hamilton, p. 228).

Vesico-Vaginal Fistula.—Two or three things are highly important in all these operations. Do not cut away any of the mucous membrane of the bladder when paring the edges of the fistula. Do not run the needle through any portion of the mucous membrane, as, if you do, this will probably be the track of a new fistula; hence carry the sutures *close to* but not *through* the vesical mucous mumbrane. In order to get as broad a surface as possible for adhesion, you may pare the edges in an oblique direction, that is, *bevel* them. Always use metallic sutures. (Mr. T. S. Wells, p. 233).

Split the margin of the fistula all round, that is separate the edges, but do not pare any away. This splitting of the parts gives a larger raw surface when properly managed than simply paring the edges. Either wire or thread sutures will answer—good thread is better than iron wire. Each ligature consists of a double thread and when all are passed through the flaps, a piece of vulcanized india-rubber cord is run through the looped extremities, which are succesively pulled tight upon it, and the free ends are then tied over a smaller piece of cord at the opposite side of the fissure; the sutures are not to be tightly drawn, as, if the raw edges are tightly drawn together they will *puff up* between the india-rubber quills, and the flaps will slough from being too tightly confined. The advantages of this operation are: that it gives a more extensive raw surface to be united, as the split surfaces by gentle approximation open their mouths, as it were to be united; this raw surface is obtained without any loss of structure; the ridges which rise up as the split parts are approximated are an advantage, preventing the water escaping and coming in contact with the wound; the operation is simple and requires no complex armamentarium. Use strong brass divaricators, or even a few common iron spoons, properly bent, will act as divaricators, and a good sharp scalpel will do the work well enough. (Mr. M. H. Collis, p. 236).

MISCELLANEA.

Alcoholic Stimulants in Disease.—These are *stimulants* and *tonics* i.e., *medicines*, and not *food*. It is a grave error to give them too indiscriminately in the earlier stages of acute disease. When required give them in very moderate quantities along with food, except to patients who have been largely dependent upon them before the illness. In short, Dr. Todd's views in many respects are incorrect, and not to be followed. (Dr. W. T. Gairdner, p. 289).

American Hellebore.—*(Veratrum Viride).*—This drug possesses a great power of calming a nervous excitement, and reducing the force and frequency of the heart's action. These two effects, therefore, are invaluable in a medicine. In sthenic diseases it acts like the lancet, without reducing vital power; it is more prompt and certain than digitalis, and yet is not cumulative; it is better than antimony both as a sedative, expectorant, and diaphoretic. The best preparation is Dr. Norwood's tincture, to be obtained from America. If the British practitioner procure this preparation, let him begin with half the dose recommended by Dr. Norwood. Give three or four drops every two hours till the pulse comes down, increasing the dose if necessary. When emesis is produced the pulse comes down sufficiently, the skin softens and profuse perspiration breaks out. In pneumonia, pleuritis,

acute rheumatism, croup, iritis, and other inflammatory diseases when the pulse is strong, and in which cases you would give antimony, ipecacuanha, aconite, and such like sedatives, you may more safely and efficaciously substitute the American hellebore. (Dr. Otterson, p. 296).

ESSENCE OF BEEF.—To prepare this put the meat to be used, first chopped up, into an earthen jar, closed at the top, and the edges of the cover sealed up with flour paste. This jar may be placed in the oven for an hour, or it may be placed in boiling water for that period; after which time its contents will be found to have separated into three portions, fat, solid fibre, and liquid essence. The last is easily strained off, and the fat separated by blotting paper. It is, when nicely made, a clear amber liquid, of an aromatic meaty smell and taste. About one-fourth of its weight is obtained from ordinary meat. The object of this preparation is, to administer the purely stimulating elements of meat in a separate form. It may be given in cases of weakness and exhaustion after hemorrhage, or indeed from any cause. It may be given as a substitute for brandy in cases of mental depression, grief, and shock. One tablespoonful taken early in the forenoon, with or without a little bit of crust and a mouthful of wine, "gives the brain a something that relieves it from nervousness and vapours." In many cases of dyspepsia it is a capital remedy. In a few words, it is useful whenever nutriment without bulk is required. (Dr. Druitt, p. 316.)

GALVANO-CAUTERIZATION.—In comparing the galvanic cautery with other escharotics we must take into consideration that we cannot substitute it for any of the milder caustics, which act rather as astringents. Again it differs from more powerful caustics such as chloride of zinc, Vienna paste, &c., in its action being limited to the part to which it is applied. This property may be advantageous or the contrary according to circumstances, thus, if we wished to effect any considerable destruction of tissue, as in the treatment of cancer, the use of the galvano-caustic would be contra-indicated. As to convenience of application, the galvanic cautery undoubtedly possesses an advantage over all other escharotics, and more especially over the actual cautery, in that it may be brought into contact with the parts in a perfectly inert condition, a property of great importance when the part affected is situated at the bottom of any of the natural cavities or canals. (Dr. T. J. Walker, p. 298.)

POISONING BY STRYCHNIA.—Get the patient under the influence of chloroform, and if the spasms return repeat the process again and again for hours if necessary—occasionally giving a dose of opium, compound spirit of ammonia, and camphor. (Mr. W. Travers, 'Lancet,' October 12, 1861, p. 257.)

A COMMENTARY ON MIDWIFERY,

AND THE

DISEASES OF WOMEN AND CHILDREN FOR THE LAST HALF-YEAR.

The Medical writings of the British Isles and America have contained an abundance of most interesting matter within the last six months, which we have endeavoured to *winnow* with as much care as possible. In the *Synopsis of Medicine*, at the commencement of the volume, the reader will find most of the valuable facts which bear on the treatment of Diseases published during the past half-year; but, in the following Commentary, we have dwelt more at large on some of the more important questions relating to Midwifery and the Diseases of Women and Children, which the reader will find in still more detail in the body of the work.

The various methods of inducing Premature Labour are still the subject of discussion. We have never yet failed to bring on labour by simply introducing a male silver catheter, and after gently moving it round the inside of the os uteri so as to detach some portion of the membrane, quietly pushing it through the amnion and evacuating the liquor amnii. Perhaps Dr. Simpson's plan is better, though we think that the amnion is more frequently ruptured than is suspected in his operation, and will then only just be the process which we generally adopt. He introduces a flexible male catheter with its stilet, and after withdrawing the stilet, allows the flexible portion to remain in the womb till labour commences, which it almost invariably does in about twelve hours; whereas, if the amnion be ruptured the process is delayed for other ten or twelve hours. Both ways are safe enough generally to the mother, and we have seldom or never found the child injured, unless the pains became incessant and *uninterrupted*, in the same way as by giving a too powerful dose of ergot of rye, whilst the downward progress of the child was arrested by an unyielding os uteri. There must be one evil however in Dr. Simpson's method, which is not alluded to. We all know that in withdrawing the wire stilet from a male flexible catheter, it cannot be done without pulling down the point very considerably. May not this process sometimes be injurious in a thin womb, that is, supposing we introduce the instrument in its usual catheter form? If it be introduced nearly straight, this objection does not hold. We think, however, that this ought to be remembered, and if you introduce this flexible catheter with its stilet in it, you had better do it with the instrument *as straight as you can well introduce it.*

It seems generally to be acknowledged that it is better to have a dilated os uteri before puncturing or separating the membranes, when we can easily accomplish this dilatation. There are some cases where we cannot do so readily. Few cases, however, will resist the Dilators of the Os Uteri which are now being used. Some practitioners puncture the membranes whenever they can reach them with the finger, the catheter, or any other instrument; some tap the membranes as remotely from the os as possible; some separate the membranes simply from the os and cervix with the finger; some place a flexible catheter in the womb and leave it there; and some use warm or cold water injections, either upon the os or within the womb itself. All these methods assume that it is not well to excite labour pains till the parts are dilated enough to admit the easy expulsion of the child, the same as in nat-

ural labour, and so far they are praiseworthy. A safe method of dilating the os is that recommended by Dr. Barnes, who introduces a Caoutchouc Dilator into the orifice, and gradually distends it with water. Dr. Keiller some time ago recommended the same thing, but used air instead of water; and dilating instruments introduced one over the other, as by Mr. Wakley's instruments for stricture, were also some time ago recommended; but we have much more power and command over water than over air, and Dr. Barnes's instrument seems to us excellent. You may introduce it empty just into the os and cervix, and then forcibly but gradually dilate with water. When the parts are ready, you may safely puncture the amnion or not, as you see necessary. It is certainly better to dilate the parts with a soft water-bag resembling the process of nature herself before encouraging the uterus to contract forcibly. We think this same dilating process might be applied to some of the rigid os uteri cases in natural labour; it would save the poor woman endless hours of misery in some cases, and even the life of the child. Instances occur in which the os will not be dilated—it is too rigid; in these cases the os may be incised with safety. Some of our best writers recommend this; and we have occasionally met with cases which have ended favourably where the rigid and unyielding os has been lacerated by the uterine pains alone forcing the head through, so that these parts will bear greater ill-usage, in cases of necessity, than we have hitherto suspected. We must acknowledge, however, that for a long period in our own practice, we have never found any ill effects from simply puncturing the membranes and leaving the rest of the case to nature. A more simple, and almost equally successful practice, is to inject tepid water into the cervix uteri by means of a gum elastic catheter and a Higginson syringe, as used by Dr. Priestley. Labour will generally come on in twelve hours. In reading the discussions on this subject we are struck with what we consider the erroneous explanation of Dr. Hall Davis, respecting the cause of asphyxia of the child, in such cases. This talented physician says that the child is asphyxiated *from pressure exerted on its body* by the womb in the absence of the liquor amnii. This we think an error. It is asphyxiated by the long-continued and uninterrupted action of the fibres of the womb so compressing the vessels of the womb itself that the circulation of the mother is no longer of any use to the child, and the placenta might as well have been separated. This is clear enough. It is an explanation which we were the first to give of this interesting fact nearly twenty years ago, and which we have had repeatedly confirmed since.

One of the most interesting and important questions to decide at the bedside of a woman in labour, when the Placenta is presenting and Flooding exists to a dangerous degree, is how to stop this hemorrhage, and whether to remove as much of the placenta from the os uteri as the finger can reach, or to deliver by turning. A great deal depends on circumstances in such a case, but as a general rule, we should say that when the os uteri is sufficiently dilated to admit the hand, you had better turn the child at once and deliver. This is an easy process at this stage of labour, the hemorrhage is arrested with certainty, and the life of the child is saved; so that all the results which are required are accomplished with comparative safety and certainty—very little is left to chance. But it often happens that the os uteri is not enough dilated to admit the hand, and is not even capable of being easily dilated artificially, the parts are too rigid. What is to be done now?—the woman is fearfully flooding. If you pursue the usual treatment, you will now rupture the membranes and most effectually plug the vagina. The os uteri will then gradually dilate, whilst the plug prevents all hemorrhage externally, although this may go on internally till the coagulum arrives at or a little above the level of the bleeding orifices when, we maintain, the flooding will be again arrested and give time for the more complete dilatation of the os, so as to admit of the hand to turn. This process likewise does not sacrifice the life of the child, and is generally safe to the mother, if done adroitly. But another and easier process has often been adopted of late years, and recommended by some of our best practitioners, viz., separa-

ting as much of the placenta from the internal os uteri as can be reached by one or two fingers, or by a small portion of the hand. Of course it will be safest for the child to separate as small a portion of the placenta as we can, as the total separation would almost certainly take its life if the labour were at all prolonged: so much however must be detached as will arrest the bleeding. Dr. Barnes is one who has most earnestly recommended the practice. He insists however on one thing which is necessary in order to make the practice successful, viz., *contraction of the womb*. We have therefore especially two things to do in this new practice—to detach the placenta partially, and induce contraction of the womb. To separate the placenta simply will not be sufficient, we must also excite the contraction of the womb, or the operation will fail. To do this we should not be satisfied with simply giving a dose of secale, but we should first rupture the membranes if we can reach them, and then effectually plug the vagina, as in the old practice. The child's head will then soon be pressed forcibly upon the separated placenta, so as to prevent bleeding internally, and the plug in the vagina, assisted probably by the partially-extruded placenta, will prevent flooding downwards, so that the danger will be completely averted and the child delivered naturally. It is clear that contraction of the womb is necessary to stop the flooding, as the head would remain at some distance above the bleeding orifices and would not thus act as a compress unless forced down by the contraction of the womb: and therefore we think that it is not sufficiently insisted upon to rupture the membranes and plug the vagina effectually, as well as to give a dose of secale. To rupture the membranes may be generally advised, but it is often omitted to advise also to apply the plug. When the os is obstinately rigid, Dr. Barnes recommends the introduction of a caoutchouc dilator, in the form of a bag, which is slipped empty into the part and dilated with air or water. We think, however, that nature herself is the best dilator, and when we have effectually plugged the vagina we may safely wait for the natural dilatation, as we can almost *defy* the bleeding from proceeding to excess after the vagina has been completely plugged with rags. Occasionally we think the fingers of the practitioner may be usefully employed to assist in the dilatation, the plug however to be again used as soon as the dilating operation is done with, if the hemorrhage continue. In cases, then, of Placental Presentation, when the os uteri is too rigid to admit the hand, and when flooding is going on to excess, we would recommend the following practice:—1st. Introduce one or two fingers and separate as much of the placenta as may be around or near the os. 2nd. Rupture the membranes, if possible, either with the finger or the catheter. 3rd. If flooding keep off a little, take this opportunity of artificially dilating the os uteri, either with the fingers or with Dr. Barnes' dilator. 4th. Take a long bit of soft rag, a foot or more long, grease it well with oil or lard, and push it well up to the os, keeping hold of the last portion with the left hand: with the right hand push up the vagina smaller bits of rag till the passage is completely plugged. The left hand has still hold of the long bit first introduced, which should now be bent backwards over the perineum. The object of this is, that at any moment, without introducing the fingers into the vagina, the long bit can be withdrawn, and with it all the rest come away. The woman may want to make water in the night and cannot bear to wait for the practitioner, she can therefore withdraw the plug herself, under certain restrictions, which she could not do if used as commonly directed. Moreover, the practitioner, in some measure, can keep the fingers of his right hand clear of the offensive discharge by using the left. 5th. If the os begin to lose its rigidity (but not before, for fear of injuring the child,) give a good dose of secale cornutum, and do not trust to ready-made preparations, but make the decoction yourself in a tea-cup, on the top rib of the fire-grate. We are aware that some practitioners may advise the secale to be given as soon as the membranes are ruptured; but we would caution such to observe the rule which we laid down above twenty years ago, viz., not to cause *incessant* and *uninterrupted* contraction of the womb by secale with a rigid os uteri. We have often been amused to read the opinions of some of our best

practitioners as to the cause of the child often being born dead after a powerful dose of secale. The reason is simple enough: the child is supplied with oxygen from the blood of the mother; but, if the uterus be kept in a state of incessant and uninterrupted contraction by secale, little or no blood can circulate in the womb, and hence the child is simply asphyxiated; and for the same reason, hemorrhage is often materially checked, simply by these incessant contractions, without any other artificial assistance.

In the discussion on Dr. Uvedale West's paper on the Ergot of Rye, at the Obstetrical Society, the opinions expressed both by Dr. Tyler Smith and Dr. Hall Davis are not sufficiently explicit on the action of this medicine. Dr. Tyler Smith asserts that "it can never be given safely, except under circumstances in which the forceps are applicable." This means that the child is low down, and the uterus is unable to complete the labour without help. This is a good rule as far as it goes, but it would in some cases, be dangerous. When the womb has ceased to act energetically, it would require a very great dose of ergot to re-commence the action; it is better, therefore, to give the medicine as soon as the power of the womb is felt to flag, and when there is no obstruction to prevent the descent of the child. If the uterus be stimulated to incessant action without moving the child downwards, asphyxia will almost certainly be produced; on the other hand, if the os uteri be well dilated, the parts relaxed, and such a moderate dose of ergot be given as will only moderately stimulate the womb, and allow intervals of rest, there is little or no danger to the child. We have watched its effects in hundreds, we may almost venture to say in a thousand cases, and never knew the child asphyxiated when there were *intervals* in the parturient action; but when the womb had not time given to it to have its blood replenished, the danger to the child is great. Dr. Hall Davis's opinion that *pressure on the body of the child* may assist or even cause its death, seems to us to be incorrect: the child will bear the most violent compression that the womb can exert, without danger, provided this pressure be not incessant; and we maintain that it is on the principles we have already laid down that the most satisfactory explanation can be given.

It is almost unfair in modern writers not to give due credit to the person who seems to have first recommended and carried out the practice of Separating the Placenta with the finger from the os uteri in cases of placental presentation. So long ago as 1822, Mr. Kinder Wood, of Manchester, first noticed that in cases of this kind when the placenta spontaneously separated itself, the flooding ceased. He, therefore, imitated nature in this process, and whenever he could do so, he introduced his finger and wiped it round the internal os uteri, separating the placenta from the part; this generally stopped the flooding. Dr. Clay, who has strenuously stood up for the rights of his able friend and late master, does not, however, lay so much emphasis on some other points mentioned in the previous part of this article, viz., rupturing the membranes and plugging the vagina; but we think it very likely that these points were not forgotten by Mr. Wood, and would certainly not be omitted by such a man as Dr. Clay. The main point is the separation of the placenta with the finger when the contracted os uteri would admit of no further manipulation, and this practice seems due to Mr. Kinder Wood.

The greatest *bore* to both patient and medical attendant, in a case of labour, is the long time taken up in the Dilatation of a Rigid Os Uteri in a first labour. He would be a benefactor to both women and doctors who discovered a safe method of shortening the process. Sometime ago we pointed out that chloroform had an excellent effect in accomplishing this object, and we continue to think very highly of it, as directed to be used by us in a former volume. Dr. B. Fordyce Barker suggests that the use of Belladonna for two or three weeks previous to labour will so relax the parts, that the first stage of labour will be shortened; but we should be afraid that belladonna would relax also the whole uterus, and thus weaken its efforts during labour. The effects of chloroform are more evanescent. Chloroform certainly for a short time checks the uterine contractions, but its effects soon pass off, and these con-

tractions are soon resumed, so that you may relax the os uteri beautifully; and when you have accomplished your purpose you can leave it off, and allow the womb to recommence its efforts. But we suspect that you cannot command the belladonna, which has been given for weeks, to cease its relaxing powers just when you like: these effects will be continued all through the labour more or less. Dr. Barker's treatment, however, is interesting. He watched its effects in 147 cases, commencing the belladonna a week or two before labour with a minimum dose, and gradually increasing it till the throat became dry, and slight giddiness and dimness of sight came on. He generally began with a quarter of a grain of the extract of belladonna three times a day, increasing it till its effects became evident.

What is to be done in a case of labour, when the Head makes no progress, and yet there seems to be not sufficient contraction of the parts to account for the difficulty? You feel the head, you feel that there is room enough, and the pains are strong enough, and yet no progress is made. In an interesting case of Mr. Jardine Murray, of Brighton, such an occurrence took place. On examining carefully, it was found that the elbow was coming down with the head, and the hand was thrown round to the back of the child's head, so that the elbow pressing on some part of the brim was the cause of the obstruction. This case is certainly rare. Mr. Murray proceeded to deliver by turning, but Dr. Eastlake, though in the most friendly tone, dissents from this practice, and advocates that *when the child is dead* the delivery should be accomplished by craniotomy; and when alive, by the forceps, according to the Dublin practice. This becomes an interesting question, and we humbly maintain that it will easily be settled by proceeding in the following way:—It is almost certain that in all such cases the liquor amnii will have been evacuated before the presentation and difficulty have been ascertained, because the truth cannot be properly ascertained till the hand has been carried a considerable distance into the uterus. It is also certain that here we have not an ordinary case of *impaction* low down in the pelvic cavity, but a simple obstruction at the *brim*, with plenty of room. There will consequently be comparatively but little difficulty in introducing the hand and turning, during the state of anæsthesia from chloroform. This was the opinion of Mr. Murray, and we agree with him entirely. Dr. Eastlake thinks that as the child was dead craniotomy might have been preferable, but we differ from him. Craniotomy, when the child's head was at the brim and even above it, would have been quite as dangerous, or more so, to the mother, as skilfully turning the child, and moreover might not have dislodged the elbow completely from its position. The best practice seems to us, therefore, to take the opportunity, while the hand is in the womb ascertaining the difficulty, to turn the child if possible, and if this cannot be done, then it will be time enough to deliver either by the forceps or by craniotomy. In such a case the elbow ought of course to be dislodged from its resting-place, if possible, before anything else is done.

An interesting case of Spontaneous Evolution is related by Mr. Heslop, of Haverfordwest. The head and arm were descending together, and had actually become impacted in the pelvis, when a sudden and violent contraction of the uterus completely turned the child over and brought down the breech and legs; the labour was completed immediately. There could be no deception here; one arm was at first protruded externally, and the head presenting above it; suddenly the legs and breech presented themselves, to the astonishment of Mr. Heslop. This shows how wonderfully the powers of the womb will sometimes rectify matters, although these powers are not often to be trusted on such occasions. It shows also that some practitioners, who have lately attempted to substitute external manipulation for the internal process of turning the child in utero, may have been materially assisted in their efforts by the peculiar direction which they have given to the contractions of the womb. Instead of introducing the hand and arm, and bring down one or both legs, they have pushed up the presenting arm and shoulder with one hand whilst with the other, applied to the abdomen externally, they have attempted as it were to turn the child over; or so manipulated as to give a

fresh direction to the efforts of the womb. But although the practice may have been occasionally successful, we should never venture to trust to it if we could by any means introduce the hand and turn. It must be confessed however, that when the shoulder and arm of the child are impacted low down in the pelvis—*jammed up* as it were in the pelvic bones—it is a difficult matter to get down the leg. We have occasionally met with some most awkward cases in our own practice, but have always hitherto succeeded by following the practice of our eminent and venerable colleague, Mr. Smith of Leeds, viz., pushing up the end of a silk handkerchief, or a strong bit of linen, with one or two fingers or part of the hand, so as to get it round the back of the knee-joint. By perseverance you may *loop* this round the flexed joint, and work it downwards, so as to form a powerful loop to pull at; or you may get hold of a foot and *noose* it securely. Now you can pull with the handkerchief, whilst with the fingers of the other hand you can push the child upwards. The womb will sometimes seem to assist in the operation, instead of resisting the process.

It is rather curious to notice the difference of opinion, amongst even our leading practitioners, on the method and uses of Supporting the Perineum during labour. Some say, do not support it at all but let nature do the whole work; others say, support it well with the palm of the hand, and press the head forwards. But all these celebrated writers and eminent practitioners omit all allusion to the shoulder of the child, which is a frequent cause of rupture of the perineum for the following reason: The practitioner is bending the whole of his attention to the passage of the head; he supports the perineum very carefully, and gently presses the head forwards towards the pubic arch, so as to relieve the great strain on the perineum; but as soon as the head passes he gives up all attention to the perineum, and forgets entirely that it has to be stretched again by the shoulder. Moreover, if he did remember this circumstance, he cannot follow the edge of the stretched perineum with the palm of his hand from the cleft of the neck, as it gradually becomes more and more stretched in passing over the point of the shoulder. The consequence is, that although he has prevented the perineum from being ruptured during the passage of the head, he allows it to be ruptured by the passage of the shoulder. To support the perineum during the passage of the head and shoulders, and to assist the head to take that curve towards the pubic arch which nature points out, is so useful, that we shall not attempt to answer the arguments against the practice, but simply give our own views. When the head has advanced sufficiently low the occiput is placed in the sub-pubic arch, and there becomes nearly stationary till the rest of the head has cleared the perineum. It is evident that as the occiput remains in one place, and the rest of the head advances, it must take a curve something like putting the long finger of a clock *backwards* from twenty minutes *to* 1, to twenty minutes *past* 12 o'clock; twenty minutes to the hour represents the edge of a perineum at the full stretch, and twenty minutes past the hour represents the point to which the forehead of the child is advancing. To support the perineum properly, therefore, you must not push it upwards so as to retard the passage of the head, but your object is to steadily push the head forwards, which you are obliged to do sometimes, by having the hand *much further back* than the edge of the perineum. Your object in short, is to make the head take that *curve* which it ought to do; or as it were, to carry the long finger of the clock backwards from twenty minutes to 1 to twenty minutes past 12. Surely when we see the obvious wish of Nature, we ought to assist her to accomplish it when we can do so, and not leave her to her unaided powers, when perhaps she is nearly exhausted. But we think that to support the perineum the palm of the hand is not so good as the back of the knuckles of the left hand, well wrapt up in a soft napkin. In the usual obstetric position, if you support the perineum with the palm the fingers are upwards towards the sacrum, and you can only *support* the part if you wish to make the head make its natural curve you will have to *pull* it forwards with the palm of the hand. You cannot well *push* it forwards but in *pulling* the head forwards you soon lose the edge of the perineum as it slips

over the head, and you quite lose all feel of it as it gets into the cleft of the child's neck and again becomes stretched over the shoulder. Now here is the fault which many medical men commit. But if you fold the hand in a napkin and apply the *flattened knuckles* across the perinenm, you can feel accurately with the disengaged thumb where the perineum is, and what degree of tension is now existing. This will regulate your own efforts. You can in this way support the perineum accurately up to the very edge, make the head obey its natural curve, and when the head has passed you can follow the edge of the perineum with the thumb, feel it again becoming stretched by the shoulder, and again be ready to give it support as it stretches over the shoulder; in no other way can you follow it accurately,

It may be said that in first labours, the perineum is generally more or less ruptured. When we consider how essential it is for a woman to have a good perineum in order to support the parts above, it becomes the duty of the medical attendant to see that these ruptured parts are approximated and healed immediately after labour, now that the wire needle is such a safe and easy remedy. It may be years afterwards that the woman will suffer from this injury, the parts becoming more distended or stretched. If practitioners will but heal these ruptured parts at once, we shall not hear of such numerous cases of prolapsions of the womb in future. The operation is so simple, that it will almost be inexcusable in the practitioner avoiding the trouble; a couple of needle sutures, a little smooth wood, and a few pierced duck-shot, will be sufficient apparatus. The edges of the wound are easily approximated and kept in position; they unite most readily, and the parts are as strong as before.

In the use of the Forceps, it is curious to notice in some cases how easily the face may be made to turn into the hollow of the sacrum by twisting it to the right side of the pelvis, and in other cases, how this is reversed. When the face is a good deal to the left side, you will find that when the forceps are properly applied over the ears, it is actually sometimes easier to make the face take a circuitous turn round to the right of the mother into the hollow of the pelvis, than to the left of the mother over the rectum; consequently, it is a good rule, wherever the face is placed, to try to make it turn to the right of the mother, unless, indeed, it is already so much to the left that it seems an easy matter to carry it over the rectum in that direction into the hollow of the sacrum. It would seem as if the body of the child often descended into the pelvic cavity with the face a little twisted over one shoulder, that is, with the left shoulder to the right acetabulum, and its head twisted a little over this shoulder, because, in some cases, when even the face is decidedly looking to the left you cannot turn it over the rectum, but can easily turn it in a circuitous way to the right of the mother. Some writers, indeed, suppose that this is the turn that the child takes spontaneously in natural labour, when the face has entered the pelvis in a direction towards the rectum. This practice is well illustrated in a case published by Dr. Hamilton, of Falkirk. The face presented to the left side and the labour was tedious, making it advisable to use the forceps; the face could easily be made to turn as far as the rectum, but no further; it was, however, easily turned towards the right synchondrosis, thus making a sweep round the pelvis, and causing the labour to end easily. As a rule, therefore, it may be well in turning the face into the hollow of the sacrum, to make it do so by turning it round to the right of the mother, so as to avoid passing over the rectum; this, however, cannot always be done.

It happens in three or four forceps cases out of every hundred, that this instrument is too short. The Head has not only been elongated by long-continued pressure in its passage downwards, but its shape has been altered, so that the ordinary short forceps is not only too short, but will not fit the head. Dr. Graily Hewitt draws our attention to an instance of this kind, and has had made a forceps adapted to such cases. The blade is eight inches long (instead of 6½ or 7), the curve that of a circle of 14 inches (instead of 10 or 11 inches); in other respects it is like the short forceps. We are glad to see Dr. Barnes recommending the forceps of Mr. Roberton,

of Manchester, and believe it to be one of the best kinds of forceps used in this country. It is well known what a degree of pressure on the perineum is liable to be exerted by most forceps, unless the practitioner take the greatest care and avoid pressing the instrument backwards. Of Mr. Roberton's forceps, Dr. Barnes speaks in the following terms:—"For some years he had used exclusively the forceps of Mr. Roberton, of Manchester. This instrument was admirably adapted to all the ordinary emergencies, whether the head lay at the brim, in the cavity, or near the outlet. The pelvic curve, the length of the blades, and the length of the shank, kept the perineum free from any strain whatever, beyond what was incidental to the passage of the head itself. The instrument added nothing to the danger of the soft parts." Now that the application of the forceps seems to be a more frequent practice than it used to be, it is necessary that we should make more generally known such opinions and such instruments as these.

Medical practitioners in general are not yet sufficiently alive to the danger of going from surgical operations, sloughing sores, and post-mortem examinations, to attend women in labour. If they will read Mr. Nunneley's treatise 'On Erysipelas,' they will find this subject handled with great ability. This author points out how intimately connected Puerperal Fever is with erysipelas. We think he might have safely extended his remarks to all bad surgical cases where the fingers of the practitioner may be the medium of contagion to the woman; indeed, in some of the cases detailed by Mr. Nunneley, we can trace the puerperal fever conveyed to the lying-in woman, rather from the sloughing sore and gangrenous wound than from simple erysipelas. For example, take the following quotation from Mr. Nunneley's book:

"I shall first mention, as instances of the probable inducement of puerperal fever from erysipelas, some facts related by Dr. Paley, of Ripon, but formerly of Halifax, and which came under his own immediate observation. A man who resided near Halifax some years since was affected with a most severe attack of erysipelas, which rapidly terminated in gangrene. Whilst the surgeon was in the act of dressing the sloughing sore, he was called off to a female in labour, to whom he immediately went; this patient, and five others in succession, died of well marked puerperal fever, although the disease was not known in the neighbourhood, had not been for many years, nor had any other practitioners any cases. Very recently a man in the neighbourhood of Ripon had a similar affection of the scrotum, and immediately afterwards puerperal fever occurred in the practice of the medical man who had the care and dressing of this patient.

"Mr. Blagden has related the case of one of the midwives of the British Lying-in Hospital, who, a few days after attending in labour a woman who died of extensive peritoneal inflammation, was seized with a severe attack of erysipelas of the face. 'On the 16th of March, 1831, a medical practitioner, who resides in a populous parish in the outskirts of London, examined the body of a woman who had died a few days after delivery, from inflammation of the peritoneal coat of the uterus. On the morning of the 17th of March he was called to attend a private patient in labour, who was safely delivered on the same day. On the 19th she was attacked with the worst symptoms of uterine phlebitis, severe rigors, great disturbance of the cerebral functions, rapid feeble pulse, with acute pain in the hypogastrium, and peculiar sallow colour of the whole surface of the body. She died on the fourth day after the attack, the 22nd of March, and between this period and the 6th of April this practitioner attended two other patients, both of whom were attacked with the same disease in a malignant form, and fell victims to it." (Nunneley on Erysipelas, p. 73.)

Now, in the cases related by Dr. Paley, the surgeon was called to the woman in labour *while in the act of dressing the sloughing sore*, and this woman and five others in succession fell victims to his practice. Mr. Nunneley goes on to relate numerous other cases much to the same effect, and we draw attention to this subject, owing to the carelessness with which surgeons often regard it.

It is too much forgotten or unknown how often the fingers may be smeared with what may seem innocent fluid, but which is highly poisonous, and may in a very small quantity prove destructive to a woman when placed in contact with the denuded and sore surfaces of the vagina or cervix uteri. You would not suppose that the serum found in the peritoneal cavity, for example, would do much harm if a minute portion were carried on the practitioner's finger, or under his nail, into the vagina of a woman; but this might be a fatal piece of ignorance in the medical attendant, and we again warn our medical brethren not to attend women in labour when they are in attendance on bad surgical cases, or examining post-mortem subjects; they will inevitably cause puerperal fever of a fatal character to exist in their practices from time to time. If the reader will turn to Mr. Spencer Wells' case of ovariotomy in the present volume, he will find a remark which fully bears out what we have just expressed. In a case of ovariotomy he had a small scratch on his finger, made during the operation, which was inoculated only with the serum of the peritoneal cavity, but he suffered severely for two days, had severe rigors, lasting several hours, with intense headache, relieved by vomiting and copious perspiration, which lasted about eighteen hours. He further says,—"all experience has proved inoculation of the fluids of those dying of puerperal peritonitis, erysipelas, &c., to be far more dangerous when the bodies have been fresh than when putrefaction has commenced." Now we ask any candid surgeon, after reading this opinion, whether he thinks it proper and just to a woman, to be operating in a case of hernia, or any other such capital operation, and go to attend her in labour on the same day—nay, perhaps, to hurry from the operating room or the post-mortem examination direct to the labour. Is he not wilfully risking the life of his patient, after he has been informed by the best authors that such a practice is dangerous in the extreme. Dr. Simpson, of Edinburgh, in one of his lectures, uses the following words:—"A gentleman who was formerly surgeon to a very large hospital, and also to extensive practice in midwifery, informs me that during the period of his surgical superintendence of the hospital, and when consequently often touching the discharges from all kinds of wounds and breaches of surface, puerperal fever was from time to time common in his private practice, and at the same time he saw many of his hospital surgical patients die with similar symptoms. Since giving up the surgical charge of the hospital, the occurrence of puerperal fever has ceased in his private midwifery practice. (Edinburgh Medical Monthly Journal, Nov., 1850, p. 429.) Such facts require no comment, and we leave it to operating surgeons to do as their consciences direct them.

If we look over the older treatises on Puerperal Convulsions, we are struck with the improvement in the modern treatment of these formidable attacks. Formerly, the patient was bled to a fearful extent, sometimes to the amount of sixty ounces. The same effect on the system is now rapidly accomplished by the simple exhibition of chloroform. In a case related by Dr. F. W. Wilson, it is interesting to see the remarkable effects of this remedy: the case was apparently desperate, the fits were occurring every quarter of an hour, labour had commenced, and the os was dilatable; chloroform was administered, and in a minute or two its effects were visible, and in ten minutes the fits ceased. The next thing to be done was to deliver the woman by the forceps; this was done well; and, except for another convulsion three hours afterwards, which was again relieved by the chloroform, the case ended satisfactorily. We would suggest that in such a case as this, in which labour was commencing and the os dilatable, not only should the chloroform be administered, but the woman should be delivered by turning the child the moment the hand can be easily introduced through the os uteri. It is of no use waiting till the child have descended low enough to use the forceps—turning is actually in these cases a more easy and a safer operation than the use of the forceps. We would also suggest that in all suspicious cases, the urine should be examined for albumen, and if this be present, the practitioner must be prepared for such an attack. Albumen will often be present in urine made *before* but not *after* an attack of puerperal convulsions.

Sometimes a dreadful case of Puerperal Convulsions will occur when the os uteri is rigid and not dilated; you will then be tempted to bleed largely, as was Dr. Robert Harper, of Holbeach; but we would suggest that the chloroform be tried first, as it will often not only check the convulsions, but soften and relax the os uteri so as to enable us to turn the child. We may, in fact, use the chloroform whenever the convulsions come on, whether before, during, or after, the labour, with great success.

We cannot have a much better instance of the efficacy of chloroform in Puerperal Convulsions than the one related by Dr. Page, of Landport. Before labour, soon after labour, and sixteen hours after labour, did these alarming convulsions occur, and in every instance did the chloroform succeed. It has been doubted whether chloroform be a safe remedy in the apoplectic kind of puerperal convulsions, where coma with stertorous breathing are present, and some even recommend that *bleeding* should precede its use. From this opinion we differ, and consider that we may use the chloroform in such cases with even more safety and success than in cases when, from any cause, the patient has been exhausted. Chloroform, in fact, is always safer when there is plenty of power in the system, than when there is little; it is a powerful sedative, and we think it much safer and more efficacious to be used without, and instead of, bleeding, than with it. Dr. Page's case is an excellent one, but perhaps he would have done even better without the bleeding.

In a case of Morbid Poisoning after Parturition, how does the poison get into the general circulation—is it absorbed from without, taken from without by the veins of the womb, or is it generated within the vein itself? It is known that pus may be injected into a vein without causing those violent symptoms which we might have anticipated; but when a vein is poisoned by some morbid poison applied to it, and immediately forms a fibrinous plug, and when this plug of unhealthy fibrin decomposes and generates pus, then the smallest quantity,—a drop or two,—will give rise to the most dangerous symptoms of puerperal fever, and at other times, to deposits of pus in various parts of the system. The pus, then, is supposed to be secreted by the poisoned vein, at the part originally affected. Mr. Henry Lee asserts, we believe with truth, that few blood-poisons are more deleterious than decomposed fibrin, and Dr. Richardson does not believe in the absorption of pus from without, but from within the vein, and that in such like cases there is a great increase of fibrin in the blood.

The world at large talks of the improvements of the age, yet it knows but little of the progress of medicine, some of the greatest improvements in which are the simplest, such as the substitution of Metallic Sutures for silk or hempen ones. It is quite singular to read some of the cases of wounds in which a bullet had lodged in some deep parts for years without any particular ill effects; but in the 'Medical Times and Gazette,' for last August, there is related a case where a small bullet had been lodged in the heart itself for some time! It is common enough to hear of bullets being lodged in other parts, but we never before heard of one having been lodged in the heart. This only shows how little irritating a metallic body is to living structures compared with silk or hemp, which would soon have caused purulent inflammation. It is equally singular to notice the length of time which some good idea takes to be brought into practical operation. Mr. Gossett cured a vesico-vaginal fistula in London, by the use of a gold-wire suture in 1834, and Mr. Wells heard Mr. Morgan, at Guy's Hospital lecture on the uses of platinum wire as a suture in the year 1840, and yet it has taken fifteen or twenty years to bring these ideas forward. We may chiefly thank Dr. Simpson for the introduction of metallic sutures.

We must draw the reader's attention to the paper of Mr. Collis, of the Meath Hospital, on the subject of Vesico-Vaginal Fistula. If other surgeons can accomplish so easily as he does what he recommends in this operation, it will be considerably simplified. Why should not the same principle be carried out in numerous other operations of a similar kind, such as cleft palate, hare-lip, and ruptured perineum? Instead of simply paring the

edges of a vesico-vaginal fistula like some operators, or bevelling them off like others, he *splits* them—separates the mucous membrane of the two surfaces, and exposes the raw intermediate structure so as to be accurately approximated. The mucous edges will thus be separated and a little everted, as the wounded and split edges are brought together by the sutures. The size of the fistula need not be enlarged, as there is no paring away of the edges. Good thread will answer as ligature; Liston's needles fixed in long handles, with the eye near the point, are the best to carry the thread through. Other things are carefully described by Mr. Collis in his paper, to which we would refer the reader. By these means we think this delicate operation may be simplified; but of course a great deal will depend on the nature of the edges of the fistula. Mr. Collis, we suspect, will sometimes find the edges too thin to be thus *split*, or too thick and hard, so that his operation will be adapted only to those cases in which the parts can be split and divided as he recommends; whereas, the paring or bevelling processes may be applicable to all cases. If the edges are very thick and callous, we suspect that it would be no easy matter to *split* and *evert* them, so as to bring the raw surfaces together. Mr. Collis, however, thinks that his operation is adapted to almost every case, and to many which could not be subjected to the older operations with any chance of success.

We have sometimes thought that some of the dreadful cases of Prolapsion of the Womb might be materially relieved, not simply by the now common operation of paring the edges of the perineum, and uniting them, thus reducing the size of the external orifice, but by also reducing the size of the relaxed and dilated vagina itself, which was one of the original causes of the evil. To pare off some portion of the vagina either circularly or longitudinally, and to heal this, would produce a cicatrix: a cicatrix produced either in this artificial way or by an accidental rupture would be very objectionable in a woman who was likely to have more children, as it is well known that it would be torn during the next labour, and would very likely lacerate some of the contiguous structures, which the cicatrix had most probably got hold of; but in women past the period of child-bearing this objection would not exist, and we may safely diminish the diameter of the vagina by snipping off one or more portions of the internal membrane, and uniting the separated edges by the wire ligature, the same as in other parts. This has been done successfully by Dr. Roberts, of Manchester, in a woman aged 49, and therefore a case for such an operation. In a severe case of procidentia he "dissected off a triangular slip of the mucous membrane covering the posterior surface of the uterus, and brought the edges of the wound together by silver wire; removed a similar piece from the anterior surface and drew the edges together in the same manner." He then pared the edges of the perineum in the usual way and united them by means of hare-lip pins, in the manner recommended by Mr. Spencer Wells. Of course the closure of the external orifice would prevent the state of the vagina being accurately examined. The cicatrices of the mucous membrane on the back and front of the womb could not be properly examined, and we regret that this one operation was not done at two different times. We would recommend in future that the wounds of the mucous membrane be cured first, in order to see the amount of good from them alone, and afterwards that the perineum be proceeded with in the usual way.

Some years ago it will be remembered how fashionable it was to apply Caustics to the os uteri, to disperse enlargements, and to heal what were called ulcers. This practice has now fallen almost into total disuse; we do not, at any rate, hear much about it, but some of the ill consequences are now and then heard of. Dr. Whitehead, of Manchester, says, that in many cases the reduction of bulk in the os and cervix was followed by total closure of the orifice of the womb; so complete was this closure, that he had to open it with a lancet and keep it open artificially till the healing was complete.

Dysmenorrhœa, when owing to constriction of the cervix uteri, is often a more troublesome, and generally a more painful disease, than most of the

diseases of menstruation; and although we now understand its pathology better than we did, we are still obliged in many cases to have recourse to various methods of dilating the os and cervix uteri. Dr. Coghlan uses a neat kind of bistoury, by means of which he saves much time in dilating this passage. It is only carrying out the usual practice, but every little hint may be useful, and we think this may be so too. The cervix is sometimes so narrow that there is no room for the menstrual secretion to pass; the suffering therefore, almost resembles a kind of miscarriage. The uterus is even excited to a kind of parturient action, and the life of the woman is made wretched; you can hardly introduce the wire or stilet of a catheter up the passage. It is curious to witness the improvement effected in some cases by skillfully dilating the cervical canal; first, try to pass a No. 1 flexible bougie, or a bit of smooth pine wood, or a fine bougie of gutta percha. Dr. Coghlan's instrument consists of a central blunt or probe point, less than a quarter of an inch long, and about as thick as a No. 1 bougie, and proceeding from this are two cutting sides; the whole instrument is very small, and by pushing the pointed probe along the os and cervix, it acts as a guide to the cutting sides, and a slight incision is made safely, accurately, and *laterally;* a fine leaden bougie of the size required is then introduced. This leaden bougie will irritate less than almost any other, and it can be shaped at the sides, so as to correspond to the incisions made by the cutting instrument, and will thus prevent them from contracting. The parts thus remain more or less permanently dilated, and at the next menstrual period the secretion has room to get away, and the relief will often be found to be great; if not, the process must be repeated. There seems to us to be one disadvantage in Dr. Coghlan's instrument, which is that it has no sheath to protect it as it is passed along the vagina. Dr. Simpson's instrument, on the other hand, is introduced into the cervical canal completely protected by its sheath, like a hernia knife; the cutting edge can then be brought from its hiding-place, and *as it is withdrawn*, can *nick* or *cut* the exact part which is required. In all these cases, however, we think that Dr. Coghlan's small leaden cervical pessaries will cause less irritation than pessaries made of wood, sponge, or most other materials, on the same principle as the metallic suture causes less irritation than the common sutures. There is another disadvantage in this instrument of Dr. Coghlan's namely, that it does not begin its incisions as it is *withdrawn*, but as it is pushed forwards. Now, it is often much better to follow Dr. Simpson's plan, which is well accomplished by his *metrotome*, viz., to begin the incision very slightly *at the os internum cervicis*, and as you *withdraw* the instrument to *enlarge the incision downwards*, so as almost to completely divide the lips of the os uteri. It strikes us that, if these two suggestions of Dr. Simpson and Dr. Coghlan were combined, we should gain something, viz., the cutting operation of Dr. Simpson, and the leaden pessary of Dr. Coghlan. One disadvantage of the leaden pessary would be, that it would probably *drop out* when the patient stood up. A gutta-percha pessary might be extemporized equally well, and altered on the spot so as to suit the parts, but would not have the advantage of being metallic. What we have been writing only applies, of course, to those cases which are owing to mechanical obstruction at the os and cervix; but we think that there is more connection than is generally supposed between membranous and mechanical dysmenorrhœa, and therefore this same treatment is not altogether inapplicable to membranous dysmenorrhœa.

It sometimes happens that a young unmarried woman parts with what appears to be a Deciduous Membrane, the result of conception. It is necessary to be very cautious in giving an opinion in such a case, as it is now known, or strongly suspected, that the deciduous membrane of conception is not a new membrane, but only the mucous membrane of the womb thickened, and that it may exist occasionally in a fair degree of development in cases of dysmenorrhœa. We can easily imagine that in some cases the womb of the unmarried female, especially during the menstrual period, may have more blood than usual sent to it, or that the blood which is sent to it,

although in normal quantity, is used up in the production of hypertrophy of the mucous membrane. In such cases we shall have occasionally many of the symptoms of miscarriage, with sometimes a deciduous membrane expelled. Dr. Beatty related a case of this description to the Dublin Pathological Society; his patient parted with one of these membranes every month, for twelve months in succession. It strongly resembled a "decidua vera" and was evidently owing to a process similar to that which produced the true deciduous membrane.

In some cases of Amenorrhœa, when the system has been replenished with a sufficient quantity of blood, but where, from disease as it were, the womb has lost the habit of menstruating, it may be useful to apply a local stimulant, or something to excite the organ. But this must be done with great caution, if done by means of instruments. Dr. Simpson, in some of his cases, has *dry cupped* the interior of the womb. He introduces a tube, resembling in length and size a male catheter, with a large number of thickly-set small orifices, with an exhausting syringe adapted to its outer end. When the instrument has been introduced, the syringe exhausts the air and draws the mucous membrane of the womb up to, and even perhaps a little into, the small holes of the tube, and thus draws a little blood from the congested membrane. This may have to be done several days successively, and eventually it may be the means of restoring the organ to its usual habits. But in other cases it fails, probably because the ovaries require to be stimulated as well as the womb. This may be sometimes accomplished by causing the patient to wear an intra-uterine pessary, made half of copper and half of zinc, and measuring 2½ inches in length. This pessary is fixed on a bulb which is kept outside the os uteri, and has a small hole at its under surface, into which a bougie or catheter can be fixed, so as to facilitate its introduction, all of which is described at page 270. We must acknowledge, however, that we have great doubts as to the permanent good effects of this kind of interference. We may stimulate any organ, and especially any glandular organ, to do extra work, but may we not be interfering too much in the good and gentle conservative powers which nature is generally willing to make use of, if we will but give her time and opportunity? The power of electricity in such cases has been much extolled, and by some disputed; at any rate it is a harmless measure, if not beneficial, and therefore we should prefer it to more violent proceedings. Dr. Althaus extols its powers, both in the form of Faridisation and the continuous current, but not in the frictional form. Faradisation seems to be the best method, and it should be persevered in for some time. Faradisation of the skin of the soles of the feet by means of wire brushes, or of the abdominal surface by means of moistened conductors, or the application of one moistened conductor to the nape of the neck and another over the pubes, seem to be the best mode of using this remedy.

In Ovarian Dropsy it is best not to tap except there is only one cyst, and it is better never to inject with iodine if there are more cysts than one. The injection of iodine has certainly cured some of these cases, but we must remember that cases which have been injected with iodine are believed not to do so well if the cyst has afterwards to be removed. Therefore, we would recommend Dr. Clay's plan, not to inject with iodine if we intend afterwards to recommend the removal of the cyst by operation. But, on the other hand, if you intend to try the iodine, do not be satisfied with one or even two failures, as a third injection will sometimes succeed after two others have failed. Also, remember never to inject too much iodine, as was often done in the early use of this treatment, but be satisfied with about two ounces of the Edinburgh tincture. It is somewhat curious, as Dr. Clay remarks, that this disease seems to attack young unmarried females oftener than older and married subjects, and is worse to treat in these younger subjects, being more solid and more rapid in growth.

In the operation of Ovariotomy it cannot be too much impressed upon us that great danger arises not only from detaching the ovarian cyst from contiguous parts when adherent, but also when the contents of the cyst escape

into the peritoneal cavity; the fluid is sometimes peculiarly irritating, and is supposed to have even some malignant property. Its effects when spread over a large surface of peritoneum must be highly irritating in some cases. Is it right, therefore, in a case of pregnancy accompanied with ovarian dropsy, to attempt the removal of the cyst during gestation? We think this question may be easily answered. It must be a very peculiar and remarkable case which calls for the operation during pregnancy, and even tapping ought never to be performed in such a case, except under some peculiar and extreme circumstances. We should consider the danger of meddling with ovarian cysts during pregnancy so great, except perhaps in some cases tapping them, that we should even prefer bringing on premature labour as the less evil of the two.

It is now well known that Metallic Ligatures cause much less irritation than the common ligatures; bullets even have been known to be imbedded in parts for years without causing great inconvenience as before mentioned. These metallic wires used as ligatures cause adhesive rather than than suppurative inflammation. We should be glad, therefore, to see them used in tying the peduncles of ovarian tumours and ovarian cysts instead of the usual ligatures. It is possible that the time may come when such a peduncle may be tied with a wire ligature which may safely be left in the abdomen without exciting mischief. Mr. Redfern Davies used a silver wire to compress the veins in a case of varicocele, and when he tried to remove it he failed, and therefore, cut off the wire as short as he could. In a week the part was healed with the wire inside the wound, the varicocele being perfectly cured. (See page 141). In many ovarian cases the ligature causes great mischief, and we would suggest that more attempts should be made to use the metallic ligatures.

The treatment of Asphyxia in Infants just born is an important question, and one on which the opinions of medical men are much divided. Some recommend the hot bath, and some a cold surrounding air. Many years ago Dr. Edwards, of Paris, very decidedly spoke against the hot bath, and indeed, we believe against all increase of temperature applied to the surface asserting that this increase of artificial temperature in the child only caused a kind of waste of oxygen, which was required for other purposes. He had found that new-born mammiferous animals die most slowly in water of a temperature of about 60°F., which is nearly cold, and most rapidly in water of blood heat. Dr. Snow explained this fact as follows:—The heat stimulates the capillary circulation, and thus promotes the deoxygenation of the blood in a more rapid manner than when the surface is cool. Thus the oxygen is, as it were, wasted rapidly and uselessly. Moreover, we think that, in some slight degree, the skin must be a respiratory organ. Look, for example, at the reddened oxidized blood in the cheeks of a person who is walking in a cool and brisk wind. The hot bath, therefore, to say the least, is a very doubtful remedy in cases of asphyxia in new born infants, and for our own part we never use it. There was an interesting discussion on this subject lately at one of the meetings of the Medico-Chirurgical Society, after a paper by Dr. Waters. This gentleman proved that the lungs of animals which had been put into hot baths were more congested, firmer, and heavier than in animals not so treated. Both sides of the heart were loaded with blood in those which had been submerged in hot water, and in the lungs the blood was sometimes coagulated, and was, indeed, less fluid generally, and more disposed to coagulate when removed from the vessels. In no instance did the hot bath produce a respiratory act. All these phenomena are directly adverse to the renewal of respiration in asphyxiated infants. *Heat*, in fact, does not excite respiration so much as *cold*. Nature herself excites the first respiratory act by by bringing the child from a hot to a cold medium, and we all know how cold, suddenly applied either by a dash of cold water or air, will stimulate the respiratory act. It is curious sometimes to watch the effect of a single respiration in the infant—how this almost necessitates others, especially when the air of the first inspiration is gently expelled by slow compression of the chest; when the air is thus gently expelled, it is almost cer-

tain to re-enter by the expanding chest causing more or less vacuum. The art of treating asphyxia in the infant is thus simplified. Let it be exposed to a cold air; employ slight friction of the skin; tickle its nose or fauces with a feather; give a gentle slap or two on its buttocks or elsewhere; dash a little cold water on its face or body; and, if these means fail, introduce a small catheter into the trachea, or apply a little clean linen over the mouth, and *very gently* distend the chest with air. Don't do this violently, but extremely gently. You want simply to dilate the respiratory organs, in order to be enabled to squeeze the air out again, and thus to create such a partial vacuum that the air must necessarily rush in again. The fault generally committed is, that this is done *too much*—the practitioner often blows down the trachea of the infant as if he were blowing up a bladder, and he does this *too rapidly* and too continuously. The process differs a little when it has to be used in the adult after drowning. In this case the lungs have to be emptied of bad air *first*, but in the infant no air at all has yet entered. If it were a more ready method, it would no doubt be useful in some of these cases, to use oxygen itself. In our 11th volume, Dr. George Wilson, of Edinburgh, says that oxygen can in a few moments be generated for this purpose. By mixing chlorate of potash with a tenth of its weight of black oxide of manganese, the black oxide of copper, or certain other oxides, and applying heat to the retort by a spirit-lamp, you can obtain 200 cubic inches of oxygen in four minutes. The gas begins to come off in a few seconds after the application of the flame, and literally gushes in a full stream till the whole is evolved. Why is not this simple and inexpensive method more frequently utilised by practitioners?

It is surprising to notice the rapidity with which Hydrocephalus in children sometimes sets in; but the reason is, that the observer has not watched its stealthy preliminary stages. The cerebrum and intellectual faculties in a child, those portions of the brain situated above the cerebral ganglia, are not so necessary to its existence as might be supposed. Provided that the base and lower portions be tolerably healthy, the organic functions will be carried on tolerably well. The upper portions may be considerably unhealthy without those severe symptoms which would at once attract the attention of the medical attendant. Hence, in a child of a tubercular disposition, we ought always to be on our guard against tubercular effusions on any part of the brain, and pay greater attention to head symptoms, which in another kind of case might not be required. As Dr. West says, "whenever the tubercular cachexia is much developed, a very small amount of cerebral disease is quite sufficient to destroy the patient, and the slight symptoms during life will be found to be the expression of a very small amount of local mischief." The correctness of this opinion, however, depends on the part of the brain where the tubercular disease exists. A considerable amount of disease may exist *high up* in the brain, without causing death, but let a small amount of disease attack the *lower parts* where the par vagum on each side arises, and death will not be long in ending the scene. You may often see a child playing about without pain, but every now and then apparently oppressed with something like giddiness or confusion, and perhaps the next day apparently all right; this may go on for weeks. There is tubercular disease in an incipient degree in the meshes of the pia mater on the upper parts of the brain, but the base is pretty free. The diseased parts are not those which give rise to pain, none of the cerebral ganglia are yet much involved, and neither sensation, nor motion, nor even intellect, are yet much affected; but examine the *pulse* repeatedly, and you will find it occasionally irregular, sometimes too slow, sometimes too rapid; the *iris* is also rapidly variable, or too dilated; the stomach is irritable, drowsiness is more frequent, the child in the midst of his play goes and rests his forehead on the sofa or chair. The tubercular affection is rapidly spreading to the base of the brain, and you must be accurate in your diagnosis. The best available treatment is to endeavour, in the early stages, to prevent the progress of the mischief. You must treat the case as a scrofulous one, and not an inflammatory one. Few of these cases will bear the old methods of depletion.

INDEX TO VOL. XLIV.

PAGE

Abdominal collapse, Dr. Barnes on 245
Abortion, Dr. Priestley on cases in which the membranes and placenta are retained 185
Abscess, mammary, treatment of 276
" pelvic, Mr. Skey on 122
Accidental separation of the placenta at the seventh month . . . 186
Actea racemosa, Dr. Mac Donald on its use in acute rheumatism . . 25
Acute dysentery, its treatment by ipecacuanha 57
" rheumatism, with unusual complication 26
" rheumatism, on the use of actea racemosa in 25
Adams, Dr. J., on uterine hemorrhage 195
" Mr. W., on anterior and posterior curvatures of the spine . . 91
Albuminuria, Dr. Beale on the urine in 63
Alcohol, Dr. Hooper on the action and uses of 291
Alcoholic stimulants in hospital practice, Dr. Gairdner on . . . 289
Althaus, Dr., on electricity in amenorrhœa 270
Amenorrhœa, Dr. Althaus on electricity in 271
" " Dr. Simpson on the local treatment of . . . 271
Amputation, Mr. Teale on the advantages of the rectangular mode of . 70
Anasarca, Dr. Witt's method of relieving 68
Anderson, Dr. T. McCall, on the diagnosis of psoriasis . . . 170
" " " " on the treatment of phthiriasis . . . 158
Aneurism, Mr. Price on the treatment of 100
" gluteal, Prof. Syme's case of ligature of 97
" popliteal, treated by compression 97
Apoplexy, Dr. Chambers on blood-letting in 41
Arnott, Dr. J., on the treatment of stricture by incision . . . 134
Arsenious acid as a substitute for quinine in fever 25
Arsenic smoking in asthma 50
Asphyxia, Dr. Watts on 280
Asthma, Dr. Julius on arsenic smoking in 50
Atrophy of children from dirt-eating 282
Auscultation, Dr. Chambers's "Drill" for 50
Barker, Dr. B. F., on shortening the first stage of labour . . . 223
Barnes, Dr. R., on abdominal collapse 245
" " on the induction of premature labour . . . 180
" " on the treatment of placenta prævia . . . 188
Barton, Dr. J. K., his treatment of deformities from severe burns . . 151
" " " on excision of a portion of the os calcis . . . 80
Beale, Dr. L. S., on the urine in albuminuria 63
Beatty, Dr., his case of dysmenorrheal membrane 274
Beef-tea as a medicinal stimulant 317
Bell, Dr. J., on the prevention of pitting in small pox . . . 150
" Dr., on typhus fever 20
Benzine, its use in the treatment of scabies 157
Black eye, use of the root of the convallaria in 173
Bladder, Mr. Thompson on a new remedy for irritable condition of . . 142
Blisters, Mr. Miles on the cure of gonorrhœa by 166
" use of in the treatment of various veins and ulcers . . . 97
Blood-letting in apoplexy, Dr. Chambers on 41
Blood-poisoning following scarlatina and parturition, Mr. Coulson's case of . 240
Bouchut, M., on a pathognomonic sign of scarlatina 23
Breast, Mr. Nunn, on inflammation of the 276

PAGE
Brown-Sequard, Dr. C. E., his remarks on paralysis 34
" " " on reflex paraplegia 29
Budd, Dr. W., his treatment of diphtheria 56
Bullet lodged in the heart for twenty years 104
Burns, Dr. Barton on the treatment of deformities resulting from . . 151
Bursal tumours, use of the thread seton in 80
Carey, Dr. F. E., on the treatment of delirium tremens by digitalis . . 37
Caries of the os calcis, and excision of a portion of the bone . . 80
Cartilages, loose, in the knee-joint 75
Catheterism of the eustachian tube, Mr. Voltolini on 173
Caustic potash, effects of its undue application to the uterus . . 256
Cerebral disease unattended by cerebral symptons, Dr. West on . . 284
Cervix uteri, Dr. Greenhalgh on removal of the 255
Chalybeate, Dr. Skinner's new form of 301
Chambers, Dr. T. K., his "Drill" for auscultation 50
" " on blood-letting in apoplexy 41
Cheesman, Dr. G., his case of extra-uterine gestation . . . 230
Childs, Mr. G. B., his treatment of gonorrhœa 164
Chisholm, Prof., his operation for the radical cure of hernia . . . 107
Chordee, Dr. Mouslon's treatment of 166
Chlorate of potash and sugar, efficacy in phthisis 48
" of potash injections in gonorrhœa 170
Chloroform accidents, Dr. Kidd's rules for guidance in . . . 313
" ether, turpentine, &c., formula for administering . . . 316
" its use in puerperal convulsions 198
Chronometry of life and disease, Mr. Walker on the 302
Cicatrices from burns, Dr. Barton on the treatment of . . . 151
" from suppuration of tubercular glands, treatment of . . 143
Clay, Dr. C., his observations on placenta prævia 193
" on ovariotomy and its results 256
Clover hay, its use as a remedy in hooping-cough 49
Coghlan, Dr. J., on dysmenorrhœa and sterility 275
Collapse, abdominal, Dr. Barnes on 245
Collis, Mr. M. H., on a new mode of treating vesico-vaginal fistulæ . . 236
Colchicum, Dr. Hammond on the diuretic action of 308
Coma, on the causes of 35
Compression, popliteal aneurism treated by 97
Conservative surgery, a rare instance of 105
Convulsions and convulsive affections, on the causes of . . . 36
" puerperal, Dr. Harper's cases of 196
" puerperal, Dr. Wilson's case of 199
" puerperal, use of chloroform in 198
Coulson, Mr. W., his treatment of flat ulcers of the rectum . . . 122
" on hysteria masking secondary syphilis . . . 265
" on secondary abscess following scarlatina and parturition . 240
Courty, Prof., M.A., his treatment of wens and other cysts . . . 147
Crimea, Dr. Lyons on the deaths from fever in the 22
Crinoline accidents, Dr. Odling on 307
Cunningham, Dr. R. W., his treatment of acute dysentery . . . 57
Curvature of the spine, anterior and posterior 91
Cysts, M. Courty on the treatment of without excision . . . 147
Davies, Mr. J. R., on the radical cure of varicocele 141
Decayed teeth, cases of furred tongue on one side arising from . . 53
Decidua, Prof. Virchow on a peculiar degeneration of . . . 195
Deciduous membrane, Dr. Beatty on 274
Delirium and insanity, causes of 35
Delirium tremens, Dr. Carey on its treatment by large doses of digitalis . 37
" " Dr. Peacock on its treatment by large doses of digitalis . 39
" " Dr. Williams on its treatment by large doses of digitalis . 38
Diabetic urine, new mode of estimating the quantity of sugar in . . 64
Diarrhœa, Dr. Hynes on the efficacy of nitric acid and opium in . . 59
Digitalis, delirium tremens treated by large doses of 37
Diphtheria, Dr. Budd's treatment of 56
Diphtheritic ulcer of the uterus, Dr. Ellis on 250
Dirt-eating, Dr. Moore on the atrophy of children from . . . 282
Disease, on the chronometry of life and 302

PAGE.
Dislocations of the shoulder-joint, Dr. Smith on 82
Drill for ausculation, Dr. Chambers's 50
Dropsy, Dr. Witt's method of relieving 68
" following scarlatina, Dr. Taylor's treatment of . . . 24
Drowning, Mr. Hunter on the treatment of asphyxia from . . . 312
" inefficacy of the hot-bath in cases of 281
Druitt, Dr., on the preparation of the essence of beef . . . 316
Duncan, Dr. J. M., on the obliquity of the fœtal head in parturition . . 215
Dysentery, acute, Dr. Cunningham on its treatment by ipecacuanha . . 57
Dysmenorrhœa from mechanical obstruction, Dr. Coghlan on . . 275
Dysmenorrhœal membrane, Dr. Beatty's case of 274
Eastlake, Dr. H. E., on a case of impeded labour . . . 227
Eczema, Prof. Hebra on the treatment of 159
Electricity and galvanism in amenorrhœa, Dr. Simpson on . . . 273
" in amenorrhœa, Dr. Althaus on 270
Ellis, Mr. R., on diphtheritic ulcer of the uterus . . . 250
" on inflamed ulcer of the uterus 252
Ergot of rye, is it dangerous or not to the life of the child? . . . 204
Essence of beef, Dr. Druitt on the 316
Eustachian tube, Dr. Voltolini on catheterism of the . . . 173
Excision of the knee-joint, Mr. Fergusson on a case of . . . 79
" of the os calcis, Dr. Barton's case of 80
Extension, its efficacy in treating contractions from burns . . . 152
Extra-uterine gestation, Dr. Cheesman's case of 230
Eye-lids, Mr. Teale's operation in cases of adhesion of . . . 172
Farre, Dr. A., his case of induction of premature labour . . . 184
Femoral artery, Dr. Rawson's case of traumatic aneurism of the . . 96
Fergusson, Mr. W., on excision of the knee-joint . . . 79
" " on Pirogoff's operation 82
" " on the lateral operation in lithotomy . . . 140
Ferri carbonas effervescens, Dr. Skinner's formula for . . . 300
Fever, Dr. Latham on the difference between curing and treating . . 18
" in the crimea, Dr. Lyons on the deaths from . . . 23
" in the Zambesi, Dr. Livingston on 17
" typhus, Dr. Bell on 20
First stage of labour, Dr. Barker on shortening . . . 223
Fistula, vesico-vaginal, Mr. Collis on the treatment of . . . 236
" vesico-vaginal and recto-vaginal, Mr. Spencer Wells on . . 233
Fœtal head, Dr. Duncan on the obliquity of the 215
Foot, Mr. Fergusson's case of removal of the 82
Forceps obstetric, Dr. Hamilton on their use in tedious labour . . 205
" obstetric, Dr. Hewitt on a modified form of . . . 208
" obstetric, Dr. Sinclair on the timely use of the . . 209
Foster, Mr. M., his remedy for whooping-cough . . . 49
Fracture of the radius, Dr. Gordon on the treatment of . . . 86
Furred tongue, Mr. Hilton's curious cases of 53
Gairdner, Dr. W. T., on the use of alcoholic stimulants in hospitals . . 289
Galvano-caustic apparatus, Dr. Walker on 298
Gay, Mr. J., on incisions for stricture compared with dilation . . 124
" " on internal obstructions by bands . . . 305
Gestation, extra-uterine, Dr. Cheesman's case of 230
Gibb, Dr. G. D., on saccharine fermentation of human milk . . 278
Gluteal anuerism, Prof. Syme's operation in a case of . . . 97
Godfrey, Mr. J. J., his treatment of scabies by benzine . . . 157
Gonorrhœa, Dr. Chambers on the treatment of 166
" Dr. Irvin on chlorate of potash injections in . . 170
" Dr. Mouslon on subnitrate of bismuth as an injection in . 166
" Mr. Childs on injections of liq hydrarg. pernitratis in . 164
" Mr. Miles on its treatment by blisters . . . 166
Gordon, Dr. A., on fractures of the lower extremity of the radius . . 86
Gout, M. Trousseau on the treatment of 314
" M. Trousseau's medical treatment of 27
Gray, Dr. J., on laceration of the perineum during labour . . . 200
Greenhalgh, Dr. R., on removal of the cervix uteri . . . 255
Hamilton, Dr. G., on the use of the forceps in tedious labour . . 205
" " on uterine hæmorrhage 228

PAGE.
Hammond, Dr. W. A., on the diuretic action of colchicum . . . 308
" " on uræmic intoxication 62
Harper, Dr. R., his three cases of puerperal convulsions . . . 196
Heart, Dr. Balch's case of a bullet lodged in for 20 years . . . 104
" disease, its connexion with generation of the kidneys . . 61
Heath, Mr. C, on the treatment of stricture of the urethra . . . 132
Hebra, Prof., on the treatment of eczema 159
Hemorrhage, uterine, Dr. Adams on the influence of contraction in stopping 195
" uterine, Dr. Hamilton on 228
Hemorrhoids, internal, Mr. Lane on the treatment of . . . 114
Hernia, Prof. Chisholm's operation for the radical cure of . . . 107
" Mr. Wood's operation for the radical cure of . . . 106
" congenital inguinal, Dr. Wilson's complicated case of . . 109
" scrotal, Dr. Agnew's operation for 109
Heslop, Mr., his case of spontaneous evolution 226
Hewitt, Dr. G., on a modified form of obstetric forceps . . . 208
" " on supporting the perineum in labour . . . 199
Hilton, Mr. J., his cases of furred tongue on one side . . . 53
" " on hysterical pains in the hip or knee-joint . . . 266
" " on the influence of rest in diseases of the joints . . 71
Holt, Mr. B., his treatment of stricture of the urethra . . . 126
Hooper, Dr. D., on the action and uses of alcohol 291
Hooping-cough, Mr. Foster on a new remedy for 49
Hornidge, Dr. T. K., on the influence of ozene 300
Hospital practice, on the use of alcoholic stimulants in . . . 289
Hot-bath, Dr. Waters on its effects in asphyxia 280
" Mr. Hunter on its physiology, use, and abuse . . . 311
Housemaid's knee, Mr. Skey's treatment of 79
Human milk, Dr. Gibb on vibriones in the 278
Hunter, Mr. C., on the hot-bath, its physiology, use, and abuse . . 311
Hunter, Mr. J., on irritation of the urethra in females . . . 265
Hysteria masking secondary syphilis, Mr. Coulson on . . . 265
Hysterical pains in the hip or knee-joint, Mr. Hilton on . . . 266
Hynes, Dr. P. J., on nitric acid and opium in diarrhœa . . . 59
Impeded labour, Mr. Murray's case of 226
Infants, on the weight of new-born 279
Inflammation of the breast, Mr. Nunn on 276
Inguinal hernia, Dr. Wilson's case of oblique congenital . . . 109
Injections of chlorate of potash in gonorrhœa 170
" of liq. hydrarg. pernitratis in gonorrhœa 164
" of subnitrate of bismuth in acute gonorrhœa . . . 166
Internal hemorrhoids, curative treatment of 114
Intestinal obstruction by bands, Mr. Gay on 305
Intestine, MM. Velpeau and Verneuil on perforation of the . . . 112
Ipecacuanha, acute dysentery treated by large doses of . . . 57
Irritable conditions of the bladder, new remedy for 142
Itch, efficacy of benzine as a remedy for 157
Joints, Mr. Hilton on the influence of rest in diseases of the . . 71
Julius, Dr. F. G., on arsenic smoking in asthma 50
Kidd, Dr., his rules for guidance in chloroform accidents . . . 313
Kidney disease accompanied by albuminuria, Dr. Williams on . . 60
Knee-joint, Mr. Fergusson on excision of the 74
" Mr. Hilton on hysterical pains in the 266
" Mr. Square on loose cartilages in the 75
Labour, Dr. Barker on an effort to shorten the first stage of . . . 223
" Dr. Hamilton on the use of forceps in 205
" Dr. Hewitt on laceration of the perineum during . . . 199
" Dr. Murphy on supporting the perineum during . . . 201
" Dr. Skinner on rupture of the perineum during . . . 203
" Dr. West on the use of ergot of rye during 204
" impeded from dorsal displacement of the arm . . . 226
" premature, Dr. Barnes on the induction of 180
" premature, Dr. Farre's case of induction of 184
" premature, Prof. Simpson on the induction of . . . 178
Laceration of the perineum during labour, Dr. Gray on . . . 200
" of the perineum during labour, Dr. Hewitt on . . . 199

PAGE.
Laceration of the perineum during labour, Dr. Skinner on . . . 203
Lane, Mr. J., on some diseases of the rectum 114
Latham, Dr. P. M., on the difference between curing and treating fever . 18
Lee, Mr. H., on syphilis 161
" " on the radical cure of varicocele 140
Leucorrhœa and vaginitis, Dr. Waller on 267
Ligature of internal hemorrhoids 115
Lin. aq. calcis on cotton wool for preventing pitting in small pox . . 150
Liquor hydrargyri pernitratis injections in gonorrhœa . . . 164
Lithotomy, Mr. Fergusson on the lateral operation in 140
Livingstone, Dr., on fever in the Zambesi 17
Loose cartilages in the knee-joint 75
Lymphatic glands, Mr. Price on tubercular disease of the . . . 143
Mac Donald, Dr F. R., on actea racemosa in acute rheumatism . . 25
Maisonneuve, M., on complete restoration of the tibia by the periosteum . 90
Mammary abscess, treatment of 276
Mayne, Dr. R., on acute softening of the medulla spinalis . . . 28
Medulla spinalis, on acute softening of the 28
Miles, Mr. H. C., on the cure of gonorrhœa by blisters . . . 166
M'Nab, Dr. J., on sesquicarbonate of ammonia in scarlet fever . . 24
Moore, Dr. W., on the atrophy of children from dirt eating . . . 282
Mouslon, Dr., his treatment of acute gonorrhœa 166
Murphy, Dr. E. W., on supporting the perineum in labour . . . 201
Murray, Dr. J. J., on impeded labour from dorsal displacement of the arm . 226
Nævus, Mr. Price on the surgical treatment of 100
Nichols, Dr. J., on a rare instance of conservative surgery . . . 105
Nitric acid and opium, efficacy of in diarrhœa 59
" its use in the treatment of prolapsus ani 117
Nose, Dr. Nichols' case of union of a completely separated portion of . 105
Nunn, Mr. T. W., on inflammation of the breast 276
Obliquity of the fœtal head in the mechanism of parturition . . . 215
Obstetric forceps, Dr. Hewitt on a modified form of 208
" forceps, Dr. Sinclair on the timely use of 209
Odling, Dr., on crinoline accidents 307
Os calcis, Dr. Barton's case of excision of a portion of the . . . 80
Otterson, Dr., on veratrum viride 296
Ovarian disease, danger attending pregnancy complicated with . . 247
" disease, symptoms, diagnosis, and treatment of . . 261
Ovariotomy, Mr. Spencer Wells on 263
" and its results, Dr. Clay on 256
Ozone, Dr. Hornidge on its influence on organic substances . . 228
Paye, Dr. F., on the use of chloroform in puerperal convulsions . . 198
Paralysis, Dr. Brown-Sequard on 34
Paraplegia, reflex, treatment of 30
Parturition, Dr. Duncan on the obliquity of the fœtal head in . . 215
Peacock, Dr. T. B., on the treatment of delirium tremens by digitalis . 39
Pediculi capitis, M. Devergie on the treatment of 158
" corporis, treatment of 159
" pubis, treatment of 159
Pelvic abscess, Mr. Skey's cases of 122
Perineum, Dr. Gray on its laceration during labour 230
" Dr. Hewitt on its laceration during labour 199
" Mr. Murphy on supporting during labour 201
" Dr. Skinner on its laceration during labour . . . 203
Perforation of the intestine, MM. Velpeau and Verneuil on . . . 112
Pharyngoscope, Dr. Voltolini on the 173
Phthiriasis, Dr. McCall Anderson on the treatment of . . . 158
Phthisis, Dr. Turle on chlorate of potash and sugar in . . . 48
Phymosis, Mr. Price on the operation for 139
Pirogoff's operation, Mr. Fergusson's mode of performing . . . 82
Pitting in small-pox, its prevention by lin. aq. calcis on cotton wool . 150
Placenta, Mr. Stanger's case of accidental separation at the seventh month . 186
" prævia, Dr. Barnes on the treatment of 188
" prævia, Dr. Clay on 193
Placental retention, Dr. Priestley on the treatment of cases of . . 185
Pneumonia, Dr. Williams on the diagnosis of 44

PAGE
Popliteal aneurism, its treatment by compression 97
Pregnancy complicated with ovarian disease 246
Premature labour, Prof. Simpson on the induction of . . . 176
" labour, Dr. Barnes on the induction of 180
" labour, Dr. Farre's case of induction of 184
" labour, Dr. Hall Davis on the induction of . . . 182
" labour, Dr. Priestley on the induction of 182
" labour, Dr. Tyler Smith on the induction of . . . 183
" labour, Mr. Baker Brown on the induction of . . . 181
Presbyopia, Mr. Wells on 173
Price, Mr. P. C., on the operation for phymosis 139
" on the surgical treatment of vascular tumours . . 100
" on tubercular disease of the lymphatic glands . . 143
Priestley, Dr. W. O., on abortion in which the membranes and placenta are retained 185
Procidentia uteri from perineal laceration, Dr. Roberts' case of . . 243
Prolapsus ani, Mr. Smiths' treatment of certain forms of . . . 117
Psoriasis, Dr. McCall Anderson on the diagnosis of 170
Puerperal convulsions, Dr. Harper's case of 196
" convulsions, Dr. Page on the use of chloroform in . . . 198
" convulsions, Dr. Wilson's case of 199
Quinine, Mr. Turner on arsenious acid as a substitute for . . . 25
Radius, Dr. Gordon on fracture of the lower extremity of the . . 86
Rawson, Dr. T. J., his case of traumatic aneurism of the femoral . . 96
Rectangular amputation, Mr. Teale on the advantages of . . . 70
Recto-vaginal fistula, Mr. Wells on 233
Rectum, Mr. Coulson's treatment of flat ulcers of the . . . 122
" Mr. Lane on some diseases of the 114
" prolapsus of, its treatment where the ligature is inadmissable . 117
Reflex paraplegia, Editor of the British Medical Journal on . . . 29
Reid, Prof. his case of acute rheumatism with unusual complication . 26
Rest, Mr. Hilton on its influence on diseases of the joints . . . 71
Restoration of the tibia by the periosteum after its removal . . . 90
Rheumatism, acute, Prof. Reid's case of 26
" acute, Dr. MacDonald on actea racemosa in . . . 25
Roberts, Dr. D. L., his operation for procidentia uteri from perineal laceration 243
Roberts, Dr. W., on estimating the quantity of sugar in urine . . 64
Saccharine fermentation of the milk in the female breast . . . 278
" urine, new test for 67
Scabies, Mr. Godfrey on its treatment by benzine 157
Scarlatina, Dr. M'Nab on sesquichlorate of ammonia in . . . 24
" M. Bouchut on a pathognomonic sign of 23
Scarlatinal dropsy, use of benzoate of ammonia in 24
Scars from ulceration after suppuration of tubercular glands, treatment of . 143
Scrotal hernia, Prof. Chisholm's operation for 107
" hernia, Mr. Wood's operation for 106
Secondary abscess following scarlatina and parturition, Mr. Coulson on . 240
" syphilis, case of hysteria masking , ... 265
Shoulder-joint, Dr. Smith's treatment of dislocation of the . . . 82
Simpson, Dr. J. Y., on the induction of premature labour . . . 178
" " on the local treatment of amenorrhœa . . . 271
Sinclair, Dr. E. B., on the timely use of the obstetric forceps . . 209
Skey, Mr. F. C., his treatment of housemaid's knee. . . . 79
Skey, Mr. F. C., on abdominal or pelvic abscess 122
Skinner, Dr. T., on rupture of the perineum during labour . . . 203
" " on the ferri carbonas effervescens . . . 300
Small-pox, Dr. Bell on the prevention of pitting in 150
Smith, Mr. H., on the treatment of certain forms of prolapsus ani . . 117
" Dr. N. R., on dislocations of the shoulder-joint . . . 82
Softening of the medulla spinalis, Dr. Mayne on 28
Speech, causes of loss of 34
Speculum, vaginal, Weiss' improved 233
Spinal marrow, Dr. Mayne on acute softening of the . . . 28
Spine, Mr. Adams on anterior and posterior curvatures of the . . 91
Spontaneous evolution, Mr. Heslop's case of 226
Sputum, its uncertainty in the diagnosis of pneumonia . . . 44

PAGE
Square, Mr. W. J., on loose cartilages in the knee-joint . . . 75
Stanger, Mr. G. E., his case of accidental separation of placenta . . 186
Sterility, and dysmenorrhœa, Dr. Coghlan on 275
Stricture of the urethra, Dr. Arnott on its treatment by incision . . 134
" of the urethra, Mr. Gay on its treatment by dilatation . . 124
" of the urethra, Mr. Heath on forcible dilatation in . . . 132
" of the urethra, Mr. Holt's cases of forcible rupture of . . 126
Subnitrate of bismuth injections in gonorrhœa 166
Sugar in diabetic urine, Dr. Roberts on estimating the quantity of . . 64
Symblepharon, Mr. Teale on the treatment of 172
Syme, Prof., his case of ligature of the gluteal artery for aneurism . . 97
Syphilis, Mr. H. Lee on 161
Syphilitic degeneration of the decidua 195
" psoriasis, diagnosis from non-syphilitic 170
Taylor, Dr. J., on benzoate of ammonia in dropsy after scarlatina . . 24
Teale, Mr. T. P.., on the advantages of rectangular over ordinary amputations 70
Teale, Mr. T. P., jun., on the treatment of symblepharon . . . 172
Thompson, Mr. H., on a new remedy for irritability of the bladder . . 142
Thread seton, its use in the treatment of bursal tumours . . . 80
Throat, ulcerated, treatment of 106
Tibia, M. Maisonneuve on its restoration by the periosteum . . . 90
Tongue, furred, curious cases of, from various causes 53
Traumatic aneurism of the femoral artery 96
Trousseau, M., on the treatment of gout 314
" " his medicinal treatment of gout 27
Tubercular disease of the external lymphatic glands 143
Tumours, vascular, on the surgical treatment of 100
Turle, Dr. J., on chlorate of potash and sugar in phthisis . . . 48
Turner, Mr. J., on arsenious acid as a substitute for quinine . . . 25
Typhus fever, Dr. Bell on 20
Ulcerated throat, Mr. Westcott's treatment of 106
Ulceration, diphtheritic, of the cervix uteri 250
" inflammatory, of the uterus 253
Ulcers of the rectum, treatment of 122
Uræmic intoxication, Dr. Hammond on 62
Ure Mr., his case of varicose veins and ulcers treated by blisters . . 99
Urethra, Mr. Hunter on irritability of, in females 265
" stricture of, Dr. Arnott on incision of 134
" stricture of, Mr. Gay on dilatation in 125
" stricture of, Mr. Heath on forcible dilatation in . . . 132
" stricture of, Mr. Holt on the immediate treatment of . . 126
Urine, diabetic, clinical method of estimating the sugar in . . . 64
" in albuminuria, Dr. Beale on the 63
" new test for sugar in 67
Uterine hemorrhage, Dr. Hamilton on 228
" hemorrhage, influence of contraction in arresting . . . 195
Uterus, Dr. Greenhalgh on removal of the neck of the 255
" Dr. Whitehead on the effects of undue application of caustic potash to 256
" Mr. Ellis on diphtheritic ulcer of the 250
" Mr. Ellis on inflamed ulcer of the 252
Vaginal speculum, Weiss' improved 233
Vaginitis, symptoms and treatment of 267
Varicocele, Mr. Davies on the radical cure of 141
" Mr. Lee on the radical cure of 140
Varicose veins, Mr. Wood on the radical cure of 103
" veins and ulcers treated by blisters. Mr. Ure's case of . . 99
Vascular tumours, Mr. Price on the surgical treatment of . . . 100
Velpeau and Verneuil, M. M., on perforation of the intestine . . 112
Veratrum viride, Dr. Otterson on 296
Vesico-vaginal and recto-vaginal fistulæ, Mr. Wells on 233
" " fistulæ, Mr. Collis's treatment of 236
Vibriones in human milk, Dr. Gibb on 278
Virchow, Prof., on a peculiar degeneration of the decidua . . . 195
Voltolini, Dr., on catheterism of the eustachian tube 173
Walker, D. T. J., on the galvano-caustic apparatus 298
" Mr. A. de N., on the chronometry of life and disease . . 302

PAGE

Waller, Dr. C., on leucorrhœa and vaginitis 267
Waters, Dr. A. T. H., on asphyxia and the effects of the hot-bath . . 280
Weight of new-born infants 279
West, Dr. C., on cerebral disease, unattended by cerebral symptoms . . 284
" Dr. U., on the use of ergot of rye in labour 204
Wells, Mr. T. S., on ovariotomy 263
" " on vesico-vaginal and recto-vaginal fistulæ . . . 233
Wens, M. Courty's treatment of, without excision 147
Westcott, Mr. E., his treatment of ulcerated throat 106
Williams, Dr. C. J. B., on kidney disease accompanied by albuminuria . 60
" Dr. D. M., on the treatment of delirium tremens by digitalis . 38
" Dr. T., on the uncertainty of the sputum in the diagnosis of pneumonia 44
Wilson, Dr. F. W., his case of epileptic puerperal convulsions . . 199
" Dr. W. J., his case of oblique congenital inguinal hernia . . 109
Witt, Dr. C., on anasarca 68
Wood, Mr. J., his operation for the radical cure of hernia . . . 106
" " on the radical cure of varicose veins 103

GENERAL INDEX

TO

PARTS XLI., XLII., XLIII., AND XLIV.

OF

THE RETROSPECT OF MEDICINE.

1860—1861.

	PART	PAGE
Abdominal collapse, Dr. Barnes on	44	245
Abortion, Dr. Priestley on cases in which the membranes and placenta are retained,	44	185
Abscess, chronic mammary treated by strapping	43	255
" mammary, treatment of	44	276
" of the prostate gland, operation for	42	174
" of the prostate gland, Dr. Pitman's case of	43	153
" pelvic after labor, Dr. Gibb on	43	306
" Mr. Skey on	44	122
Abscesses of the female breast	41	247
" deep, Mr. Hilton on opening	43	172
" sinuses, and ulcers, Mr. Hilton's treatment of	43	173
Accidental separation of the placenta at the seventh month	44	186
Aconite acute traumatic tetanus treated by	42	45
Acne, chronic catamenial, Prof. Simpson's treatment of	43	258
Actea racemosa, Dr. MacDonald on its use in acute rheumatism	44	25
Acupressure, Dr. Simpson on its use in arresting hemorrhage	41	132
" causes of its failure in some cases	41	137
" in deligation of arteries in aneurism, &c.	41	138
" its advantage over the ligature	41	138
Acute diseases, Dr. Todd's lectures on	41	310
" dysentery, its treatment by ipecacuanha	44	57
" rheumatism, with unusual complication	44	26
" rheumatism, on the use of actea racemosa in	44	25
Adenocele, or mammary glandular tumour, Mr. Birkett's case of	42	261
Air-passages, Dr. Tomson on removal of foreign bodies from the	43	50
Albumen in the urine, Dr. Beale on the clinical importance of	42	111
" in the urine, Dr. Beale on the tests for	42	107
Albuminuria, acute and chronic, Dr. Corfe on the treatment of	42	116
" Dr. Beale on the urine in	44	63
" its connexion with puerperal insanity	43	235
Alcohol, Dr. Hooper on the action and uses of	44	291
" Dr. Todd's views of its mode of action in disease	43	289
" Dr. Smith on its mode of action in diseases	43	289
" its effects in the induction of kidney disease	43	64
Alcoholic stimulants in hospital practice, Dr. Gairdner on	44	289
Alcoholism, Dr. Smernoff on the treatment of	42	63
" chronic, Dr. Wiltshire's treatment of	42	49
" chronic, Mr. Canton's treatment of	42	56
Alkalies, Dr. Jones on the action of	41	381
Allarton's operation for stone, Mr. Holt on	42	183
" operation for stone, Mr. Ward on	42	186
" operation for stone, Mr. Wood's objections to	43	142

	PART	PAGE
Alopecia areata, Dr. McCall Anderson on	43	167
Amenorrhœa, Dr. Althaus on electricity in	44	271
" Dr. Simpson on the local treatment of	44	271
" Prof. Simpson's treatment of eruptions occurring in	43	258
" Prof. Simpson's treatment of neuralgia occurring in	43	257
" M. Joret on the employment of apiol in	43	257
Amputation at the knee-joint and at the knee, Mr. Butcher on	43	101
" at the knee-joint Prof. Ferguson on	41	120
" Mr. Teale on the advantages of the rectangular mode of	44	70
" Mr. Walton on removal of articular cartilage in	41	130
" Mr. Symonds on the dressing of stumps after	42	152
Anæsthesia, artificial, M. M. Azam and Follin on	41	296
Anæsthetic, Mr. Wilmhurst on a new	43	274
Aneurism by anastomosis, Mr. Pemberton on its treatment by excision	42	158
" popliteal, Dr. Moffatt's mode for making pressure in	42	157
Anasarca, Dr. Witt's method of relieving	44	68
Anchylosis, employment of forcible flexure in	41	131
" Mr. Coote on	43	113
" of the elbow in a faulty position	43	123
Aneurism, Mr. Price on the treatment of	44	100
" gluteal, Prof. Syme's case of ligature of	44	97
" popliteal, treated by compression	44	97
Animal food, its influence on the color of the skin	42	286
Annular ulceration of the cervix uteri	43	217
Antimony, its use in tubercular pneumonia	41	80
Anus, Mr. Bell's treatment of eruptions around the	42	193
Apiol, on its employment in amenorrhœa and dysmenorrhœa	43	256
Apoplexy, Dr. Chambers on blood-letting in	44	41
Argenti nitras fusus, containing chloride of silver	41	286
Arnica as an application to bruises	41	188
Arsenate in soda in Scrofula, Dr. Bouchut on	43	27
Arsenic, its action on the system in disease	41	289
Arsenic smoking in asthma	44	50
Arsenious acid as a substitute for quinine in fever	44	25
" influence of fatty bodies on the solidity of	42	300
Arteries, Mr. Canton on atrophy and degeneration of the	43	313
" degeneration of, Mr. Canton on gout as a cause of	43	29
Articular cartilage, Mr. Walton on removal of in amputation	41	130
Artificial dilitation of the os uteri	43	210
" pupil, Mr. Critchett on the formation of the iridesis	43	192
" teeth, on the use of coralline for	43	140
Asphyxia, Dr. Watts on	44	280
Assalini's forceps, Dr. Granville on	43	307
Asthenic dropsy, on the value of tonics in	42	93
Asthma, Dr. Julius on arsenic smoking in	44	50
" Dr. Salter on the essential nature of	42	92
" Mr. Courty's treatment of, by injection of atropine	41	89
" Mr. Pridham on the the treatment of	42	87
" " " "	43	51
" notice of Dr. Hyde Salter's work on	43	308
Atrophy of children from Dirt-eating	44	282
Atropine, Dr. Cowdell's cases treated by hypodermic injection of	41	60
" injection of, in Asthma	41	89
Auscultation, Dr. Chambers' "drill" for	44	50
" " "order of drill" for	43	51
" fœtal, practical remarks on	41	215
Bath, the lamp, Dr. Taylor on	43	282
" Turkish, Golden on	43	279
" Turkish, Dr. Richardson on the	43	282
Beef-tea as a medicinial stimulant	44	317
Belladonna and opium on the relations of	41	308
" Dr. Hughes' cases illustrating the influence of	42	298
" Dr. Hughes on its influence on the pneumogastric nerve	42	296
" indications and contra-indications to its use in paraplegia	43	35
" its use in the treatment of hooping-cough	42	98

	PART	PAGE.
Belladonna, on dilatation of the pupil produced by	42	301
" on opium as an antidote to poisoning by	42	302
" plaster, Mr. Haselden's improved formula for	42	297
Benzine, its use in the treatment of scabies	44	157
Biniodide of mercury, its use in squamous diseases	41	184
Black eye, use of the root of the convallaria in	44	173
Bladder, Mr. Thompson on a new remedy for irritable condition of	44	142
" singular case of inversion of during pregnancy	42	262
Blister, how to make a	41	303
Blisters, Mr. Miles on the cure of gonorrhœa by	44	166
" use of in the treatment of varicose veins and ulcers	44	97
Blood in fever and inflamation, Dr. Addison on the	41	30
Blood-letting in apoplexy, Dr. Chalmers on	44	41
Blood-poisoning following scarlatina and parturition, Mr. Coulson's case of	44	240
Bloody sweat, Dr. Chalmers on a case of	43	169
Bodies for anatomical purposes, how to preserve	42	280
Bone, diseased, M. Sedillot on scooping as a substitute for resection	41	131
Brain, Dr. Brown Sequard on the diagnosis of diseases of the	43	38
" Mr. Hunter, on free respiration in certain states of the	41	54
" state of, during sleep	42	71
Brandy and glycerine in excoriations and fissures of the nipples	43	260
Bread, fermented and aerated, Dr. Dauglish on the dietetic value of	42	290
Breast, Mr. Nunn, on inflammation of the	44	276
" tumour of, Mr. Erichsen on the diagnosis of	41	246
Bright's disease Dr. Beale on	42	116
" disease Dr. Goodfellow on the pathology of	43	297
" disease, Dr. Goodfellow on the two leading typical forms of	43	62
" disease, Dr. Goodfellow on the treatment of	42	69
" disease, Dr. Orr's case of recovery from	41	111
Bronchial arteries, Drs. Waters and Heale on the anatomy of	41	91
" muscles, Dr. Duncan on paralysis of the	42	95
Bronchitis, chronic, combined with emphysema	42	100
Bronchocele, Dr. Murney's cases treated by strychnia	42	281
Bruises, Mr. Henry on arnica as an application for	41	188
Bullet lodged in the heart for twenty years	44	104
Burns, Dr. Barton on the treatment of deformities resulting from	44	151
Bursal tumours, use of the thread seaton in	44	80
Butcher's box splint, description of	43	101
Cæsarian section, Dr. Edmund's case of	43	219
Camphor and glycerine for the suppression of milk	43	260
Cancer, Mr. George's case treated by sulphate of zinc	43	260
" of the liver, diagnosis of	41	95
" use of peroxide of hydrogen in	42	300
Carbonate of ammonia pills in chronic bronchitis	42	100
" acid gas, recovery from poisoning by	41	297
Carcinoma uteri, Dr. Skinner on deodorisation in	41	244
Caries of the os calcis, and excision of a portion of the bone	44	80
Cartilages, loose, in the knee-joint	44	75
Castor oil plant, use of as a galactagogue	41	288
Cataract, congenital, Mr. Critchett's observations on	43	196
" diabetic, Dr. Willshire on	43	77
" Mr. Walton on the diagnosis of	42	221
" on the operations for	43	182
Catheter for dilating strictures, Dr. Shiletoe's new form of	42	180
Catheterism of the eustachian tube, Mr. Voltolini on	44	173
Catheters, Mr. Smith on a new and uniform guage for	43	147
Caustic potash, effects of its undue application to the uterus	44	256
" Prof. Smith's new	41	286
Caustics, on the treatment of stricture by	43	152
Cauterization, how to mould chloride of lime into sticks for	42	280
Cells, Dr. Bernard on diseases from the vitiated development	41	43
Central leucoma, efficacy of iriddesis on	43	193
Cerebral disease unattended by cerebral symptoms, Dr. Wests on	44	284
Cerebro-spinal fluid, Dr. Williams on	41	306
Cervix uteri, Dr. Barker on annular laceration of	43	217

	PART	PAGE
Cervix uteri, Dr. Greenhalgh on removal of the	44	255
Chalk-and-gum bandage	43	311
Chalybeate, Dr. Skinner's new form of.	44	301
Chancre, Mr. Harrison on the specific characters of	41	194
Chemical agents of disease in the living body Mr. Bernard on	41	309
" questions in connexion with diseases of the liver	42	105
Childhood, Dr. Jenner on the diseases of	41	249
Chlorate of potash and glycerine as a disenfectant	43	277
" of potash and sugar efficacy in phthisis	44	48
" of potash injections in gonorrhœa	44	170
Chloride of lime moulded into sticks for cauterization	42	280
" of sodium in the urine	41	107
Chlorodyne, Dr. Washbourne on the effects of	43	275
Chloroform accidents, Dr. Kidd's rules for guidance in	44	313
" Dr. Dick's case of traumatic tetanus treated by	41	51
" Dr. Kidd on the nature of death from	42	294
" Dr. Petrie on the cause of death from	43	275
" ether, turpentine, &c., formula for administering	44	316
" its beneficial effects in the treatment of croup	41	71
" its use in puerperal convulsions	44	198
" its use in strangulated hernia	42	167
Chordee, Dr. Mouslon's treatment of	44	166
" Mr. Milton on the treatment of	41	193
Chorea, Dr. Jones on	43	42
" its treatment at the Birmingham General Hospital	43	42
" use of Faraday's electricity in	42	70
Chronic bronchitis and emphysema	42	99
" bronchitis, use of carbonate of ammonia pills in	42	100
" mammary abscess treated by strapping	43	255
" rheumatism, its treatment by congelation	42	26
Chronometry of life and disease, Mr. Walker on the	44	302
Cicatrices from burns, Dr. Barton on the treatment of	44	151
" from suppuration of tubercular glands, treatment of	44	143
Ciliary muscle, division of in the treatment of glaucoma	42	208
" muscle, Mr. Hancock's operation for dividing	42	213
Cleft palate, Mr. Price's needle for applying metallic sutures in	42	165
Clover hay, its use as a remedy in hooping cough	44	49
Colchicum, Dr. Hammond on the diuretic action of	44	308
Collapse, abdominal, Dr. Barnes on	44	245
Coma, on the causes of	44	35
Composition of the urine in health and disease, notice of Dr. Parkes' work on	43	312
Compound fracture, excision of the knee-joint for	43	120
Compression, popliteal aneurism treated by	44	97
Condy's disinfecting fluid, its use in carcinoma uteri	41	244
Congelation, Dr. Arnott on the treatment of chronic rheumatism by	42	26
Congestion of the lungs, Dr. Smith on dyspnœa as a cause of	42	85
Conical cornea, movements of the pupils after operations for	43	300
" cornea, Mr. Bowman's treatment of	43	199
Conservative surgery, a rare instance of	44	105
Continued fever, Dr. Tweedle on the forms of	41	17
Contracted pelvic brim, turning in cases of	41	212
" pelvic brim, turning in cases of	42	240
Contractions at the shoulder-joint, Mr. Coote on	43	116
Contraction of the knee, Mr. Holmes Coote on	41	125
Contractions of the knee-joint, Mr. Coote on	43	113
Convulsions and convulsive affections, on the causes of	44	36
Convulsions puerperal, Dr. Harper's cases of	44	196
" puerperal, Dr. Wilson's case of	44	199
" puerperal, Prof. Scanzoni on the treatment of	42	242
" puerperal, use of chloroform in	44	198
Corelysis, Mr. Streatfield on the operation of	42	194
Cornea, penetrating ulcer of, efficacy of iriddesis in	43	193
Corns on the sole of the foot, Mr. Coote on	41	187
Cranial perforator, Dr. Thomas's new	43	219
Craniotomy and forceps cases, statistics of	42	234
Creasote, its use in the treatment of dysentery	42	101

	PART	PAGE
Crimea, Dr. Lyons on the deaths from fever in the	44	22
Crinoline accidents, Dr. Odling on	44	307
Cross—presentations, Dr. Doig on	42	231
Croup, Mr. Smith on a fibrinous clot in the heart in some cases of	41	71
" Mr. Spence on tracheotomy in	41	89
" on its mercurial treatment in rural districts	41	90
" treated by the inhalation of chloroform	41	69
Curvature of the spine, anterior and posterior	44	91
" of the spine, Mr. Coote on the treatment of	43	121
Cutaneous hemorrhage, Dr. Chambers' case of	43	169
Cystic disease, Dr. Collis on	43	286
Cysts, Mr. Courty on the treatment of without excision	44	147
Datura, Dr. Anderson on opium as a remedy in poisonings by	42	309
Deafness, on ether as a remedy for	43	208
Death and almost entire absorption of fœtus of six months' development	42	267
Death from chloroform, Dr. Kidd on the nature of	42	294
Decayed teeth, cases of furred tongue on one side arising from	44	53
Decidua, Prof. Virchow on a peculiar degeneration of	44	195
Deciduous membrane, Dr. Beatty on	44	274
Degeneration of arteries on gout as cause of	43	29
" and atrophy of the arteries	41	313
Delirium and insanity, cause of	44	35
" tremens, Dr. Carey on its treatment by large doses of digitalis	44	37
" tremens, Dr. Peacock on its treatment by large doses of digitalis	44	39
" tremens, Dr. Williams on its treatment by large doses of digitalis	44	38
" tremens, Dr. Marston on the form and treatment of	42	53
" tremens, Mr. Jones treatment by digitalis	42	59
" tremens, cases treated without stimulants	42	51
" tremens, Prof. Dunglison on the rational treatment of	41	55
Dentition, Dr. Jacobi on popular errors respecting	43	264
Deodorization in carcinoma uteri, Dr. Skinner on	41	244
" in obstetric medicine, Dr. Skinner on	42	269
Depilation, modus operandi	43	154
Detachment of the placenta on a new sign of	43	223
Detection of arsenic, antimony, copper and bismuth by electrolysis	41	317
Diabetic urine, new mode of estimating the quantity of sugar in	44	64
" sugar, Dr. Beale on the tests for	42	118
" urine, Dr. McGhie on the methods of testing	42	123
Diabetes, Dr. Pavy on certain points connected with	43	79
" on cataract, Dr. Willshire's clinical remarks on	43	77
" use of peroxide of hydrogen in	42	300
Diarrhœa, Dr. Haynes on the efficacy of nitric acid, opium, in	44	59
Diathetic diseases of childhood, Dr. Jenner on	41	249
Dietetic inventiveness of hunger	42	100
Digestibility of various articles of diet, Dr. Dayon	43	54
Digitalis, delirium tremens treated by large doses	44	37
" Mr. Jones employment of it in delirium tremens	42	59
" its action on the system in diseases	41	290
Dilatation of the uteris in labour	43	210
Diphtheria, Dr. Budd' treatment of	44	256
Diphtheritic ulcer of the uterus, Dr. Ellis on	44	250
Dirt eating, Dr. Moore on the atrophy of children from	44	282
Disease, Dr. Richardson on its study by synthesis	41	47
" arising from vitiated development of cells	41	43
" in the living body on the chemical agent of	41	309
" M. Claude Bernard on	42	286
" Dr. Smith on the action of alcohol in	43	289
" on the chronometry of life and	44	302
Diseases of the skin in children, notice of Caillault's work on	43	319
Disinfectant, M. Martinet on chlorate of potash and Glycerine as a	43	277
Dislocations, Dr. Watson on a new instrument for	43	123
" of the shoulder joint, Dr. Smith on	44	82
Dislocation of humerus, Dr. Skey on	41	127
" of the ulna forwards at the elbow, Mr. Canton's case of	42	150
Drill for ausculation, Dr. Chambers	44	50

	PART	PAGE
Dropsy, Dr. Witt's method of relieving	44	68
" following scarlatina, Dr. Taylor's treatment of	44	24
" asthenic, Dr. Sisson on the value of tonics in	42	93
" on the fallopian tube, Dr. Simpson on the diagnosis of	42	244
" ovarian, Mr. Wells's performance of ovariotomy in four cases of	42	251
" ovarian, on the diagnosis of	42	247
Drowned, Drs. Christian and Sharpey on the treatment of the	43	277
" the Sylvester method of resuscitating persons apparently	43	277
Drowning, Dr. Hunter on the treatment of asphyxia from	44	312
" inefficacy of the hot bath in cases of	44	281
Duplex uterus, case of pregnancy with	42	241
Dysentery acute, Dr. Cunningham on its treatment by ipecacuanha	44	57
" Dr. Gairdner on the use of creasote in	42	101
" Dr. Oliver on the treatment of	41	94
Dysmenorrhœa, M. Joret on the employment of apiol in	43	256
" from mechanical obstruction, Dr. Coghlan on	44	275
Dysmenorrhœal membrane, Dr. Beatty's case of	44	274
Dyspepsia, Dr. Leared on the efficacy of strychnine in	41	322
Dyspeptic asthma, Mr. Pridham on	42	87
Dispnœa as a mechanical cause of congestion of the lungs	42	85
Ear polypus of, treated by slow torsion	41	106
Ecraseur on the effects of removal of hemorrhoids by the	43	135
Eczema, Prof. Hebra's treatment of	43	165
" Prof. Hebra on the treatment of	44	59
" on the hands, M. Guillot's ointment for	41	184
Egypt as a residence for European invalids, Dr. Reil on	41	315
Electricity, its efficacy in chorea	42	70
" neuralgia treated by powerful currents of	42	49
" and galvanism in amenorrhœa, Dr. Simpson on	44	273
" in amenorrhœa, Dr. Althaus on	44	270
Electrolosis, Prof. Bloxam on the detection of metallic poisons by	41	317
Emphysema and chronic bronchitis, treatment of	42	99
Eneuresis, Dr. Hughes on use of belladonna in	42	298
Enteric fever, Dr. Gairdner on the diagnosis of	42	18
" fever on the symptoms and pathology of	41	18
" Dr. Wardell on	43	24
Entropion, M. Sichel on the treatment of a frequent form of	42	227
" Dr. Williams new treatment of	43	205
Enucleation intra uterine fibro plastic tumours removed by	43	307
Epilepsy, Dr. Radcliffe on the theory of	42	64
" Dr. Brown Sequard's treatment of	43	44
" Dr. Pidduck on the treatment of	41	51
Epistaxis arrested by perchloride of iron	41	88
Ergot of rye, is it dangerous or not to the life of the child?	44	204
" indications and contra-indications to its use in paralysis	43	35
Eruption around the anus, treatment of	42	193
Essence of beef, Dr. Druitt on the	44	316
Ether, as a remedy for deafness	43	208
Eustachian tube, Dr. Voltolini on catheterism of the	44	173
Excision of the knee-joint, Mr. Ferguson on a case of	44	79
" of the os calcis, Dr. Barton's case of	44	80
" of the hip-joint, Mr. Erichson on	42	140
" of the knee-joint, report of nine cases of	42	136
" of the knee-joint, Mr. Butcher on	43	101
" of the knee-joint, Mr. Coote on the practice of	43	120
" of the knee-joint for compound fractures	43	121
" of the shoulder, Mr. Coote on the practice of	43	116
" of the knee-joint, Dr. Watson on	41	121
" of the knee-joint Mr. Earnshaw's case of	41	124
" of the knee-joint, Mr. Pemberton on	41	114
Excoriated nipples, Dr. Pierce's treatment of	41	249
Experimental pathology, M. Claude Bernard on	41	39
Extension its efficacy in treating contractions from burns	44	152
Extra-uterine gestation, Dr. Cheesman's case of	44	230
Eye, M. Laborie on its examination by reflected natural light	42	313

	PART	PAGE
Eyes, Mr. Le Gros Clark's case of injury of, by molten lead	43	208
Eyelid, upper, difficulty in producing union in laceration of	41	201
Eyelids, Mr. Teale's operation in cases of adhesion of	44	172
Fallopian tube, Dr. Simpson on Dropsy of the	42	244
Faradization of the skin in chorea	42	70
Fatal disease produced by the trichina spiralis	42	284
Fatty bodies, their influence on the solubility of arsenous acid	42	300
Fatty kidney, occurring as an independent form	43	298
Favus, Dr. Gairdner on the treatment of	42	192
" modern methods of treatment	41	183
" Dr. Pirrie's observations on	43	161
" treatment of	43	159
Femoral artery, Mr. Porter on a new mode deligating the	43	124
" Dr. Rawson's case of traumatic aneurism of the	44	96
Ferri carbonas effervescens, Dr. Skinner's formula for	44	300
Fever, Dr. Tweedie on changes of the blood in	41	17
" and inflammation, Dr. Addison on	41	30
" Dr. Latham on the difference between curing and treating	44	18
" enteric and typhus, Dr. Tweedie on the diagnosis of	41	18
" enteric, Dr. Tweedie on the symptoms and pathology of	41	18
" enteric, and typhus, Dr. Gairdner on the diagnosis of	42	18
" enteric, Dr. Wardell on	43	24
" intermittent, its treatment by inhalation of quinic ether	42	25
" in the Crimea, Dr. Lyons on death from	44	23
" in the Zambesi, Dr. Livingston on	44	17
" rheumatic, Dr. Inman on the value of lime-juice in	41	137
" rheumatic, Dr. Todd on treatment of	41	318
" typhoid, Dr. Brinton on the treatment of	42	24
" typhoid and typhus, Dr. J. Bell on the identity of	43	17
" typhoid and typhus, Dr. Wardell on the non-identity of	43	24
" typhus, Dr. Bell on the condition of the heart in	41	35
" typhus, Dr. Tweedie on the symptoms and pathology of	41	21
" typhus, Dr. Tweedie on the treatment of	42	21
" typhus and typhoid, Dr. Kennedy on	42	17
" typhus, Dr. Bell on	44	20
Fibrinous deposition in the heart, Dr. Richardson on	41	62
" deposits in the heart, Mr. Crisp on	43	49
Fibro-plastic uterine tumour removed by enucleation	43	307
Fibrous tumours of the uterus, Mr. Baker Brown's treatment of	43	242
" " of the uterus, Mr. Harper's instruments for curing	43	245
First stage of labour, Dr. Barker on shortening	44	223
Fissure of the anus, Mr. Rouse on	42	170
" of the rectum simulating disease of the uterus	42	265
Fissures of soft palate, new suture for	41	282
Fistula in ano, Mr. Bonnafont on the injection of iodine in	41	149
" salivary, Mr. Butcher on	43	140
" vesico-vaginal, Mr. Baker Brown on the operation for	43	240
" vaginal, Mr. Hilliard's instruments for	43	237
" vesico-vaginal, Dr. Baker Brown's new operation for	41	222
" vesico-vaginal, Dr. Battey's new treatment and apparatus for	41	149
" vesico-vaginal, McCollis on the treatment of	44	236
" vesico-vaginal and recto-vaginal, Mr. Spencer Wells on	44	233
Fœtal auscultation, Dr. Druitt's remarks on	41	215
" head, Dr. Duncan on the obliquity of	44	215
Fœtus of six months, Dr. Fountain's case of death and absorption of	42	267
Follicular inflammation of labia, Dr. Tilt's treatment of	43	254
Foot, Mr. Ferguson's case removal of the	44	82
Forceps, Assalinis on a modification of	43	367
" obstetric, Dr. Knight's new form of	42	229
" obstetric, Dr. Hamilton on their use in tedious labour	44	205
" obstetric, Dr. Hewitt on a modified form of	44	208
" obstetric, Sinclair on the timely use of the	44	209
" and perforator compared by Dr. Doig	42	231
Forcible flexure in anchylosis	41	131
Foul sores, Mr. Henry on the treatment of	41	189

	PART	PAGE
Fracture, compound, Dr. Porter on excision of the knee-joint for	43	120
" of the glenoid cavity, Mr. Skey on	41	130
" of the pelvis and rupture of the urethra	41	182
" of the radius, Dr. Gordon on the treatment of	44	86
Fungus, condition of uterine mucous membrane, M. Bequerel's treatment of	41	246
Furred tongue, Mr. Hilton's curious cases of	44	53
Galactagogue, Dr. Routh on the use of the castor-oil plant as a	41	288
Gall-stones, Dr. Thudichum on the pathology and treatment of	41	96
" probable mode of their formation	41	96
" Dr. Thudichum on the formation of	42	105
Galvanic current, case of paralysis agitans removed by the	41	53
Galvano-caustic apparatus, Dr. Walker on	44	298
Ganglions of the wrist, Prof. Gross' treatment of	41	285
Gastric affections, Dr. on oxalate of cerium in	43	58
Gestation, extra uterine, Dr. Cheesman's case of	44	230
Glaucoma, Dr. Hancock on division of the ciliary muscle in	41	197
" Mr. Bowman on iridectomy in	42	202
" Mr. Hancock on division of the ciliary muscle in	42	208
" Mr. Rouse's cases treated by division of the ciliary muscle	42	213
" and its surgical treatment. Mr. Hulke on	42	205
" and optic papilla, Dr. Mackenzie on	42	215
" case of division of the ciliary muscle in	42	212
" Dr. Quaglino's conclusions respecting iridectomy in	43	184
" Mr. Nunnely's new operation for	43	187
" acute, Mr. Dixon's case of iridectomy in	43	184
Gleet, Mr. Cooke's treatment of	41	196
Glauco-genic theory, Dr. Pavy's objections to the	43	91
Gluteal aneurism, Prof. Syme's operation in case of	44	97
Glycerine in skin diseases	42	193
Gonorrhœa, Dr. Chambers on the treatment of	44	166
" Dr. Irvin on chlorate of potash injections in	44	170
" Dr. Mouslon on subnitrate of bismuth as an injection in	44	166
" Mr. Childs on injections of lig. hydrarg. pernitratis in	44	164
" Mr. Miles on its treatment by blisters	44	166
" Mr. Cooke's treatment without copaiba	41	196
" Mr. Milton on its treatment without specifics	41	190
" on the distinction between true and spurious	41	196
Gout, Dr. Garrett on the treatment of	43	276
" and degeneration of arteries, Mr. Canton on the connection between	43	29
" Dr. Garrod on the nature and treatment of	41	35
" M. Trousseau on the treatment of	44	314
" M. Trousseau's medical treatment of	44	27
Hands, eczema of, M. Guillot's ointment for	41	184
Hare-lip, Mr. Startin's new suture for	41	282
" Mr. Woakes' new mode of operating for	42	163
Hay-asthma, Dr. Lawford on the proper use of stramonium in	42	93
Headache, congestive, efficacy of belladonna in	42	300
Heart, Dr. Balch's case of a bullet lodged in, for 20 years	44	104
" Dr. Cockle on the influence of the nerves on the action of	41	72
" Dr. Cockle on the physical diagnosis of diseases of	41	73
" Dr. Richardson on fibrinous deposits in the	41	62
" condition of, in typhus fever,	41	35
" Mr. Crisp on fibrinous deposits in the	43	49
Heart disease, its connexion with generation of the kidneys	44	61
Heart's throb, Mr. Maclise on the signification of the	42	71
Heat apoplexy, on its prevalence during the hot-weather campaign in 1858	42	73
Heath's Manual of Minor Surgery and Bandaging, notice of	43	310
Hemicrania, Dr. Barton on the hypodermic injection of morphia in	42	46
Hemorrhage, Dr. O'Ferrall on the use of larch-bark in	42	77
" from the bowel in children a sign of polypus	41	255
" from leech-bites, use of collodion in	41	139
" from the tonsils arrested by the perchloride of iron	41	138
" in the grey matter of the spinal cord,	42	40
" surgical, acupressure as a means of arresting	41	132

	PART	PAGE
Hemorrhage, uterine, Dr. Adams on the influence of contraction in stopping	44	195
" uterine, Dr. Hamilton on	44	228
Hemorrhoids, internal, Mr. Lane on the treatment of	44	114
" Mr. Smith on the application of nitric acid in	41	323
" on their removal by the ecraseur	43	135
Hepatine, Dr. Pavy on its production in the liver	43	91
Hereditary nature of physical malformations	42	272
Hernia, Dr. Morton's operation in case of	43	131
" Dr. Niven's new operation for	43	134
" Dr. Power's case treated by inverting the patient	43	135
" Mr. Bigg's new "Triple Lever Truss" for	43	134
" Mr. Bryant's suggestions for the treatment of	43	133
" Mr. Jordan's modification of Wutzer's instrument	41	148
" Mr. Lee's modification of Wutzer's operation	41	147
" Mr. Wells on the causes of failure in Wurtzer's operation	43	129
" Mr. Woods operation for the radical cure of	41	143
" Mr. Woods operation for the radical cure of	44	106
" congenital inguinal, Dr. Wilson's complicated case of	44	109
" Prof. Chisholm's operation for the radical cure of	44	107
" Prof. Syme's new method of reducing	43	127
" scrotal, Dr. Agnew's operation for	44	109
" strangulated, Mr. Bryant on the use of chloroform in	42	167
" strangulated, Mr. Jessop on the reduction of	42	166
Herpes circinatus, treatment of	43	156
Hip-joint, Dr. Hoppe on affections of, from uterine disease	43	254
" Mr. Erichsen on excision of the	42	140
" disease, distinctive character of the three forms of	42	140
Homœopathy, Mr. Braithwaite on the statistics of	41	255
" not legally sanctioned in Austria, Prussia or France,	41	260
Homœopathic statistics, Dr. Gairdner on	41	264
" statistics on the cholera at Huddersfield, exposed by Dr. Taylor	41	277
Hooping-cough, Dr. Fuller on the use of belladonna in	42	98
" Mr. Foster on a new remedy for	44	49
Hospital practice, on the use of alcoholic stimulants in	44	289
" statistics, Dr. Stone on the fallacy of	41	260
Hospitals, Miss Nightingale on the mortality of different	41	260
" Mr. Thompson's statistics of	41	272
Hot-bath, Dr. Waters on its effects in asphyxia	44	280
" Mr. Hunter on its physiology, use, and abuse	44	311
Housemaid's knee, Mr. Skey's treatment of	44	79
Human milk, Dr. Gibb on vibriones in the	44	278
Hydatid tumour of the liver, Dr. Garrod on	42	103
Hydrocele, Mr. Davidson's case treated by the iron-wire seton	41	181
" Mr. Simpson's case treated by the iron-wire seton	41	182
Hydrogen, Dr. Richardson on the peroxide of	42	300
Hypochondriasis, puerperal, treated by tincture of actea	43	233
Hypodermic action of morphia, Mr. Spender on	42	40
" injection of atropine, cases treated by	41	60
" injection of morphia in hemicrania	42	46
" inoculation, sedative, Mr. Lafarge on	43	285
Hysteria masking secondary syphilis, Mr. Coulson on	44	265
Hysteric tympanitis, Drs. Ramskill and Jones' case of	41	242
Hysterical pains in the hip or knee-joint, Mr. Hilton on	44	266
Impeded labour, Mr. Murray's case of	44	226
Incontinence of urine, efficacy of belladonna in	42	298
Induction of premature labour, Mr. James on the	43	217
Infants, Mr. Ballard on the diseases of	41	162
" on the weight of new-born	44	279
Inflammation and abscess of prostate gland	42	174
" Dr. Addison on	41	30
" of neck of uterus, treatment of	42	264
" of the breast, Mr. Nunn on	44	276
" of the spinal cord, Dr. Sequard on	42	29
" sthenic, on drugs which arrest	41	288
Inflation of the lungs of infants, Dr. Wilson's instruments for	43	261

	PART	PAGE
Ingrowing toe-nail, treatment of	43	179
Inguinal hernia, Dr. Wilson's case of oblique congenital	44	109
" hernia, Mr. Wells on Wurtzer's operation for	43	129
" hernia, Prof. Syme's new operation for	43	127
Injections of chlorate of potash in gonorrhœa	44	170
" of liq. hydrarg. pernitratis in gonorrhœa	44	164
" of subnitrate of bismuth in acute gonorrhœa	44	166
Injury to the eyes by molten metal	43	208
Insanity, puerperal, its connection with albuminuria	43	235
Intermittent fever, inhalation of quinic ether in	42	25
Internal hemorrhoids, curative treatment of	44	114
Intestinal contents, Dr. Day on the	43	55
" obstruction by bands, Mr. Gay on	44	305
Intestine, MM. Velpeau and Vernueil on perforation of the	44	112
Iodide of arsenic, its use in squamous diseases	41	184
Iodine, injection of, in fistula in ano	41	149
Ipecacuanha, acute dysentery treated by large doses of	44	57
Iriddisis, Mr. Critchett on formation of artificial pupil by	43	192
Iridectomy in glaucoma, British Medical Journal's remarks on	43	184
" in glaucoma, Dr. Quaglino on	43	184
" in glaucoma, Mr. Bowman on	42	202
" in glaucoma, Mr. Hulke on	42	205
" in glaucoma, Mr. Nunneley on	43	187
" Mr. Greenway's new operation for	43	182
" opinions of Continental surgeons on	43	181
Iritis, double acute, Mr. Laurence's treatment of	41	203
" syphilitic, Mr. Walton on	42	224
Iron, its action on the system in diseases	41	186
" and quinine, Dr. Fergus on a new combination of	43	274
Iron-wire seton, cases of hydrocele treated by	41	181
Irritable conditions of the bladder, new remedy for	44	142
Itch, efficacy of benzine as a remedy for	44	157
Joints, Mr. Hilton on the influence of rest in diseases of the	44	71
" Mr. Pirrie on chronic diseases of the	43	112
" on scrofulous diseases of the synovial membrane of	42	145
" treatment of wounds of the	43	112
Kidney disease, accompanied by albuminuria, Dr. Williams on	44	60
" Dr. Beale on morbid changes affecting the structure of the	42	114
" Dr. Goodfellow on Bright's disease of the	43	297
" Dr. Goodfellow on the effect of alcohol in inducing disease of	43	64
" Dr. Goodfellow on the two leading types of Bright's disease of	43	62
" Dr. Goodfellow on the treatment of Bright's disease of	43	69
Knee, Mr. Coute on the treatment of contraction of	41	125
Knee-joint, amputation at the, Mr. Butcher on	43	101
" amputation at, Mr. Fergusson's case of	41	120
" excision of the, Mr. Butcher on	43	104
" excision of, for compound fractures	43	120
" excision of, Dr. Watson on the operation of	41	121
" excision of, Mr. Farnshaw's case of	41	124
" excision of, Mr. Pemberton on	41	114
" Dr. Lynch on a peculiar affection of the	43	118
" Mr. Fergusson on excision of the	44	74
" Prof. Fergusson's case of removal of loose cartilage from the	42	148
" Prof. Fergusson's case of excision of	42	136
" Mr. Coote on the treatment of contractions of the	43	116
" Mr. Gant's case of excision of	42	138
" Mr. Hilton on hysterical pains in the	44	266
" Mr. Le Gros Clark's case of excision of	42	137
" Mr. Simon's cases of excision of	42	137
" Mr. Solly's cases of excision of	42	137
" Mr. South's cases of excision of	42	137
" Mr. Square on loose cartilages in the	44	75
Labia, follicular inflammation of, Dr. Tilt's treatment of	43	254

	PART	PAGE.
Labour, Dr. Baker on an effort to shorten the first stage of	44	223
" Dr. Hamilton on the use of forceps in	44	205
" Mr. Hardy on a special position in	43	301
" Dr. Hewitt on laceration of the perineum during	44	199
" Dr. Hicks on a new method of version in	42	235
" Dr. Murphy on supporting the perineum during	44	203
" Dr. West on the use of ergot of rye during	44	204
" impeded from dorsal displacement of the arm	44	226
" premature, Dr. Barnes on the induction of	44	180
" premature, Dr. Farre's case of induction of	44	184
" premature, Mr. James on the induction of	43	217
" premature, Dr. Noeggerath on Cohen's method of inducing	41	207
" premature, Prof. Simpson on the induction of	44	178
Laceration of the perineum during labour, Dr. Gray on	44	200
" of the perineum during labour, Dr. Hewitt on	44	199
" of the perineum during labour, Dr. Skinner on	44	203
Lachrymal obstructions, Mr. Teale, jun.'s treatment of	41	202
Lactalica, Dr. Gardner on	43	258
Lamp bath, Dr. Taylor on the	43	282
Larch-bark, its use in internal hemorrhage	42	77
Laryngismus stridulus, use of belladonna in	42	297
Laryngoscope, Czermak on the use of the	42	77
" on demonstrations with the	42	80
Lead, on the action of	41	289
Leech-bites, use of collodion in arresting hemorrhage from	41	139
Leucorrhœa and vaginitis, Dr. Waller on	44	267
Ligature of the femoral below Poupart's ligament	43	124
" of internal hemorrhoids	44	115
Lightning, M. Boudin on the attitude of persons killed by	41	305
Lin. aq. calcis on cotton wool for preventing pitting in small pox	44	150
Linear extraction in congenital cataract, Mr. Bowman's operation	43	197
Liquor hydrargyri pernitratis injections in gonorrhœa	44	164
Lithotomy, Mr. Bowman's prostatic dilator	41	169
" Dr. Buchanan on the causes of death in	41	150
" Dr. Buchanan's rectangular operation	41	155
" Mr. Erichsen on the median and lateral operations	41	164
" Mr. Fergusson on the lateral operation in	44	140
" Prof. Fergusson on the lateral and median operations	43	142
" Mr. Holt on Allarton's operation of	42	183
" Prof. Smith's instrument for	42	188
" Mr. Teale on the median operation	41	159
" Mr. Thompson's new director	41	171
" Mr. Todd's prostatic dilator	41	172
" Mr. Ward on Allarton's operation of	42	186
" Mr. Wood's new form of staff for	43	140
" Mr. Wood's objections to the median operation	43	142
" median, Chirurgus on	41	175
" median, Mr. Steele's case of	41	174
Lithotrity, Mr. Coulson's observations on	41	175
Liver, Dr. Garrod on hydatid tumour of the	42	103
" Dr. Inman on the action of mercury on the	43	59
" Dr. Thudichum on the action of mercury on the	43	58
" Dr. Thudichum on the treatment of diseases of the	42	105
" Mr. McWhinnie's case of displacement of	43	61
" cancer of, Dr. Harding on the diagnosis of	41	95
Loose cartilage of the knee-joint, removal of	42	148
" cartilages in the knee-joint	44	75
Lower lip, Mr. Murray on a new malformation of the	42	272
Low fevers, use of peroxide of hydrogen in	42	300
Lumbrici, Dr. Brisbane on the use of santonine for	42	102
Luna, Drs. Waters and Heale on the anatomy of the	41	91
Lunar caustic, new form of	41	286
Lungs, Dr. Smith on dyspnœa as a cause of congestion of	42	85
Lupus, Mr. Cooke's effectual cure of	41	186
Lymphatic glands, Mr. Price on tubercular disease of	44	143

	PART	PAGE
Madeira and Cairo, differences between the climates of	41	315
Malformation of the lower lip, Mr. Murray on a new kind of	42	272
Mammary abscess, treatment of	44	276
" abscess, Dr. Harris' mode of preventing	43	260
" chronic, Mr. Coulson's mode of preventing	43	256
" tumour, Mr. Birkett's case of	42	261
" tumour, Mr. Erichsen on	42	256
Mania puerperal, Dr. Simpson on the treatment of	43	235
Maunder's operative surgery, notice of	43	312
McKinnell's system of ventilation	42	282
Meconite of morphia, subcutaneous injections of, in puerperal convulsions	42	242
Medical degree of St. Andrews, Dr. Braithwaite on the	42	314
Medicine, Dr. Cockle on the aim of	41	312
Medicinal substances, M. Bernard on the general effects of	42	287
Medulla oblongata, state of, in epilepsy	42	67
" spinalis, on acute softening of the	44	28
Menorrhagia, Dr. Routh on the treatment of	43	303
Mercurial treatment of croup, Mr. Ferguson on	41	90
Mercury, Dr. Inman on its action on the liver	43	59
" Dr. Thudichum on its action on the liver	43	58
Metallic poisons, on their detection by electrolysis	41	317
" sutures, Mr. Price's new needle for applying	42	165
Microscope, its use in the diagnosis of phthisis	42	82
Milk, Dr. Gardner on the means of influencing the secretion of	43	258
" Dr. Harris on glycerine and camphor for suppression of	43	260
" abscess, efficacy of belladonna in	42	298
Milton, Mr. J. L., on the treatment of gonorrhœa without specifics	41	190
Mitral-valve disease, certain points of physical diagnosis in	41	73
Morphia, hypodermic injection of, in hemicrania	42	46
" its use in acute sclerotitis and iritis	41	203
" on the hypodermic action of	42	47
Muscles, physical condition of, during myalgia	41	58
Myalgia, Dr. Inman on the physical condition of the muscles in	41	58
" notice of Dr. Inman's work on	43	308
Myelitis, chronic, on the treatment of	42	39
Myopia, Mr. Solomon's treatment of	42	226
Nævus, Mr. Price on the surgical treatment of	44	100
Narcotic poisoning, Dr. Crook's treatment of	42	313
Needle, Mr. Price's, for the ready application of metallic sutures	42	165
Neuralgia, M. Bequerel on its treatment by electricity	42	49
" Dr. Sieveking on the treatment of	43	39
" facial, use of veratria in	43	41
" idiopathic peripheral, Mr. Lobb on	41	57
" intercostal, use of veratria in	43	40
" occurring in cases of amenorrhœa, treatment of	43	256
Nicotine, as an antidote to strychnine	41	304
Nipples, excoriated treatment of	41	249
" sore, Dr. Harris' treatment of	43	260
" treatment of excoriations and fissures of the	43	260
Nitrate of ammonia, Dr. Coles on	43	273
Nitric acid, Mr. Smith's method of applying to piles	41	323
" acid and opium, efficacy of, in diarrhœa	44	59
" acid, its use in the treatment of prolapsus ani	44	117
Nose, Dr. Nichol's case of union of a completely separated portion of	44	105
Obliquity of the fœtal head in the mechanism of parturition	44	215
Obstetric forceps, Dr. Hewitt on a modified form of	44	208
Obstetrical forceps, Dr. Knight's new form of	42	229
Obstetric forceps, Dr. Sinclair on the timely use of	44	209
Obstetric position, Mr. Hardy on a special	43	301
Odontalgia from carious teeth, Dr. Eisenmann's remedy for	41	39
Œgophony produced without effusion into the pleura	41	86
Oil of tar as a deodorant, Dr. Skinner on	42	269
Opaque capsule with adherent pupil, efficacy of iriddesis in	43	193
Operations during pregnancy	43	241

	PART	PAGE
Opiated colchicum wine, its value in rheumatic affections	41	39
Opium, Dr. Bodington on the physiological effects of	42	308
" as an antidote to poisoning by belladonna	42	302
" as a remedy in poisoning by datura	42	309
" its action on the system in various diseases	41	290
" on contraction of the pupil produced by	42	301
" plasters, Mr. Haselden's improved formula for	42	297
" use of, in poisoning by belladonna	41	308
Ophthalmia, rheumatic, Mr. Lobb's treatment of	42	227
Opthalmoscope and its application, Mr. Walton on	42	218
Os calcis, Dr. Barton's case of excision of a portion of the	44	80
Os uteri, Dr. Arnott on rigidity and dilation of the	43	210
Osseous anchylosis of the elbow in a faulty position	43	123
Ovarian disease, danger attending pregnancy complicated with	44	247
" disease, symptoms, diagnosis, and treatment of	44	261
" dropsy, Mr. Boinet on the diagnosis of	42	247
" dropsy, Dr. Simpson on injection of iodine in	41	229
" dropsy, Dr. Simpson on the surgical treatment of	41	227
" tumour, Dr. Tanner on the removal of an	43	249
" tumour, Mr. Fox's case of spontaneous cure of	43	252
" tumour, treated by injection of iodine	43	253
Ovariotomy, Mr. Baker Brown on the operation of	43	247
" Dr. Lloyd Roberts' successful case of	43	250
" Dr. Simpson on the operation of	41	234
" Dr. Tyler Smith on the causes of the mortality of	43	246
" Mr. Spencer Wells on	44	263
" Mr. Spencer Wells on the operation of	43	248
" Mr. Spencer Wells' cases of	41	240
" Mr. Wells' four cases of	42	251
" and its results, Dr. Clay on	44	256
Oxalate of cerium on gastric affections	43	58
Oxalic acid diathesis, Dr. Beale on the	43	314
Oxygen as a therapeutic agent, Dr. Birch on	41	291
Ozonized cod-liver oil, its influence on the pulse	43	48
Ozone, Dr. Hornidge on its influence on organic substances	44	228
Paget Mr. J., on perineal section in urethral stricture	41	182
Palpitation with proptosis, goitre, and cr., on a case of	43	284
Paracentesis in ovarian dropsy, Dr. Simpson on	41	236
Paralysis, Dr. Althaus on electricity as a means of diagnosis	41	54
" Dr. Brown Sequard on	44	34
" agitans, Dr. Reynold's treatment by galvanism	41	53
" Dr. Roberts on some of the more unusual forms of	43	30
" of the bronchial muscles	42	95
" of the lower extremities	42	40
" of the lower extremities, Dr. Brown Sequard on	43	34
Paraplegia, reflex, diagnosis and treatment of	42	29
" reflex, treatment of	44	30
Parasitic diseases of the skin, Dr. McCall Anderson on	43	154
Parasiticide applications	43	155
Parturition, Dr. Duncan on the obliquity of the fœtal head in	44	215
Pasteboard splints, invented by M. Merchi	41	132
Pathological histology of tuberculosis, Prof. Virchow on the	43	294
Pathology, experimental, M. Claude Bernard on	41	39
Pediculi capitis, M. Devergie on the treatment of	44	158
" corporis, treatment of	44	159
" pubis, treatment of	44	159
Pelvic abscess, Mr. Skey's cases of	44	122
" abscess after labour, Dr. Gibbs on	43	306
Perchloride of iron, Dr. Davenport's use of, in epistaxis	41	88
" of iron, on the effects of the	41	139
" of iron, use of, in hemorrhage from the tonsils	41	138
Perforation of the intestine, MM. Velpeau and Verneuil on	44	112
Perforator, cranial, Dr. Thomas'	43	219
Pericarditis, Dr. Gairdner on	43	45
" Dr. Markham on the treatment of	41	77
Perineal section, Mr. Cock's method of	43	312

	PART	PAGE.
Perineal section, Mr. Paget on	41	182
Perineum, Dr. Gray on its laceration during labour	44	230
" Dr. Hewitt on its laceration during labour	44	199
" Mr. Murphy on supporting during labour	44	201
" Dr. Skinner on its laceration during labour	44	203
Periodicity as a character of disease, Dr. Tilt on	42	286
Peroxide of hydrogen, Dr. Richardson on the	42	300
Pessary, Zwank's, as modified by Dr. Eulenburg	41	220
Phantom tumour on the womb, value of chloroform in	41	242
Pharyngoscope, Dr. Voltolini on the	44	173
Phosphate of lime in the urine in the crystalline form, Dr. Hassall on	42	131
" of lime in the urine, pathological importance of	42	133
Phosphates in the urine, Dr. Beale on the	41	107
Phosphorus, M. Scherer on its detection in cases of poisoning	42	313
Phthisis, Dr. Brown on its diagnosis by the microscope	42	82
" Dr. Dickson on its successful treatment by hypophosphites	41	84
Phthiriasis, Dr. McCall Anderson on the treatment of	44	158
" Dr. Quain on the hypophosphites of soda and lime in	41	83
" Dr. Smith on the treatment of	41	80
" Dr. Toulmin on the hygienic and medical aspects of	42	84
" Dr. Turle on chlorate of potash and sugar in	44	48
" Madeira and Cairo as climates for patients in	41	315
Phymosis, Mr. Price on the operation for	44	139
" M. Ridreau's new operation for	42	181
Pirogoff's operation, Mr. Fergusson's mode of performing	44	82
Pitting in small-pox, its prevention by lin. aq. calcis on cotton wool	44	150
" in small-pox, Dr. Stokes on the prevention of	41	185
" in small-pox, Dr. Stokes on the prevention of	42	193
Pityriasis, use of sea-bathing in	41	184
" capitis, use of glycerine in	42	193
Placenta, Mr. Clay on a new sign of post-mortem detachment of	43	223
" Mr. Stranger's case of accidental separation at the seventh month	44	186
" prævia, Dr. Barnes on the treatment of	44	188
" prævia, Dr. Clay on	44	193
Placental retention, Dr. Priestley on the treatment of cases of	44	185
Plasters, opium and belladonna, formulæ for	42	297
Pneumogastric nerve, Dr. Hughes on influence of belladonna on the	42	296
Pneumonia, Dr. Williams on the diagnosis of	44	44
" production of œgophony, Dr. Law's case of	41	86
" tubercular, Dr. Gairdner on antimony in	41	80
Pneumothorax, Dr. Jones' case of simulation of	41	87
Poisoning by belladonna, Mr. Seaton on	41	308
" by strychnia, Dr. Part on a case of recovery from	43	267
" by strychnine, case of recovery from	42	313
" narcotic, Dr. Crook's treatment of cases of	42	313
" on the detection of phosphorus in cases of	42	312
Poisons, peroxyde of hydrogen as an antidote for	42	300
Polypus of the ear, Mr. Bowman's treatment by torsion	41	206
" of the rectum, Mr. Bryant's treatment of	43	263
" of the rectum, hemorrhage from bowel in children a sign of	41	255
" uteri, Dr. Duncan's case of	43	255
Popliteal aneurism, its treatment by compression	44	97
" aneurism, new mode of making pressure in	42	157
Position in labour, on a special	43	301
Post-partum hemorrhage, Mr. Hubbert's treatment of	42	244
Potassa fusa, its use in the treatment of stricture	43	148
Pregnancy, Mr. Johnston's case of inversion of the bladder during	42	262
" Mr. Shaw's case of removal of warty growths during	43	241
" complicated with ovarian disease	44	246
" spurious, Dr. Priestley on	43	218
" with duplex uterus	42	241
Premature labour, Dr. Barnes on the induction of	44	180
" labour, Mr. Baker Brown on the induction of	44	181
" labour, Dr. Hall Davison on the induction of	44	182
" labour, Dr. Farre's case of induction of	44	184
" labour, Mr. James on the induction of	43	217

	PART	PAGE.
Premature labour, Dr. Priestly, on the induction of	44	182
" labour, Prof. Simpson on the induction of	44	178
" labour, Dr. Tyler Smith on the induction of	44	183
" labour, induction of, by Cohen's method	41	207
Presbyopia, Mr. Wells on	44	173
Preservation of bodies for anatomical purposes	42	280
Preservation of urinary deposits, Dr. Beale on	42	276
Probe gorget for median lithotomy, Mr. Teale's	41	159
" gorget for median lithotomy, Mr. Thompson's	41	171
Procidentia uteri from perineal laceration, Dr. Robert's case of	44	243
Prolapse of the rectum, Mr. Smith on the treatment of	43	136
Prolapsus ani, Mr. Coulson's treatment of	41	149
" ani, Mr. Smith's treatment of certain forms of	44	117
" ani of children, Foucher's treatment	43	263
Proptosis, goitre, palpitation, &c., Drs. Jones and Williams on	43	284
Prostate, Mr. Meade on inflammation and abscess of the	42	174
Prostate, Dr. Messer's report of the condition of, in old age	42	178
Prostate gland, unsuspected abscess of the	43	153
Prostatic dilator, Mr. Bowman's	41	171
" dilator, Mr. Teale's	41	161
" dilator, Mr. Todd's	41	172
Prurigo, Prof. Hebra's treatment of	43	165
Psoriasis, Dr. McCall Anderson on the diagnosis of	44	170
Puerperal convulsions, Dr. Harper's case of	44	196
" convulsions, Dr. Page on the use of chloroform in	44	198
" convulsions, Dr. Wilson's case of	44	199
" convulsions treated by subcutaneous injection of morphia	42	242
" hypochondriasis, Prof Simpson on the treatment of	43	234
" insanity, Prof Simpson on its connection with albuminuria	43	235
" mania, Prof. Simpson on the treatment of	43	232
Pulse, Dr. Thompson on the influence of ozonized cod-liver oil on	43	48
Punctum, Mr. Streatfield's new operation for the restoration of the	43	206
Pupil, effects produced by opium and belladonna on the	42	301
Quinic ether, its efficacy in intermittent fevers	42	25
Quinine and iron, new combination of	43	274
" Mr. Turner on arsenious acid as a substitute for	44	25
" its action on the system	41	289
Quinio, or rough quinine, M. Batka on	41	308
Radius, Dr. Gordon on fracture of the lower extremity of the	44	86
Recovery of the punctum by a new operation	43	206
Rectangular amputation, Mr. Teale on the advantage of	44	70
Recto-vaginal fistula, Mr. Wells on	44	233
Rectum, Mr. Brown on some diseases of, in woman	42	265
" Mr. Coulson's treatment of flat ulcers of the	44	122
" Mr. Lane on some diseases of the	44	114
" Mr. Rouse on the diagnosis of ulcer of the	42	172
" Mr. H. Smith on prolapse of the	43	136
" polypus of the, in a child, successfully removed	43	263
" prolapsed, treated by subcutaneous injection of sulphate of strychnia	43	263
" prolapsus of, its treatment where the ligature is inadmissible	44	117
Reflex paraplegia, Editor of the British Medical Journal on	44	29
" paraplegia, Dr. Sequard on the diagnosis and treatment of	42	29
Renal casts, Dr. Beale on	42	113
Reparative Process in Human Tendons, notice of Mr. Adam's work on	43	309
Resection of the knee-joint, Mr. Butcher's directions for	43	111
" on scooping of diseased bone as a substitute for	41	131
Respiration, importance of attention to, in certain stages of the brain	41	54
Rest, Mr. Hilton on its influence on diseases of the joints	44	71
Restoration of the tibia by the periosteum after its removal	44	90
Retroversion of the uterus, Dr. Tyler Smith on	43	212
" of the uterus, Dr. Skinner on reduction of	43	213
Rheumatic fever, Dr. Inman on the value of lime-juice in	41	37
" fever, Dr. Todd on treatment of	41	318
" ophthalmia treated by the galvanic current	42	227

	PART	PAGE
Rheumatism, Dr. Eisenmann on opiated colchicum wine in	41	38
" acute, Dr. Inman on	41	37
" acute, Dr. MacDonald on actea racemosa in	44	25
" acute, Prof. Reid's case of	44	26
" chronic, Dr. Arnott on its treatment by congelation	42	26
" treated by hypodermic injection of atropine	41	60
Rickets, Dr. Jenner on	41	250
Ricord's abortive treatment of gonorrhœa, Mr. Milton on	41	190
Ringworm of the head, treatment of	43	157
Rodent ulcer, Mr. Hutchinson on	42	190
Saccharine fermentation of the milk in the female breast	44	278
" urine, new test for	44	67
Salivary fistula, Mr. Butcher on	43	140
Santonine, its use in the treatment of lumbrici	42	102
Scabies, Mr. Godfrey on its treatment by benzine	44	157
Scalds of the glottis, Mr. Heath on the treatment of	43	311
Scarlatina, Dr. M'Nab on sesquichlorate of ammonia in	44	24
" M Bouchut on a pathognomonic sign of	44	23
Scarlatinal dropsy, use of benzoate of ammonia in	44	24
Scars from ulceration after suppuration of tubercular glands, treatment of	44	143
Sciatica, treated by hypodermic injection of atropine	41	60
Sclerotitis, acute, Mr. Laurence's treatment of	41	203
Scooping of bone as a substitute for resection	41	131
Scrofula, Dr. Bouchut on arseniate of soda in	43	27
Scrofulous chronic synovitis, Mr. Perrie on	43	112
" diseases of the joints, Mr. Barwell on	42	145
" scars, Mr. Hoffman's treatment of	43	151
Scrotal hernia, Prof. Chisholm's operation for	44	107
" hernia, Mr. Wood's operation for	44	106
Scurvy, Dr. S. Ward on the treatment of	43	26
Secondary abscess following scarlatina and parturition, Mr. Coulson on	44	240
" syphilis, case of hysteria masking	44	265
Sedative hypodermic inoculation, M. Lafargue on a simple mode of	43	285
Short-sight, Mr. Solomon on the surgical treatment of	42	226
Shoulder-joint, Mr. Coote on the treatment of contractions at the	43	116
" Mr. Skey on some complicated injuries about the	41	127
" Dr. Smith's treatment of dislocation of the	44	82
Silvester method of restoring persons apparently drowned	43	277
Sinuses, Mr. Hilton's treatment of	43	173
" Mr. Jessop on the surgical treatment of	43	178
Skin diseases, use of glycerine in	42	193
Sleep, Mr. Hilton on state of brain during sleep	42	71
Small-pox, Dr. Bell on the prevention of pitting in	44	150
" Dr. Stokes on the prevention of pitting in	41	185
" Dr. Stokes on preventing pitting in	42	193
Softening of the medulla spinalis, Dr. Mayne on	44	28
Solids in the urine, Dr. Seller on the proportion of	42	124
Sore nipples, Dr. Frazer's treatment of	43	260
" nipples, Dr. Harris' treatment of	43	260
Sores of a non-specific character, Mr. Harrison on	41	194
Speculum vaginal, Weiss' improved	44	233
Speech, causes of loss of	44	34
Spinal cord, Dr. Brown-Sequard on affections of the	42	29
Spinal curvature, Mr. Coote on the treatment of	42	151
" curvature, Mr. Coote on the treatment of	43	121
" marrow, Dr. Mayne on acute softening of the	44	28
Spine, Mr. Adams on anterior and posterior curvatures of the	44	91
Splints, pasteboard, Mr. Acton on the use of	41	132
Spontaneous evolution. Mr. Heslop's case of	44	226
Spurious pregnancy, Dr. Priestley on	43	218
Sputum uncertainty in the diagnosis of pneumonia	44	44
Squamous diseases, Dr. Moore's treatment of	41	184
St. Andrews' University, Dr. Braithwaite on	42	314
Stabs in the chest, Mr. Heath on the treatment of	43	310
Sterility and dysmenorrhea, Dr. Coghlan on	44	275

	PART	PAGE
Sthenic inflammation, Dr. Jones on medicines which arrest	41	288
Stomach pump, substitute for the	42	313
" ulcer of, on the diagnosis and treatment of	41	93
Stone in the bladder, Allarton's operation for	42	183
Stramonium, on its use in hay-asthma	42	93
" poisoning, opium as an antidote in	42	309
Strangulated hernia, Mr. Bryant on the use of chloroform in	42	167
" hernia, Mr. Jessop on the reduction of	42	166
Stricture, Mr. Shillitoe's new form of catheter for	42	180
" Mr. Wood's cases treated by his new urethrotome dilator	42	182
" of the urethra, Dr. Arnott on its treatment by incision	44	134
" of the urethra, Mr. Clark on some of the consequences of	43	144
" of the urethra, Mr. De Morgan on potassa fusa in	43	148
" of the urethra, Mr. Gay on its treatment by dilation	44	124
" of the urethra, Mr. Heath on forcible dilation in	44	132
" of the urethra, Mr. Hillman on forcible rupture of	41	179
" of the urethra, Mr. Holt's cases of forcible rupture of	44	126
" of the urethra, Mr. Holt's treatment by forcible rupture	41	179
" of the urethra, Mr. Marshall's urethrotome for	41	177
" of the urethra, Mr. Paget on perineal section in	41	182
" of the urethra, Mr. Smith's new guage for catheters	43	147
" of the urethra, new instrument for cutting, dilating, and cauterizing	43	152
Strumous synovitis, Mr. Barwell on the treatment of	42	147
Strychnine, Dr. Davy on tests for	41	303
" Prof. Kurzak on tannin as an antidote to	42	311
" Dr. Leared on its efficacy in dyspepsia	41	322
" Dr. Part's treatment of a case of poisoning by	43	267
" camphor an antidote to	41	53
" indications and contra-indications to its use in paralysis	43	35
" its action on the system of diseases	41	289
" its efficacy in cases of bronchocele	41	290
" poisoning, Dr. Bennet's case of recovery from	41	61
" poisoning, Dr. Part's case of recovery from	42	313
Stumps, on the dressing of, after amputations	42	152
Styptic, on the use of larch-bark as a	42	77
Subcutaneous ligature of varicose veins of the leg	43	179
Sub-involution of the uterus after delivery, Prof. Simpson on	43	224
Subnitrate of bismuth injections for gonorrhœa	44	166
Sugar in diabetic urine, Dr. Roberts on estimating the quantity of	44	64
" in the urine, on the tests for	42	118
" in the urine, M. Musset on	43	99
" in the urine, Dr. Pavy on the tests	43	79
" in the urine, on detecting and estimating the quantity of	42	123
Sulphate of zinc in cancer of the breast	43	260
Sun-stroke, Dr. Gordon on its prevalence among soldiers in India	42	73
Super-involution of the uterus after delivery, Prof Simpson on	43	229
Suspended animation, instrument for inflating the lungs in	43	261
Suture, Mr. Startin's new, without a ligature	41	282
" wire, Dr. Levi's new needle for	41	284
Sycosis, treatment of	43	156
Symblepharon, Mr. Teale on the treatment of	44	172
Synthesis, on the study of diseases by	41	47
Syphilis, hereditary, treatment of	43	309
" Mr. H. Lee on	44	161
" on symptoms simulating	41	195
Syphilitic degeneration of the decidua	44	195
" iritis, Mr. Walton on	42	224
" psoriasis, diagnosis from non-syphilitic	44	170
Talipes equinus, on the treatment of	42	155
Tamplin's apparatus for lateral curvature of the spine	42	151
Tannin as an antidote to strychnine	42	311
" pencils in certain affections of the uterus	41	246
Tapping an ovarian dropsy, Dr. Simpson's operation for	41	227
Tartar emetic, injection of, in tetanus	41	287
Tar-water, its use as a deodorant in carcinoma	41	244

	PART	PAGE.
Tea and coffee, Dr. Day on the action of	43	57
Teeth, artificial, on the use of coralline for	43	140
Teething, Dr. Jacobi on popular errors respecting	43	264
Tests for strychnine, Dr. Davy on the	41	303
Tetanus, cases illustrating the treatment of	43	43
" essentially a zymotic disease	41	28
" indications in the treatment of	43	43
" its treatment by tartar emetic	41	287
" Mr. Hilton's clinical remarks on a case of	41	50
" Mr. F. C. Skey on	42	42
" Mr. S. Wells' case treated by woorara	41	47
" traumatic, case treated by internal use of chloroform	41	55
" traumatic, its treatment by tincture of aconite	42	46
Thread seton, its use in the treatment of bursal tumours	44	80
Throat, Mr. Price's new instrument for examining	41	142
" ulcerated, treatment of	44	106
Thrush, caused by excessive and fruitless sucking	41	328
Tibia, M. Maisonneuve on its restoration by the periosteum	44	90
Tincture of actea, its use in puerperal hypochondriasis	43	233
" of actea, its use in the neuralgia of amenorrhœa	43	257
Tinea decalvans, treatment of	43	167
" favosa treated by the carbonate of copper	43	165
" tonsurans, treatment of	43	157
Tobacco, Dr. Day on the action of	43	57
" Dr. Osborne on a peculiar symptom produced by	43	286
Toe-nail, ingrowing of	43	179
Tongue, furred, curious cases of, from various causes	44	53
" Mr. Price's instrument for examining the base of the	41	142
Tonic, use of nitrate of ammonia as a	43	273
Tonsils, Mr. Thompson's case of hemorrhage from, arrested by perchloride of iron	41	138
Trachea, use of looped wire for removing foreign bodies from	43	50
Tracheotomy, Dr. Lawford's new hook for	41	142
" Mr. Maunder on	43	285
" in croup, Mr. Spence on	41	89
Traumatic aneurism of the femoral artery	44	96
Trephine, Dr. Galt's new	42	157
Trichiasis, Dr. Williams' new treatment of	43	205
Trichina spiralis, artificial propagation of	42	284
" spiralis, fatal disease produced by the	42	284
Tubercular consumption, Dr. Touhain on	42	84
" disease of the external lymphatic glands	44	143
Tuberculotis, Virchow on the pathological histology of	43	294
Tumour, hydatid, of the liver	42	103
" in the abdomen, Dr. Jones' case of, with supposed pregnancy	42	255
" mammary glandular, Mr. Birkett's case of	42	261
" of the breast, Mr. Erichsen on the diagnosis of	42	256
" of the uterus, spontaneous expulsion of a	42	245
Tumours, non-cancerous, Dr. Collis on	43	286
" of the breast, Mr. Erichsen on the diagnosis of	41	244
" uterine, Dr. Davis on removal of, by enucleation	43	307
" uterine fibrous, treatment of	43	242
" vascular, on the surgical treatment of	44	100
Turkish bath, Dr. Golden on the	43	279
" bath, Dr. Richardson on the	43	282
" bath, Dr. Taylor on a substitute for the	43	282
Turning, as a substitute for the forceps, Mr. Jones on	41	215
" by external manipulation, Dr. Hicks on	42	235
" Dr. Barnes' case illustrating the value of	41	213
" in cases of contracted pelvic brim, Dr. Barnes on	41	212
" in cases of contracted pelvic brim, Dr. Barnes on	42	240
" in cases of pelvic distortion, Mr. Pearson on	43	218
Turpentine, rectified spirit of, as an anæsthetic	43	274
Typhoid fever, Dr. Brinton on the treatment of	42	24
" fever, Dr. Tweedie on	41	19
" fever, Dr. Wardell on	43	24

	PART	PAGE.
Typhoid and typhus fever, Dr. Bell on the identity of	43	17
Typhus fever, Dr. Bell on	44	20
" fever, Dr. Tweedie on its treatment	42	21
" fever, on condition of the heart in	41	35
" fever, on the symptoms and pathology of	41	22
" and enteric fever, Dr. Gairdner on the distinctions between	42	18
" and typhoid fever, Dr. Kennedy on	42	17
Ulcer of the rectum, Mr. Rouse on the treatment of	42	173
" of the stomach, Dr. Wade on the diagnosis and treatment of	41	93
" rodent, Mr. Hutchinson	42	190
Ulcerated bowel of typhoid fever, pathology of	41	19
" throat, Mr. Westcott's treatment of	44	106
Ulceration, diphtheritic, of the cervix uteri	44	253
" inflammatory, of the uterus	44	253
Ulcers, Mr. Hilton's treatment of	43	174
" of the rectum, treatment of	44	122
Ulna, Mr. Canton's case of dislocation of the	42	150
Union by the first intention, causes of failure of	41	137
" great difficulty in procuring, in laceration of the upper eyelid	41	201
University of St. Andrews, on the degrees granted by the	42	314
Uræmia, Dr. Richardson on	43	76
Uræmic intoxication, Dr. Hammond on	44	62
Urea, Dr. Beale on the formation of	43	313
" Dr. Day on the influence of various diseases on the excretion of	43	74
" Dr. Day on the origin of	43	73
Urethra, Mr. Hunter on irritability of, in females	44	265
" stricture of, Dr. Arnott on incision of,	44	134
" stricture of, Mr. Gray on dilatation in	44	125
" stricture of, Mr. Heath on forcible dilatation in	44	132
" stricture of, Mr. Holt on the immediate treatment of	44	126
Urethrotome dilator, Mr. Wood's new	42	182
" Mr. Marshall's, for obstinate strictures	41	177
Uric acid and urates, Dr. Beale on	41	106
Urinary deposits, Dr. Beale on the preservation of	42	276
" deposits, importance of giving increased quantities of fluids	43	315
Urine, Dr. Beale on the clinical importance of albumen in the	42	111
" Dr. Beale on increased acidity of the	43	314
" Dr. Beale on pus in the	43	315
" Dr. Beale on excess of urea in the	43	314
" Dr. Beale on the chloride of sodium in	41	107
" Dr. Beale on the phosphates in	41	109
" Dr. Beale on the tests for albumen in the	42	107
" Dr. Beale on the tests for sugar in the	42	118
" Dr. Hassall on the occurrence of crystalline phosphate in	42	131
" Dr. M'Ghie on detecting and estimating the quantity of sugar in	42	123
" Dr. Roberts' observations on the daily changes of the	41	98
" Dr. Seller on the determination of the amount of solids in	42	124
" diabetic, clinical method of estimating the sugar in	44	64
" in albuminuria, Dr. Beale on the	44	63
" new test for sugar in	44	67
" rapid excretion of substances in the	42	101
" Urinary deposits, and Calculi, notice of Dr. Beale's work on	43	313
Urticaria, use of local applications in	43	168
Uterine diseases, on affections of the hip-joint, in consequence of	43	254
" fibro-plastic tumour, removed by enucleation	43	307
" fibrous tumours, Mr. Baker Brown's treatment of	43	242
" fibrous tumours, Mr. Harper's instruments for curing	43	245
" hemorrhage, Dr. Hamilton on	44	228
" hemorrhage, influence of contraction in arresting	44	195
" hemorrhage, use of larch-bark in	42	77
" pessary, Dr. Nœggerath on a new	41	216
" polypus, Dr. Matthews Duncan's case of	43	255
Uterus, Mr. Ellis on diphtheric ulcer of the	44	250
" Mr. Ellis on inflamed ulcer of the	44	252
" Dr. Greenhalgh on removal of the neck of the	44	255

	PART	PAGE.
Uterus, Prof. Simpson on sub-involution of the	43	224
" Prof. Simpson on super-involution of the	43	229
" Dr. Skinner on reduction of retroversion of	43	213
" Dr. Swayne on spontaneous expulsion of a fibrous tumour of	42	245
" Dr. Tyler Smith on retroversion of the	43	212
" Dr. Whitehead on the effects of undue application of caustic potash to	44	256
" treatment of superficial inflammation of neck of	42	264
Vaginal discharges, injection of tar-water as a deodorant for	42	269
" speculum, Weiss' improved	44	233
Vaginitis, Dr. Foucher on the use of tannin ointment in	42	264
" Dr. Tilt on the treatment of	43	254
" symptoms and treatment of	44	267
Varices, etc., use of Startin's bar-needle and clasp for	42	160
Varicocele, Mr. Davies on the radical cure of	44	141
" Mr. Lee on the radical cure of	44	140
" Mr. Bowman's treatment by ligature and sub-cutaneous section	41	183
" Mr. Erichsen's treatment of	41	139
Varicose veins of the leg, Dr. Levis' operation for	43	179
" veins and ulcers treated by blisters, Mr. Ure's case of	44	99
" veins, Mr. Wood on the radical cure of	44	103
" veins of the legs, Mr. Erichsen's treatment of	41	139
Vascular tumours, Mr. Price on the surgical treatment of	44	100
Vena porta, cases of obliteration of the	43	61
Ventilation, McKinnell's system of	42	282
Veratrum viride, Dr. Otterson on	44	296
Vesico-vaginal fistulæ, needle for application of metallic sutures in	42	165
" fistulæ, Dr. Battey's new operation for	41	225
" fistulæ, Dr. Baker Brown's new operation for	41	222
" fistulæ, Mr. Baker Brown on the operation for	43	240
" fistulæ, Mr. Collis' treatment of	44	236
" fistulæ, Mr. Hilliard's new instruments for	43	237
" and recto-vaginal fistulæ, Mr. Wells on	44	233
Vibriones in human milk, Dr. Gibb on	44	278
Vidal's operation for varicocele	41	141
Vitreous humour, Mr. Walton on disease of the	42	222
Vomiting as an antidote to poisoning by strychnia	43	273
" obstinate spasmodic, use of belladonna in	42	297
Waxy kidney occurring as an independent form	43	299
Webb's Ready Rules for Operations, notice of	43	312
Webbed fingers, Mr. Murray's case of	42	273
Weight of new-born infants	44	279
Wens, M. Courty's treatment of, without excision	44	147
Wire suture, Dr. Levis' needle	41	284
Woorara in treatment of tetanus	41	47
" M. Bouvier on its varieties and effects	41	47
Wounds of joints, Mr. Heath on the treatment of	43	311
" Mr. Henry on the treatment of	41	189
Wrist, ganglions of, treated by sub-cutaneous division	41	377
Zymosis, Dr. Richardson on	41	28

THE

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OF

PRACTICAL MEDICINE AND SURGERY,

BEING

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CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

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AND

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CONTENTS OF VOL. XLV.

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ARTICLE.		AUTHOR.	PAGE.
1	Does Intestinal Fever ever arise spontaneously?	*Dr. William Budd*	17
2	On the Causation of Typhoid Fever	*J. Simon, Esq.*	21
3	On Typhus Fever	*Dr. Gull*	23
4	On Typhus Fever	*Prof. Griesinger*	24
5	On the Renewal of Life in Continued Fever	*Dr. T. K. Chambers*	25
6	On the Employment of Quinine in Fevers and other Diseases	*Dr. J. C. Cameron*	27
7	Anarcotine in Remittent Fevers		29
8	On the Albuminuria of Fever	*Dr. George Johnson*	30
9	On the External Use of the Solution of Pernitrate of Mercury in Epithelial Cancer, Lupus Exedens, and the Induration of Chancre	*John Gray, Esq.*	31
10	Cases of Cancer successfully Treated with the Arsenical Mucilage	*Dr. W. Marsden*	33
11	On Acupuncture in the Treatment of Muscular Rheumatism, and on Galvanism in Rheumatic Paralysis	*Dr. Arthur Leared*	35
12	On the Treatment of Rheumatic Fever, and on the Use of Alcohol in Serious Cases of this Disease	*Dr. Lionel S. Beale*	37
13	Indian Remedy for Small-pox	*Dr. M'William*	38
14	On Anæmia	*Dr. T. K. Chambers*	38

DISEASES OF THE NERVOUS SYSTEM.

15	On the Physiological and Medicinal Properties of Sulphate of Aniline, and its Use in the Treatment of Chorea	*Dr. James Turnbull*	42
16	Cases of Chorea Treated by Sulphate of Aniline	*Dr. Fraser*	47
17	Cases of Severe Neuralgia Treated by Valerianate of Ammonia	*Dr. O'Connor*	48
18	Bromide and Iodide of Potassium in Epilepsy	*Dr. Wilks*	49
19	Epilepsy Suddenly and Remarkably Relieved by the Use of Arsenic	*Dr. Bristowe*	51
20	Digitalis in Delirium Tremens	*Dr. Robert Duchesne*	52
21	Capsicum in Delirium Tremens	*Chas. Ferneley, Esq.*	53

ORGANS OF CIRCULATION.

22	On Regurgitant Aortic Diseases of the Heart	*Dr. Hyde Salter*	53

ORGANS OF RESPIRATION.

ARTICLE.		AUTHOR.	PAGE.
23	On Pneumonia	*Dr. C. J. B. Williams*	60
24	The Chronic Results of Pleurisy and Pneumonia	*Do.*	61
25	On Gangrenous Abscess of the Lung	*Do.*	62
26	On Pneumonia	*Dr. Edward Copeman*	63
27	On Pleurisy	*Dr. C. J. B. Williams*	65
28	On the Treatment of Tubercular Hemoptysis	*Dr. William Brinton*	66
29	On the Pathology of Asthma	*Dr. Hyde Salter*	68
30	On Asthma with Emphysema	*Dr. C. J. B. Williams*	74
31	On the Action of the Bronchial Muscles	*Dr. C. Radcliffe Hall*	75
32	On a Simple and Efficient Method of Performing Artificial Respiration	*Dr. William Marcet*	78
33	On Hæmoptysis	*Dr. C. J. B. Williams*	80

ORGANS OF DIGESTION.

34	On Aphthous Inflammation, and on Ulcer of the Tongue	*Thomas Bryant, Esq.*	81
35	On Tumours and Deep-seated Ulcerations of the Tongue	*Do.*	81
36	On Diphtheria	*Dr. E. L. Ormerod*	83
37	On the Treatment of Diphtheria	*Dr. J. West Walker*	85
38	Sulphuric Acid Mixture in Diarrhœa	*Dr. Andrew Clark*	86
39	On Coloration of the Fæces	*Dr. Thomas Inman*	86
40	On the Cause and Cure of Tape-Worm	*Dr. Fleming*	88

URINARY ORGANS.

41	On Hæmaturia	*Dr. Hyde Salter*	89
42	"Dumb Bells" of Oxalate of Lime, from a Clinical Point of View	*Dr. Lionel S. Beale*	90
43	Recovery from Bright's Disease	*Dr. Thos. Williams*	92
44	Albuminous Urine	*Metcalfe Johnson, Esq.*	95
45	The Influence of Arsenic upon Albuminuria in a Case complicated with Psoriasis and Lichen	*Dr. Fred. Furre*	95
46	On the Influence of Alkalies in Checking the Production of Artificial Diabetes	*Dr. F. W. Pavy*	96
47	The Influence of an Acid in producing a Diabetic State of the Urine	*Do.*	96
48	On the Diabetic Treatment of Diabetes	*Do.*	97
49	Gluten Biscuits for the Diabetic	*Dr. John Camplin*	98
50	On the Diet of Diabetic Patients	*Dr. F. W. Pavy*	98

SURGERY.

AFFECTIONS OF BONES AND JOINTS, ETC.

51	On Amputation by Rectangular Flaps	*Oliver Pemberton, Esq.*	100
52	On Amputation of the Forearm	*Prescot Hewett, Esq.*	102
53	On the Therapeutic Influence of Mechanical and Physiological Rest in Accidents and Surgical Diseases	*John Hilton, Esq.*	102
54	On Diagnosis of Disease of the Hip-joint	*Do.*	105
55	On Straightening of Contracted Joints	*James Paget, Esq.*	113
56	Treatment of "Housemaid's Knee"	*R. W. Martyn, Esq.*	113
57	On a Novel and Efficient Method of Reduction in Dislocation of the Shoulder-Joint	*Prof. N. R. Smith*	114

ARTICLE.		AUTHOR.	PAGE.
58	On the Treatment of Lateral Curvature of the Spine	*William Adams, Esq.*	115
59	On Certain Grave Evils attending Tenotomy, and on a New Method of Curing Deformities of the Feet	*Richard Barwell, Esq.*	120
60	On the Plaster of Paris Splint	*Dr. Little*	123
61	On some injuries of the Head	*James F. West, Esq.*	124
	ORGANS OF CIRCULATION.		
62	Aneurism of the Femoral Artery—Embolia—Obstruction by Sphacelus of the Leg and Foot—Amputation—Recovery	*Dr. A. Leith Adams*	125
63	On the Treatment of Aneurism of the Extremities by Flexion of the Limb	*Earnest Hart, Esq.*	126
64	New Mode of Making Compression in Popliteal Aneurism	*Dr. Bland*	130
65	On Plugging the Posterior Nares	*R. Macnamara*	131
	ALIMENTARY CANAL.		
66	On Tracheotomy	*A. M. M'Whinnie, Esq.*	135
67	On Strangulated Hernia	*Thomas Bryant, Esq.*	139
68	On Opening or not Opening the Sac in Strangulated Hernia, with Practical Conclusions	*Do.*	142
69	On the Taxis in Strangulated Hernia	*F. Le Gros Clark, Esq.*	145
70	On the Radical Cure of Reducible Hernia	*Redfern Davies, Esq.*	146
71	On a New Operation for the Cure of Umbilical Hernia	*Richard Barwell, Esq.*	149
72	On the Medical Treatment of Strangulated Hernia	*W. Blower, Esq.*	150
73	Artificial Anus Resulting from Sloughing Hernia		152
74	On the Pathology and Treatment of Hemorrhoidal Affections	*T. J. Ashton, Esq.*	152
75	Case of Successful Removal of the Entire Tongue for Cancer of that Organ	*Thos. Nunneley, Esq.*	155
76	On Division of the Gustatory Nerve and on Ligature of the Lingual Artery, in the Treatment of Cancer of the Tongue	*Chas. H. Moore, Esq.*	158
77	On Cleft Palate	*A. G. Field, Esq.*	158
	ORGANS OF URINE AND GENERATION.		
78	On Congenital Stricture of the Urethra	*James Syme, Esq.*	160
79	On Practical Lithotomy and Lithotrity	*Henry Thompson, Esq.*	161
80	On the Causes of Death following Lithotomy	*Do.*	164
81	On Lithotomy in the Female	*F. Le Gros Clark, Esq.*	168
82	On Lithotomy	*Do.*	168
83	On Varicocele	*Henry Lee, Esq.*	171
84	On the Radical Cure of Varicocele by Subcutaneous Ligature of the Spermatic Veins	*Jolliffe Tufnell, Esq.*	172
85	The Wire Seton in Hydrocele	*Dr. J. D. Gillespie*	174
86	How to Relieve Pain in Diseased Bladder		176
	SYPHILITIC AFFECTIONS.		
87	Syphilis Conveyed by the Vaccine Lymph to Forty-six Children		176

ARTICLE.		AUTHOR.	PAGE.
88	Case of Syphilitic Disease Appearing after Vaccination from a Syphilitic Child	*Dr. N. J. Haydon*	178
89	On the Calomel Vapour Bath	*Henry Lee, Esq.*	179

DISEASES OF THE SKIN.

90	On Ringworm and Vegetable Parasites	*Dr. Thomas Hillier*	180
91	On the Treatment of Scalds and Burns	*Dr. J. Young Myrtle*	183
92	On the Treatment of Varicose Ulcers of the Leg without Rest	*John K. Spender, Esq.*	184
93	On the Treatment of Indolent Ulcers	*J. Mitchell, Esq.*	185
94	On the Treatment of Goitre with the Biniodide of Mercury Ointment	*H. M. Greenhow, Esq.*	185

DISEASES OF THE EYE AND EAR.

95	On the Use of Electricity in Certain Forms of Paralysis of the Muscles of the Eye	*Dr. Soelberg Wells*	187
96	On the Insufflation of Calomel in Certain Diseases of the Eye	*Do.*	189
97	On the Use of Forceps in Extraction of Cataract	*John F. Vrance, Esq.*	190
98	On Paracentesis of the Cornea	*George Lawson, Esq.*	191
99	On the Advantages of having some Eye-Instruments Gilded, with a Note on Non-metallic Instruments	*J. F. Streatfeild, Esq.*	191
100	On the Use of Atropine-Paper	*Do.*	192

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

101	On Dysmenorrhœa and Sterility	*Dr. John Coghlan*	195
102	Veratrine in Painful Menstruation		197
103	Retained Menses of Two Years' Duration, caused by Atresia Vaginæ, and Treated by Puncture of the Uterus per Rectum	*I. Baker Brown, Esq.*	197
104	Living Spermatozoa in the Vagina Eight Days and a Half after Intercourse		198
105	Use of Oxalate of Cerium in the Vomiting of Pregnancy	*Albert Lloyd, Esq.*	198
106	Use of Pepsine in the Vomiting of Pregnancy	*Dr. G. Selwyn Morris*	199
107	Retroversion of the Gravid Uterus: the best Position During its Reduction	*Dr. Thomas Skinner*	199
108	On a New Mode of Administering Chloroform	*Dr. J. Y. Simpson*	200
109	Induction of Premature Labour in a Case of Albuminuria with Puerperal Convulsions	*Dr. Robert Barnes*	201
110	Cases of Concealed Accidental Hemorrhage at the Full Term of Utero-Gestation	*Dr. J. G. Wilson*	203
111	On the Management of the Placenta	*Dr. P. McLosky*	205
112	Partial Placental-Presentation at Eight Months	*Dr. F. R. Ramsbotham*	206
113	On Preparing for Turning in Dry Labours	*Dr. Langer*	206
114	A New Instrument for Craniotomy	*Dr. Lewis D. Harlow*	207
115	On the Uterine Douche as a Therapeutic Agent: New Instrument	*Dr. Graily Hewitt*	208
116	Arrest of Secretion of Milk	*Dr. Harries*	209
117	On Puerperal Fever regarded as an Epidemic	*Dr. R. Uvedale West*	209
118	On Puerperal Fever		210
119	On Puerperal Fever	*Dr. Fox and Dr. Tyler Smith*	212

ARTICLE.		AUTHOR.	PAGE.
120	The Treatment of Child-bed Fever		213
121	On the Pathological Anatomy of Puerperal Fever	*Prof. Buhl*	214
122	On Painful Muscular and Fascial Contractions along the Vaginal Canal	*Dr. J. Y. Simpson*	215
123	New Injecting Apparatus	*Do.*	216
124	Removal of a Fibrous Tumour from within the Abdomen	*I. Baker Brown, Esq.*	217
125	On Cauliflower Excrescence of the Os Uteri	*Dr. J. Braxton Hicks*	218
126	On Cauterisation by Electric Heat in the Treatment of Certain Diseases of Women	*Robert Ellis, Esq.*	220
127	On Ulcers of the Os and Cervix Uteri	*Do.*	221
128	On the Treatment of Large Ovarian Cysts and Tumours	*T. Spencer Wells, Esq.*	223
129	On Ovariotomy; the Mode of its Performance, and the Results obtained at the London Surgical Home	*I. Baker Brown, Esq.*	226
130	Trochar-Syringe for Injecting Ovarian Cysts		228
131	On Vesico-Vaginal Fistula	*Dr. Marion Sims*	229
132	Intra-Uterine Fibrous Polypus; Removal by Dr. Hicks' Wire-rope Ecraseur	*Dr. J. Braxton Hicks*	231
133	Treatment of Encysted Tumour of the Labium by Free Incision	—— *Tatum, Esq.*	231
134	Encysted Tumour of the Labium: Ablation of the Entire Cyst	*I. Baker Brown, Esq., and* —— *Hillman, Esq.*	232
135	On Substances Expelled from the Generative Passages: their Nature and Diagnostic Significance	*Dr. Grailly Hewitt*	232
136	Instrument for the Removal of Polypi of the Uterus	*Dr. Tyler Smith*	237
137	Case of Uterine Hydatids	*Thomas Tweedale, Esq.*	237
138	Tuberculosis and Uterine Disease		236
139	On Nævus	*Thomas Bryant, Esq.*	238
140	On the Treatment of Nævus	*F. Le Gros Clark, Esq.*	239
141	Nævus in the Centre of the Forehead of an Infant: Treatment by Excision	*J. Gay, Esq.*	242
142	On the Removal of Nævi	*J. C. Christophers, Esq.*	243
143	On the State of the Pupil in Hydrocephalus	*Dr. S. Wilks*	244
144	On the Treatment of Diarrhœa in Children	*Do.*	244

MISCELLANEOUS SUBJECTS.

145	On the Discrimination of Urinary Deposits by the Unaided Senses	*Dr. William Roberts*	246
146	On Arterial Murmurs in Incipient Phthisis	*Dr. W. S. Kirkes*	248
147	New Fracture Apparatus	*C. Evans, Esq.*	252
148	On the Substitution of Iron Wire for Thread and Silk as Ligatures for Arteries	*Thomas Nunneley, Esq.*	253
149	Chloroform and Chloroform Julep	*Dr. Thomas Skinner*	253
150	On Practical Lithotomy and Lithotrity	*Henry Thompson, Esq.*	255
151	The Alkaline Hypophosphites and their Medicinal Properties	*John Taylor, Esq.*	258
152	On Uranoplastics	*George Pollock, Esq.*	269
153	Lallemand, Perrin, and Duroy on Alcohol		270
154	On Podophyllin	*Edit. of Lancet*	271
155	On Podophyllin as an Aperient		276
156	On the Hypodermic Action of Atropia	*J. Kent Spender, Esq.*	277
157	On the Action and Uses of Atropia	*Dr. Fleming*	278
158	On the Use of Nux Vomica as an Antidote in Cases of Poisoning by Aconite	*Dr. D. D. Hanson*	279
159	The Treatment of Suspended Animation under the Influence of Chloroform	*Dr. William Marcet*	280
160	On the Lithia Springs of Baden-Baden	*Dr. J. Althaus*	281

ARTICLE.		AUTHOR.	PAGE.
161	On the Sulphuric and Gallic Acids. . .	*Dr. Lumly Earle*	283
162	New Remedy—A New Salt of Iron . .	*T. L. Gentles, Esq.*	283
163	On Codeia	*Dr. Aran*	284
164	Case of Poisoning by Almond Flavour .	*Dr. T. Herbert Barker*	285
165	Biniodide of Mercury Ointment in Goitre .	*T. Stainthorpe, Esq.*	286
166	On Vesico-Vaginal Fistula	*I. Baker Brown, Esq.*	287

SYNOPSIS.

A COMMENTARY ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN.

INDEX.

PRACTICAL MEDICINE,

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—DOES INTESTINAL FEVER EVER ARISE SPONTANEOUSLY?

By Dr. WILLIAM BUDD, Clifton, Senior Physician to the Bristol Royal Infirmary.

[Cases of small-pox are constantly arising, which, like cases of fever, cannot be traced to contagion. Yet no one supposes that small-pox, whatever its first origin, ever now arises *de novo*. If we are to believe in the spontaneous origin of typhoid fever, we must hold the same of small-pox also. But it is obvious that by the same rule we must believe in the spontaneous origin of plants and animals.]

Spontaneous generation is a pure hypothesis, but of all hypotheses the most gratuitous. And if, in reply to this, it were argued that it is much easier to conceive of a contagious fever originating anew, than of a plant or an animal—a position which must, however, by no means be taken for granted—the answer is that this is not at all the question at issue; that question being simply what is the value of the evidence which has been supposed to render such a supposition necessary in either case.

But we have already seen that this evidence is negative only, and consists solely in our inability to trace with the eye the continuity of a chain whose connecting links are known to be invisible. To conclude from this that no chain exists, is palpably absurd.

But nothing, I repeat, can show more forcibly *how* absurd it is, than the fact that the evidence, were it worth anything, would prove the spontaneous generation of plants and animals.

Stated in the form of syllogism, the position is this:—Animals and plants are not generated spontaneously, but propagate by the law of continuous succession. It is impossible, nevertheless, in countless instances, to trace their lineage. An impossibility of tracing lineage is, therefore, of no force whatever as an argument against propagation by continuous succession.

This reasoning is as simple as it is conclusive. The steps are few; every step is sure; and the conclusion to which they lead cannot be evaded.

The analogy of living types may, however, be employed to illustrate the phenomena of contagious fevers, not only in this negative sense, but, as showing, by *positive* evidence, what are the invariable characteristics which attach to the dissemination of entities *known to propagate by the law of continuous succession*, when this propagation is effected by invisible germs.

To spring up in unexpected places, and in places where their parentage, not only cannot be traced, but is, at first sight, sometimes even difficult to conceive of; to occur now in a sporadic way, and now over large areas at once; to be powerfully affected by season and climate, but in a manner to show that the influence of season and climate is secondary only, is the law that runs through them all. I need scarcely say what a counterpart this history is to that of the dissemination of the great group of epidemic and contagious disorders.

It would be easy to carry this parallel further, if need were. Sir H. Holland, in his interesting essay On the Hypothesis of Insect Life as a Cause of Disease, has, however, dealt with it in a way which enables me to dispense with any larger development of it here. I will only remark that the time is not so very remote, when, from the impossibility of tracing their descent from actual progenitors, animals and plants of well known species, and known to be endowed with enormous reproductive powers, were also universally believed to be constantly generated spontaneously.

Under the pressure of advancing science, this figment has disappeared, and the law of specific propagation by continuous succession has been finally established in its place. And I think we may plainly discern, not only from the perfect analogy in all essential points of the relations which bear on the question in the two cases, but from the course which opinion has already taken in both, that the time is not far distant when the same conclusion will be extended also to the diseases which propagate by a specific contagion.

Nothing, in the history of this question, is more curious than to see how slow naturalists were to relinquish the idea of spontaneous generation, and with what tenacity they clung to it.

"It is probable," says Plutarch, "that the first generation has been made entire and complete of earth—that is to say, by spontaneous generation." He admitted, however, that in his time, *mice* only were formed in this manner.

"Aristotle limited spontaneous generation to *insects*, to *molluscs*, and *fishes*"—that is, says M. Flourens (from whom I quote these and the following two passages), *to animals of which he knew not the true mode of generation.*

"A physiologist of the present day, M. Burdach, admits spontaneous generation for *fishes*, but he adds that it would be *too bold* to admit this for toads and frogs."

"M de Lamarck finds that the *polypus* is too complicated to be produced by spontaneous generation; but he says the *monad* can be produced in that manner. Ehrenberg, who has dissected animals still smaller than the monad and who has discovered in them a structure so marvellous, carefully avoids saying this."

It was but the other day, so to speak, that mildew was universally believed (as it still is by uneducated people) to be the direct offspring of damp, or to be generated by the decay of other organisms; and so, in like manner, until quite lately, it was believed by many that the *itch* was often actually bred by want of cleanliness. When, however, it was discovered that itch is the effect of a peculiar parasite, this notion, too, had to be given up. Fifty years ago, small-pox was very commonly imagined often to originate spontaneously; or, as the phrase was, "to be bred in the blood." Even Gregory went so far as to allow that this view might be supported by very plausible considerations. In the present day, intestinal fever, cholera, and yellow fever, are believed often to arise *de novo;* but most well informed physicians are of opinion that it would be "*too bold to admit this for small-pox.*" When the true mode of propagation of intestinal fever, cholera, and yellow fever, come to be generally recognised, it will be seen at once that it is quite as bold to teach that these diseases spring up spontaneously, as to teach that small-pox does so.

[One argument of the partisans of spontaneous origin is this—As all contagious diseases must in the *first* intance have occurred independently of contagion, there is every reason to believe they still do so.]

M. Trousseau, who, although an avowed contagionist, is an equally firm believer in "spontaneity," has put the case very clearly. After observing that the spontaneous origin of contagious disorders is not only a fact that cannot be contested, but is an important element in the development even of the most contagious among them, he adds:—

"In fact, as contagion implies necessarily the presence of two persons, the one giving and the other receiving the morbific germ, it is too obvious that in the *first* person attacked by a contagious malady the disease must have developed itself spontaneously, and was formed out of its prime elements, under the influence of causes which are completely unknown to us."

Following up this view, this distinguished teacher suggests that, many cases in which diseases of this kind spring up in isolated places can only be explained by supposing that the same thing still happens.

Small-pox, that is to say, having once originated *de novo*, must originate *de novo* now. For that, if it is to have any force at all, is what the argument amounts to.

But it is obvious, from what has gone before, that, if this argument applies to the specific contagions, no reason whatever can be shown why it should not apply to animals and plants also.

Itch, being a contagious disease, two persons are always necessarily implicated in its propagation. But there must have been a time when a *first* person had the itch without receiving it from another. The itch insect having originated *de novo* once, must therefore originate *de novo* now. The two facts stand on exactly the same level.

The truth is, that the first origin of the germs of specific contagious diseases and of the great procession of organic types which have successively appeared on the earth is alike unknown to us. By the light of science we know no more of the first evolution of the germ of small-pox or syphilis, than we do of the acarus which inhabits the human skin—of the tapeworm which was appointed to live in the human intestine—of the cystic worm which finds a temporary home in the brain of the young sheep—of the miseltoe which fastens on the oak and the apple—or of the different species of mildew which breed in the grain of wheat. We know no more, by the light of science, of the first origin of *these* living types, than we do of the higher organisms on whose substance they feed.

The precise mode in which *all* these things first came into being is hidden from us by an impenetrable veil.

On the other hand, everything tends to show, that, once created, they all propagate now only in one way. The idea that animals and plants of known species continue to spring up anew is, as we have seen, quite exploded. That small-pox is now only maintained by self-propagation, there are, as we have also seen, the strongest conceivable reasons for believing.

The history of syphilis—and, indeed, of many another contagious disorder—teaches the same lesson. Who among us can say when, or how, or by what process this terrible virus first came into being? Who can name the thrice unhappy man, on whose person, innocent of any unclean contact, it *first* implanted its foul venom? We all know how it propagates now, and that inoculation by the actual contact of two persons is essential to its continuance.

I have always thought it a very significant fact, that, in this case, where the process of transplantation can only be effected by an act of which two persons, at least, must be cognisant, the idea of spontaneous origin has never once arisen.

Let an isolated case of typhoid fever occur, and, merely because he cannot readily trace the disorder to its source, many a medical man will tell you without the slightest hesitation or difficulty that the disease has, clearly, originated spontaneously. Press him, and he will be ready at once with the argument that there is no reason in the world why the conditions which *first* gave rise to it, may not again give rise to it now.

But let a young sinner with the peccant mark upon him swear ever so stoutly that the disease he bears has come on spontaneously; let him argue as plausibly and as long as he will against the incredulity which refuses to see that the conditions which *first* gave rise to this disease, may again give rise to it now, and the very same medical man will laugh him to scorn.

In small-pox and syphilis, where, from the nature of the facts, the evidence bearing on this question is precise and sure, the answer is unequivocal.

The history of these two diseases is at hand to show—as by a great standing experiment—that these specific contagions, so far from being things that spring up at every turn, have, in reality, come into being under conditions hidden in primeval darkness, and of which we have no experience.

The history of many other contagious maladies is precisely to the same

effect. The very significant fact of the exclusive appearance of many among them, in the first instance, in particular geographical centres, whence, sometimes only after the lapse of long ages, they have afterwards spread far and wide, leads to the same general conclusion.

It will be seen that, in the course of this discussion, I have altogether left out of view the formidable objection to the commonly received theory of the spontaneous origin of contagious fevers, which may be drawn from the incomparably specific condition shown in my last communication to attach to the reproduction of the specific germs of these fevers, even in the living body itself.

In taking a survey of the whole question, the only difficulty, indeed, is to say whether the variety, the massiveness, or the precision of the evidence which may be arrayed against this theory, be the more remarkable.

Stated briefly, the considerations which are opposed to it in the case of intestinal fever, may be thus summed up.

1. The known mode of propagation of this fever is sufficient to explain the actual phenomena, so that the "spontaneous" hypothesis is wholly gratuitous.

2. Under the definite form which it has hitherto taken—that this fever is often actually caused by the products of common putrefaction—this hypothesis, when subjected to a searching examination, entirely breaks down.

3. The evidence which is supposed to render such an hypothesis necessary, would, if of any force, render it equally necessary not only in the case of small-pox, but in that of plants and animals also.

4. In regard to all the specific contagions whose history in past ages is known—such as small-pox and syphilis, for example—there are the strongest possible reasons for believing that, whatever their primary source, they propagate now only by the law of continuous succession.

5. The incomparably specific conditions which attach to the reproduction of the fever poison, even in the living body which is its natural *nidus*, render its evolution by spontaneous origin not only exceedingly improbable, but difficult to conceive of.

To some it will, perhaps, seem that I have laboured this point with unnecessary minuteness. A little reflection will show that the fact is not so.

Viewed in relation to this fever alone, the question is plainly one of cardinal importance. So far, however, from being thus limited, the same question recurs in the case of all the other epidemic and contagious disorders.

For some, possibly, a stronger case may be made out: and in these, if such there be, the point at issue must be argued on its own grounds. But on evidence in all respects identical with that which has been here discussed, a claim has been set up for the spontaneous origin of Asiatic cholera, of yellow fever, and of a host of other specific contagions. It was well, therefore, if only to avoid the need of going over the same ground again, to see once for all, what this evidence is worth.

With the case of small-pox, and that of animals and plants, before us, it will, at any rate, not be required of us to believe in the spontaneous origin of cholera, simply because the succession of cases often cannot be traced.

The conclusion at which we have come is important, also, in another way.

The moment we are entitled to look upon typhoid fever as a disease which, like small-pox, is propagated solely by its own germs, the facts relating to its spread, instead of being interesting only in a negative sense, acquire a value of a *positive* kind.

As illustrations of what is *known to happen*, when a disease so propagated is spread by liquid discharges, they are of the greatest possible help in interpreting the phenomena which characterise the dissemination of other contagions which are spread by liquid discharges too.

By the light of what we have learnt of typhoid fever, the dissemination of Asiatic cholera, of dysentery, of yellow fever, not to mention other members of the group, becomes easy to read.

In addition to all this, a point of high equity is concerned in the argument. On all accounts it is time that the opposite parties in this great debate should

be placed in their true respective positions. Whenever, and wherever it is opened, I observe that the contagionists are uniformly treated as if they, and not their opponents, were guilty of dealing in assumptions, and of going against evidence.

I need scarcely remark that the truth is precisely the opposite of this.

At all events, as regards this fever and the other members of the same family group, the contagionist alone proceeds on the solid basis of fact, and has the support of analogy, as massive as it is precise, where from the nature of things the direct evidence of fact is not to be had.

We know that the poisons of small-pox, typhoid fever, and cholera are multiplied in the human body, almost as certainly as we know that corn is raised from its own grain, or that thistles spring from their own seed.

That these poisons are multiplied in any other way is not only pure hypothesis, but, as we have seen, an hypothesis that has, besides, the fatal effect of being entirely gratuitous,—of being in opposition to an analogy that is of any worth, and of being in one of these cases, at least, only tenable at all by heaping one assumption upon another.

P. S.—As I have reason to believe that some readers have not fully apprehended the drift and purpose of the argument developed in the preceding pages, in consequence of not having seen the series of papers on the same subject, which I published in the Lancet in 1856-58-59, it may be well to state that in that series will be found data which, to myself, appear to establish by the most strict and severe induction from incontrovertible facts, the following conclusions.

1. That typhoid or intestinal fever is essentially contagious.
2. That the living body of the infected man is the soil in which the poison breeds and multiplies.
3. That the reproduction of the poison in the infected body, and the disturbance attaching to it, constitute the fever.
4. That this reproduction is the same in kind as that of which we have, in small-pox, ocular demonstration.
5. That the intestinal affection is the specific eruption of the fever, and is related to it in the same way in which the pustules on the skin in small-pox are related to that disease.
6. That the exuviæ from the surface which is the seat of this eruption contain, as we should have expected, the most virulent part of the contagious principle.
7. That, as a necessary result, sewers, and the cloacæ which fill the office of sewer, are the principle media for the transmission of the contagion; and, consequently, that in many instances the infected sewer, and not the infected man, appears as if it were the primary source of the specific poison.
8. That, once cast off by the intestine, the contagious matter may communicate the fever to other persons in two principal ways: either by contaminating the drinking water; or, which is by far the more common case, by infecting the air.—*British Medical Journal, Dec. 7, and Dec. 14, 1861, pp.* 604, 625.

2.—ON THE CAUSATION OF TYPHOID FEVER.

Mr. SIMON'S latest opinions on the much discussed and important question of the etiology of typhoid fever have undergone changes. In the last printed report to the Privy Council (April, 1861), Mr. Simon thus expresses himself strongly in favour of the essentially contagious character of the disease, and its specific power of infection:—

"Since I last reported generally on the subject of typhoid fever (Report 1858) an addition has been made to the literature of that disease, by the publication (the Lancet, July, 1859—March, 1860) of a series of papers by Dr. William Budd, of Bristol. Dr. Budd's opinions, as regards the causation of typhoid fever, are as follows:—That the fever is essentially contagious; that the living human body is the soil in which the specific poison breeds and

multiplies; that all the emanations from the sick are infectious; that by far the most virulent part of the specific poison by which the contagion takes effect is cast off by the diseased intestine of the fever patient; that the characteristic affection of the bowel in the disease is, in reality, the specific eruption of a contagious fever; that the sewers and other places into which all this virus passes are the principal channels through which the fever is propagated; *that they propagate it solely in consequence of being the channels for the diffusion of this poison*; that it no more is the offspring of common sewage than mildew is the actual offspring of damp and decay; and that 'by placing two ounces of caustic solution of chloride of zinc in the night-stool on each occasion before it is used by the fever patient, the intestinal discharges may be entirely deprived of their contagious powers.' To anticipate some arguments which might be urged against parts of this doctrine, Dr. Budd observes, that typhoid fever scarcely ever re-attacks a person who has once suffered it; and that, 'like malignant cholera, dysentery, yellow fever, and others that might be named, this is one of the great group of diseases which infect the ground.'

"The facts which Dr. Budd adduces from his own experience and from that of other observers are, in my opinion, sufficient to prove that the contagion of typhoid fever is importable by persons who have the disease. Indeed on this point Dr. Budd's history of the North Tawton fever and its off-shoots (the Lancet, July 9th) is more conclusive than anything previously known to me. And his arguments are also, I think, cogent to this general effect—that specially the bowel-discharges of the disease are means (yet not therefore necessarily the sole means) by which a patient, whether migrating or stationary, can be instrumental in spreading the infection of typhoid fever. Provisionally these conclusions must be acted upon in their present unqualified form. But doubtless it is of practical importance to learn, as exactly as possible, whether it is in all states and under certain circumstances, that the bowel-discharges of typhoid fever can effect what is here imputed to them. Typhoid fever seems to be, in its causes as in its nature, very intimately related to other diarrhœal affections."

These opinions are entitled to especial attention and worthy to be carefully noted, if only because they are the conclusions of a sound pathological and able thinker, based necessarily upon the largest generalization and the greatest number of facts which any man can possess in this country. In addition to the scientific weight thus given to them by the personal and official advantages under which they are formed, the views of the principal medical officer and the official adviser of the Privy Council represent also for the time being those of the Government, and have, therefore, the practical importance which belongs to words that may at any moment be vitalized by action. We stated that Mr. Simon had in his last report adopted certain of the contagionistic theories which Dr. Budd of Bristol so ably brought forward in the columns of the Lancet. It is proper to add, that Mr. Simon annexes certain limitations, which modify his adhesion to these important doctrines. In qualification of the quotation which we have already given, we append the following further remarks of Mr. Simon:—

"Typhoid fever seems to be, in its causes, as in its nature, very intimately related to other diarrhœal infections. And with reference to the possibility of its being only contingently contagious, I quote, for illustration's sake, from my report of 1858, the following passage referring to epidemic cholera:—'Some interesting and important experiments made in 1854 by Professor Thiersch, of Erlangen, seemed to show that cholera evacuations *in the course of their decomposition* acquire a contagious property. It is much to be regretted that experiments were not simultaneously conducted by Professor Thiersch, with a view to determine whether ordinary fæces, or ordinary diarrhœal fæces, *undergoing decomposition during an epidemic period*, would not likewise have acquired that property; for the prevalence of exterior conditions, which tend to determine in certain localities a specific infectious decomposition of excrement, seems to be the essence of an epidemic period. That this decomposition may begin in the bowels, as well as

in cesspools, seems possible enough; and perhaps herein lies the explanation of the many cases in which human intercourse has apparently diffused the disease. For, according to the observations of Professor Pettenkofer at Munich, and Professor Acland at Oxford, it would seem that during cholera-periods the immigration of persons suffering diarrhœa has been followed by outbreaks of cholera in places previously uninfected; and Professor Pettenkofer ascribes this fact to an infective influence exerted by the fæces of such persons in the cesspools and adjoining soil of ill-conditioned places to which they go. An infection of this kind would probably extend itself to the polluted well-waters of such soils, and might render them, if swallowed, capable of exciting cholera by direct contagion. It is encouraging to sanitary reformers to observe that cases of apparent introduction of cholera contagion by human intercourse are essentially different from such cases of infection as are presented by measles or small-pox. The multiplication of poison in the latter diseases takes place exclusively within the human body; it has no immediate dependence on differences of medium, and wherever human beings can cross one another's path, the susceptible person may contract infection. But the cholera-poison, if indeed it can at all be multiplied within the body, almost certainly has its great centres of multiplication elsewhere, in those avoidable foci of corruption where excrement accumulates and decays. And likewise for diffusing its contagion, if truly the disease be contagious, foulness of medium seems indispensable. Indeed, it is no ordinary foulness which taints the air or food or water with the leaven of decaying excrement. Therefore, as regards cholera, it seems highly probable that the immigration of infected persons might occur to any extent without exciting epidemic outbreaks, if it occurred only into places of irreproachable sanitary conditions, especially as regards the supply of water, and the continuous removal of house-refuse. Compare Pettenkofer über die Verbreitungsart der Cholera, 1854; Acland on the Cholera at Oxford, 1856; and Thiersch's Infectionsversuche an Thieren mit dem Inhalte ees Cholera-darmes, 1856.'"—*Papers relating to the Sanitary State of the People of England.—Lancet, Jan.* 18, *and Feb* 1, 1862, *pp.* 77, 135.

3.—ON TYPHUS FEVER.

By Dr. GULL, Physician to Guy's Hospital.

In reference to the course of the fever, Dr. Gull said that it might be likened to an inverted curve. For instance, from the outset, day by day they get gradually worse, the pulse becoming more frequent and weak, the breathing more rapid, and the symptoms of exhaustion increasing. A point is at length reached when the symptoms as gradually improve. As the first stage represents the descent, the stage of recovery represents the corresponding ascent up to the plane of health. He stated that the lowest part of the curve was arrived at on the fifteenth day, the depth of the curve being best determined by the pulse and breathing. The idea of the cyclical course of a fever was of practical importance. It indicated that the disease had a definite physiological history, and that it would, in spite of interference, complete it; that the patient must, in the nature of the case, get worse up to a certain point; and that then, if he had sufficient vigour, he would as certainly begin to mend. To enable the patient to reach this point was the important aim of treatment. That there is a certain turn after which amendment begins, has not been sufficiently recognized in later times, and hence the improvement has often been ascribed to treatment, rather than recognized as a natural feature of the disease. A few years ago, Dr. Gull said, when he was investigating typhoid fever as to the question of time, he had under his care a patient who was extremely ill with the disease, and further gradually gravitating downwards. He was strongly urged to treat her with wine and brandy. He felt so certain, however, that this gradual increase of the bad symptoms was but the natural progress to a crisis, which he felt convinced the patient had

vigour enough to reach and bear, that he did not complicate the case by treatment with stimulants. He gave simply beef-tea, broths, and milk. The pulse advanced to 120, and for some hours to 130, but on the sixteenth day it began to fall, and the patient steadily improved. If, then, in this case, he had given stimulants, the recovery might have been attributed to this rather than to the natural course of the disease. The course of typhus, Dr. Gull said, was a very simple one, perhaps the most simple of any fever; in others it was often very complicated. When the lowest point was turned, there was in typhus but little trouble afterwards. The most necessary things in this disease were good hygiene and good nursing. If the patient can be kept alive for fifteen or sixteen days, he may be expected to recover, and is usually safe.

Although, Dr. Gull said, he believed firmly in the distinction of typhus and typhoid fever, he also believed that an advanced comparative pathology would show that they had a relation one to the other. In the present state of our knowledge, however, and for practical purposes, they are to be thought of as distinct diseases.

How does the patient get well? This is a question the answer to which bears on the therapeutics of the disease. Thus, some say that the patient gets well when he has eliminated the poison. If the typhus condition depended on the presence of a certain quantity of poison, it was difficult to believe that the patient could recover if he only recovered by eliminating it, for he was placed in just the same position for imbibing it as he was when exposed at first. Then it is just when the fever is most intense that the patient begins to improve. Though a typhus miasma emanates from the sick, there is no evidence that it is thrown off by elimination, leaving the body free of it; and now that it is eliminated, that it would not be re-absorbed. There is no reason, Dr. Gull said, for admitting that the poison was in less quantity in the body on the sixteenth day than on the day previous. The marked improvement about a certain day could not be accounted for on the elimination theory; it was plainly a result of living processes governed by time. He (Dr. Gull) could not believe in the theory of recovery by elimination. Whilst the patient was getting well, he was a source of infection to those around.

The common theory chiefly supported by the chemists is, that the poison of fever acts as a ferment in the system, inducing a peculiar zymosis, as it is called, whereby the fermentisible substance is destroyed, and coincidently the fever process ends, and the patient recovers, or, at least, has but to regain strength. In order to make this theory hold together, the chemists say that a person is born with certain substances in the body, as proved by the tendencies to various fevers,—*e.g.*, a something the fermentation of which by the scarlatinal poison results in the phenomena of scarlet fever, another something acting on which the rubeoloid poison produces measles, and so for all other fevers. The patient is no longer susceptible after having once had a fever, because all the special "fermentisible" material has been exhausted by an attack. Dr. Gull said that he could not accept this theory. He thinks that instead of the system getting rid of the poison, it has, on the contrary, assimilated itself to it, and thus become indifferent to it. The facts of acclimatisation are of an analogous kind; a person who goes to a new climate is subject, and soon suffers from the diseases peculiar to the climate, which diseases do not, or less frequently and to a much slighter degree, attack the natives. And further, that the person thus exposed, in time acquires an immunity from further trouble. Again, as regards poisons, it is well known that a person after a time may take poison without harm, for instance opium, which would kill another person who was not used to it.—*Medical Times and Gazette, April* 5, 1862, *p.* 340.

4—*On Typhus Fever.*—PROFESSOR GRIESINGER, who is Director of the Medical Clinique of the University of Zurich, has recently observed a rather considerable number of cases of typhus, and gives the following differential

diagnosis between this disease and typhoid fever. In the former the exanthema appears much earlier than in the latter, and is generally much more extensive. Thousands of red spots are, in certain cases, seen to cover the trunk and the extremities, especially the forearms and the thighs; in a few cases spots are also observable on the face, and they sometimes flow together, which is never observed in typhoid fever. In the latter the spots, which are at first of a bright red, become pale and disappear after four or five days; while in the former they become dark, livid, and of a dirty colour on the third day, and they do not then disappear, but a slight hemorrhage takes place in them, whereby they appear petechial. These remains are generally visible for from ten to fourteen days, even after the febrile symptoms have entirely disappeared, which is scarcely ever observed in typhoid fever.

The whole course of typhus is also very peculiar. While in typhoid fever an increased temperature in the morning may be generally observed for three weeks, the morbid heat in typhus diminishes suddenly and without interruption within two days; just as in pneumonia, erysipelas of the face, &c. In typhus the patients complain of violent pain in the limbs, especially in the calves, which persists during the whole stage of acme; and even after all febrile symptoms are gone, the tongue sometimes remains dry and swollen. The convalescence is generally very slow and protracted, and in some patients an extraordinary emaciation results. The mortality in the cases observed by Professor Griesinger was small, as of sixteen only one died. In two cases typhus appeared immediately after typhoid fever, so that there can be no doubt about the existence of two different contagia.—*Med. Times and Gazette, Dec.* 21, 1861, *p.* 645.

5—ON THE RENEWAL OF LIFE IN CONTINUED FEVER.

By Dr. Thos. K. Chambers, Physician to St. Mary's Hospital.

[In the treatment of continued fever, recollect that a large supply of nitrogenous material must be wanted in the system: for, the nitrogenous tissues are devitalized, and running away in an enormous excretion of urea and other organic compounds, and nothing is taking their place. If we attempt to supply nitrogenous matter in the form of flesh meat of any kind, not only do we not gain our end but we do actual injury to the patient. Animal food must be given in the liquid form, not in large quantities at a time, but in small and oft-repeated doses. Milk may be given as a food with advantage, but it sometimes happens that the casein is rejected in lumps, being coagulated by the acid in the stomach. This may be guarded against by adding liquor calcis, or soda-water to the milk. Eggs are a highly nutritious food; and, if taken raw, diluted with milk and water, they are quickly absorbed. Turning our attention to the Materia Medica, we recollect that]

Ammonia, which is always being formed and given off from the animal body, is found much more abundant in certain conditions than in others, and that these conditions are those in which nutritive metamorphosis or growth was deficient as compared with destructive metamorphosis, or those in which there is retention in the blood of the products of that destructive metamorphosis. Thus, more ammonia is found in the breath after exertion than after rest; more than usual in those who injure their digestion by smoking tobacco; a great deal in uræmia, where the urea cannot escape by the kidneys; but above all, in continued low fever is this exhalation of decay noticeable, as you will find in Dr. Richardson's valuable work on the "Coagulation of the Blood," where the phrase "super-alkalinity of the blood" is applied to this condition. Dr. Richardson goes so far as to attribute to this super-alkalinity the special typhoid symptoms, and to suggest that the absorption of ammonia in excess may intensify fever in those who contract it from exposure to decaying organic matter, or human exhalations. He supports his hypothesis on the experiment of inducing the symptoms, or something resembling them, by the injection of ammonia into the veins of an

animal. The word "super-alkalinity" is expressive, and quite unobjectionable, so long as it is understood that the superabundance is not absolute, but comparative. For it is not shown that there is more alkali in the body than there ought to be, but more than there is acid to neutralize. "Sub-acidity" would be a synonymous term, and, perhaps, would be more suggestive of the means we have at our disposal for remedying the defect.

Very difficult indeed would it be for the Eliminator to get this alkali out, but it is easy for the Restoratist to get acid in. The acid I have always given is hydrochloric, and you consequently see on this lad's card—"R. Acidi hydrochlorici diluti ℥xx., syrupi ʒj., aquæ ad ℥j., alternâ quaque horâ sumat."

Rich patients like a little more sugar, and the draught is usually approved of even for its taste. But it is still more approved of for its beneficial effects. This boy said to-day of his own accord he hoped I should continue the draughts, they made him "feel so much stronger," meaning to express the sensation of relief to the depressing wearying languor of fever. In mild cases the tongue begins to clean immediately, the thirst and diarrhœa much abate, and the repugnance to food is diminished.

Whether other acids would do as well as the hydrochloric I cannot say, but it is so largely diffused through the body combined with alkali as a constituent of the tissues, that it appears peculiarly suited to the purpose. I have now used it in every case of low fever for four years, and have not lost one, except a woman who came in here with deep ulcers in the bowels, and was killed by perforation. In a clinical lecture at this Hospital in January, 1858, I gave the details of the first dozen cases treated on this plan, and I must say that my confidence in it is by no means diminished.

As you are a different audience from that which heard me at that time, perhaps I may be allowed to quote in repetition my own words—"What blood, when analyzed, comes nearest in its altered proportions to the blood in low fever? Is it not that in scurvy and purpura? There is the same excess of blackened (melanosed) blood discs, the same deficiency of neutral salts and organizable (coagulable) lymph. Everybody treats these chronic affections with acids, and why not also an acute affection which corresponds with them in one point at any rate? As to the particular acid employed, muriatic certainly deserves to be tried before others—first, because it is such a large constituent of the body that it might almost be called a food instead of a medicine; and secondly, because it is such a powerful arrester of the decomposition of animal matters. Pour it into a sewer, and you destroy the miasma. May it not in the body stop that miasma from poisoning the tissues?"

The allusion in the last sentence is of course to chlorine as a disinfectant. But I do not know that there is any evidence of the benefit derived from the hydrochloric acid treatment being due to this property. About sixteen years ago, I employed chlorine water as a medicine in fever at the Chelsea Dispensary; but I was not encouraged to carry on the treatment by tracing any benefit to it. Whether the chlorine which can be introduced in this way is not sufficient in amount (for it must be dilute, or it induces so much choking) or whether it is useless, I do not know; but I left it off for years, and adopted the muriatic acid from the description which Mr. Ash gave me of the advantageous use of it by Dr. Mackenzie.

As to the use of alcoholics in continued fever, I am guided almost entirely by the condition of the nervous system. If there is very complete prostration, delirium of a low muttering character, it is required. A tremulous state of the muscles, marked especially by a quivering of the hands and fingers, is a good test of the necessity for it; and so is a sharp, weak, unequal beat of the heart. All these indicate that the nervous system is feeling very sensitively the destructive metamorphosis going on, and has its power reduced by its sensitiveness. Then is the opportunity for the powerful anæsthetic alcohol, which in severe cases you see me order without scruple, but which I do not rank as part of the systematic *methodus medendi* of continued fever, and have not yet ordered for the lad we have been prescribing for. Above all, I would caution you against employing wine as a substitute for

the true restorative treatment which I have been describing. It may be useful as an adjunct or to assist it, but never in its place.

There is, though, another of the adjunctive methods of treatment—exceptional, but often required—which has been employed with utility in this case—the local abstraction of blood. The boy had congestive pneumonia of the lower lobe of one lung, and I did not hesitate to cup him beneath the scapula on that side. And not unfrequently when there is pain in the right iliac fossa, with diarrhœa and tenesmus, I put leeches on the belly at the seat of pain. You saw the dulness on percussion rapidly pass away from the lower lobe in our patient here; and you will often see an equal relief to the abdominal congestion by a corresponding agency.

But you will cry out that I am sadly inconsistent. I am feeding up the patient with one hand and taking away his *pabulum vitæ* with another. The reproach is just in a sense, but that a very limited one; and, in fact, may be levelled against half the operations of daily life.

We are constantly suffering a small loss for the sake of greater gain. And I think the loss of a little blood is insignificant compared with the advantage of securing a freer circulation through the lungs, a diminution of congestion in the intestinal canal. Do not be led away by the superficial notion that blood is blood, and blood is life. That is not true, for the blood varies immensely in its composition, some being very valuable, and other worthless. To lose some of the half-dead circulating fluid of fever is but little loss, and that little loss is amply compensated for by the additional nutriment which a small blood-letting will enable to be absorbed. The deficiency is soon made up again under the restorative system.

In the sequelæ of low fever, more than in any other disease, the great difference between one patient and another as respects their power of recovery, lies in their stomach. There is a little girl of four years old, now in Victoria Ward, who was admitted on the 2nd of September, for rose-spotted fever, which had come out during the concluding week of August. She got through the fever pretty favourably, but for the last seven weeks has had a succession of most formidable abscesses in the back, the cervical glands, the internal ears, and the parotid glands; yet, in spite of the exhausting effect of the large discharge of pus from these spots, she has continued advancing in convalescence, she has gained flesh and muscular power, so that now she can sit up. For this favourable result she has to thank a most wonderful appetite, which never seems satisfied, even with an amount of food which is large for an adult, and which she delights to wash down with wine and porter. No tonics seem of so much use to her as an extra snack at physic time.—*Med. Times and Gazette, Nov.* 23, 1861, *p.* 523.

6.—ON THE EMPLOYMENT OF QUININE IN FEVERS AND OTHER DISEASES.

By Dr. J. C. Cameron, Deputy Inspector General.

Quinine is the master key to the treatment of nearly every tropical disease, in nearly all of which *attention, once directed to the point,* will rarely fail to detect a periodic type—that is, a distinctly recurring exacerbation of symptoms, but not necessarily involving increased heat or rise of pulse. Thus in dysentery, the most dreaded disease of the tropics, a patient will have a rapid succession of bloody stools, with aggravation of tenesmus, and great prostration, at some certain period of the twenty-four hours; and, in such cases, one or two ten-grain doses of quinine, given, in the interval, will often ward off the attack, just as it would that of ague, and usher in recovery.

The malarious type of dysentery is one of its most fatal forms and should be regarded, in a great measure, as essentially fever, with disease of the bowels superadded, though unfortunately the urgency of the local symptoms too generally occupies the whole attention of the physician. This is the form of the disease constantly seen in woody, marshy countries like Ceylon, and

which, asthenic in itself, becomes deadly if treated, *merely for its name*, by the usual routine of aperients, leeches, calomel-and-opium, particularly when it appears amongst a body of men weakened, as soldiers often are, by constant solar exposure, night duties, and want of proper nourishing food. In such cases, after a dose of oil, quinine should be given to the extent of twenty or thirty grains in twenty-four hours, with as much ipecacuanha as the stomach can be got to bear, aided by opiates, astringents, counter-irritation, and plenty of nourishment suitable to the disease. The acute capillary bronchitis, which, treated on the old system, used to cause great mortality amongst infants and young children in those countries, is in like manner essentially fever, and its most alarming symptoms, when the tubes are stuffed with mucus, and the child half asphyxiated, will often subside with astonishing rapidity after a smart emetic, followed by frequently repeated doses of quinine, combined, if it can be done, with ipecacuanha or James's powder. The youngest infants can take with benefit from two to five grains of quinine in twenty-four hours, and in their case this mode of treating fever is a particularly great improvement, as the dosing such little creatures with calomel, antimony, &c., on the old system, was most distressing to all parties concerned, and too frequently ended in death. Few people have any idea of the fearful amount of mortality amongst the children of our soldiery in the tropics, particularly in regiments not blessed with a kind-hearted, patient assistant-surgeon, who will take pity on the neglected women and children, and devote some attention to the little folks' ailments. Nor do I believe that the powers of quinine to overcome such (apparently) acute forms of disease are at all limited to the tropics. During the last three years I have used it very freely in the same way in England, and often with rapid relief to the patient, particularly in catarrhal and bronchitic affections during the last severe winter.

When one reflects on the vast number of valuable reports which have necessarily been furnished to the respective medical boards, embodying the experience and knowledge acquired by thousands of educated men, in all corners of the globe, it is clear that a separate bureau for practical medicine ought long ago to have formed a special department of each office. At its head should be an able officer, charged to keep the scattered members of the corps acquainted with all discoveries and improvements in their art, and to *digest* and publish for general information the detailed observations on climate, topography, disease, and its treatment, which are furnished from our immense possessions. To do this effectually and permanently, he would require to send forth, not a tardy volume, like the recent "Army Medical Reports," dealing in crude generalities, through which readers must wade to conclusions, and containing flimsy "Annual Reports," which it would be a libel on the department to consider the best specimens furnished amongst several hundreds; not a ponderous tome of this sort, but a military medical journal, a regular bi-monthly or quarterly periodical, the very establishment of which would call forth a general *esprit-du-corps*, and induce many of its ready-made body of subscribers to become frequent contributors. With this there should be given at stated periods, say of three or five years, an analytical retrospect of the advances made and results established, in its pages during each interval. In this way we should have always available an immense body of information on all countries and climates, now scattered and buried, no one knows where. We should also see established on the sure foundation, not of clever theory, but of successful practice, a definite system of treating climatorial diseases; so that when a young officer landed in one of our colonies he would be enabled to begin where his most experienced predecessors left off, instead of being left, as now, either to grope his way to their conclusions at a great waste of human life and suffering, or perhaps never to reach them at all.—*Lancet, Nov.* 9, 1861, *p.* 457.

7.—ANARCOTINE IN REMITTENT FEVERS.

In the Indian Annals of Medicine for September 1861, there is an elaborate and able report addressed by Dr. A. Garden, of Ghazeepore, to the Deputy Inspector General of Hospitals, on the therapeutic uses of anarcotine, tabulating in various forms the results of its employment in nearly 700 cases of intermittent fevers. Turning to our systems and dispensatories, we find it briefly noticed that this substance has been employed as an antiperiodic by Dr. Roots and Dr. O'Shaughnessy. This brief notice has, it appears, been wholly overlooked or neglected in this country; and yet, as we shall show, anarcotine possesses therapeutic properties well worthy of the attention of British practitioners.

It is generally known, that one of the crystalline constituents of opium received from chemists the name of "narcotine," under an erroneous impression that it was the narcotic principle of that drug. So far from this being the case, it has not the slightest claim to that title; and, consequently, it is very properly proposed to prefix the privative letter *a* to the name, and henceforth designate it as *anarcotine*.

Referring to Sir W. O'Shaughnessy's original statement, we find him saying:—

"I have now employed the narcotine in sixteen cases of remittent fever, and such is my opinion of the efficacy of the remedy, that in instances of fever, intermittent and remittent, in ordinary healthy subjects, and in whom there is no complication of severe organic disease, I give it with the full expectation of arresting the next periodic return of the fever. I have seen the result follow in ten of the cases of the fever alluded to. I consider narcotine a more powerful antiperiodic than quinine. The remedy does not act silently. I have observed a degree of general heat follow its use in the first instance, and subsequently perspiration, so that it appears to excite in the system a salutary and powerful counter-action, as to stop the morbid concentration that issues in fever. I have not observed narcotine to lead to organic disturbance in the cases in which I have used it. In short, even from my scanty experience, I consider the remedy an invaluable one."

To his own testimony Sir William added the experience of several practitioners, who speak in the highest terms of its value as an antiperiodic, especially applicable—1st, in quotidian fevers; 2nd, in cases complicated with visceral enlargements of the liver, spleen, &c.—or local inflammations, as of the bowels—or cerebral congestion; 3rd, in cases of intermittents supervening upon surgical operations; 4th, after failure of quinine, or both quinine and arsenic.

Anarcotine is a white, inodorous substance crystallizing in prisms, insipid to the taste, insoluble in cold and sparingly soluble in boiling water, more soluble in alcohol and ether, insoluble in alkaline solutions. It forms salts with the mineral acids which have a very bitter taste, similar to that of quinine. The sulphate and hydrochlorate are the most convenient for internal use. Anarcotine is obtained from the residue of opium left after separation of the morphia. Hence it ought to be a very economical medicine.

Dr. Garden's investigations, as recorded in the report alluded to, were directed to the therapeutic value and peculiarities and the commercial advantages of the use of anarcotine, and the conclusions he establishes are as follows:—

We have in anarcotine a remedy which fails in only 3·6 per cent. of all cases treated by it alone; and, without regard to the previous duration of the disease, and taking both quotidian and tertian agues, it arrests the fever on the whole average before the recurrence of a third paroxysm after the commencement of the remedy.

In small doses anarcotine acts as a tonic, increasing appetite and improving the tone of the system generally. For this purpose, from half a grain to a grain of the sulphate, combined with a slight excess of sulphuric acid, is a sufficient dose.

As an antiperiodic, a grain and a half to three grains or more, even to the extent of six grains, may be given at suitable intervals. Its most appreciable effect on the system is, that it increases the heart's action, raising the pulsations from ten to twenty beats per minute beyond the ordinary amount, and renders the pulse somewhat fuller.

In large doses (five to fifteen grains) it produces increased heat of surface and diaphoresis, and sometimes disagreeable symptoms—nausea, giddiness, and vomiting. These, however, disappear on lessening the dose, in most cases. The author remarks, "In several cases I have obtained marked benefit by combining tincture of opium with the sulphate of anarcotine." "The only objection to the medicine is its tendency to produce constipation;" hence, before and during its administration, purges are necessary.

In reference to the relative economy of anarcotic and quinine, Dr. Garden's tables are full and conclusive. The average quantity of sulphate of anarcotine required to arrest intermittent fevers was found to be twenty grains for quotidians, and something less than forty grains for tertians; whilst about sixteen grains were demanded during convalescence from both kinds for the re-establishment of health. If it fails in a small per-centage of cases, this is not more than is experienced with quinine, to which it is nearly if not quite equal in therapeutic value as an antiperiodic.

As a matter of mere curiosity, not of any present significance in therapeutics, we may add that organic chemistry separates anarcotine into four homologous bodies, which, by decomposition with caustic potash, yield homologous volatile bases:—

Normal anarcotine		yielding	ammonia.
Methylic	"	"	methylamin.
Ethylic	"	"	ethylamin.
Propylic	"	"	propylamin.

—*Lancet, Jan.* 11, 1862, *p.* 53.

8.—ON THE ALBUMINURIA OF FEVER.

By Dr. George Johnson, Physician to King's College Hospital.

[The following is an extract from a clinical lecture on Typhus and Typhoid Fevers. Dr. Johnson, after showing the importance of a clinical and microscopical examination of the urine from time to time during the progress of fevers, observes:]

Renal congestion and inflammation, with albuminuria, are more frequently than is commonly supposed the cause of the early occurrence of drowsiness, and other formidable head-symptoms in the course of these diseases. When, during an early stage of the fever, the urine becomes scanty and bloody or highly albuminous, there are three points of practical importance to which your attention should be directed. First. Give a guarded prognosis, for the complication is a serious one. Second. Give alcoholic stimulants more sparingly and cautiously than you do when the kidneys are unaffected, otherwise you may increase the renal congestion and with it the stupor and other cerebral symptoms. Third. With a view to lessen the congestion of the kidneys, counter-irritate over the loins, either by poultices of one part mustard to two linseed, or, perhaps, better by dry cupping repeated daily, or twice a day.

For the relief of this renal complication during the progress of fever, whether typhus or typhoid, or scarlatina, it appears to me that the practice of packing in wet blankets, carefully and judiciously carried out, is admirably adapted. I have hitherto had but a limited experience of this plan of treatment, but the results in the hands of others have appeared to be so favourable that I have determined on giving so obviously rational a proceeding a fair trial.—*Med. Times and Gazette, March* 1, 1862, *p.* 216.

9.—ON THE EXTERNAL USE OF THE SOLUTION OF PERNITRATE OF MERCURY IN EPITHELIAL CANCER, LUPUS EXEDENS, AND THE INDURATION OF CHANCRE.

By JOHN GAY, Esq., Surgeon to the Great Northern Hospital, &c.

This preparation, is, I believe, a concentrated solution of the red oxide of mercury in hot nitric acid, and is said by chemists to have a very strong affinity for ammonia. It is on this account, perhaps, that it exerts so powerful an influence over some of those abnormal tissues of the body which are rich in the elements of that alkali, but comparatively poor in respect of vital power. To this influence I will now especially direct attention, but without claiming any originality either in the practice of employing it, or in the observations which my experience of its power has enabled me to make.

Perhaps the most striking effect of this agent is that produced by its application to *epithelial cancer*. I have now tried it in a considerable number of cases, and can affirm that, so far as this affection is curable by its perfect local eradication, the solution of the pernitrate of mercury can effect a cure. I will give two cases which will serve to illustrate its value and *modus operandi*. I might give many more, but they would be only repetitions of the same account.

Mr. A., aged 52, a farmer, in other respects a most healthy and robust man, asked my advice for a cancerous growth (epithelial) on his lower lip. It seemed to occupy the whole of the edge, but had not extended to the junction of its mucous membrane with that of the jaw. Towards the left corner, it had grown to the size of a very large walnut; and the surface had ulcerated, exuding a thin and slightly offensive discharge. There was an apparent absence of glandular contamination. It was uneasy, and growing steadily, but perhaps not rapidly; the whole having been in existence for about a year and a half. I advised either excision or caustic. The latter was preferred. I need not follow the details beyond stating that I applied the solution of the pernitrate abundantly over the whole of the ulcerated surface, not at all careful to keep the application within its limits. It gave great pain, but only for an hour or two. It had the effect of destroying a layer of the diseased growth, which came away as a slough on the third day. The remedy was applied, or rather, the surface was soaked with the solution, twice a week for a period of six weeks, with the same result after each application. As it destroyed layer after layer of the cancer, so the wound deepened; but at the same time the adjoining tissues closed in by granulation from every point of healthy tissue, as this was stealthily reclaimed from the invasion of the cancerous growth, until at length, *even under the continued application of the agent*, the whole surface threw out healthy granulations, and the wound healed with scarcely a mark, and without loss of healthy structure.

B. V., aged 30, was admitted into the Great Northern Hospital for an affection of the under lip, supposed to be an erectile tumour. It consisted of a large swelling occupying the whole of the lip, of a deep purple colour, elastic, and without any abrasion of surface. The coronary arteries had been tied when the tumour was much smaller, but without any apparent results; for it continued to grow, until at length it became both so unsightly and inconvenient that the patient desired its removal. I removed it in the usual manner, taking care to include the whole of the diseased structure in the resected tissue. The hemorrhage was unusually great; the blood flowing from many small, as well as from the usual larger arteries. The edges of the wound were brought together by pins.

The growth was, to outward appearance, simple erectile tissue; but, from the sequel, it will be found to have been also the seat of cancerous elements. On the third day, the two portions of lip had only united at the edge; whilst the remainder of the wound, even the wounds made by the needles, and its alveolar base, had become covered by a fungoid excrescence, which discharged blood and a thin pus; grew rapidly, and left no doubt of its malignant character.

Further cutting was out of the question. I at once applied the solution of the pernitrate of mercury. It was attended with great pain. A considerable slough came away. The application was repeated daily, and with the same results.

Although the growth was rapid, the destructive powers of the agent were still more so; and to my surprise, as well as that of my colleagues, the wound (which now assumed the appearance of a large ulcer) began to contract in size; the healing edge keeping close up to the limits of the diseased growth. and following it as this gave way to the action of the caustic, until cicatrisation became complete, and the patient was discharged well.

The obvious value of this agent lies in its being fatal to the disease, and powerless over the healthy tissues. The one it destroys, whilst it spares the other; and not only so, but it appears to quicken the healing energies of the latter; for the repairing process keeps pace with the speed with whicht he former comes under its exterminating influence; so that no sooner is the last vestige of the disease gone, but the wound is almost cicatrised.

In the Guy's Hospital Reports, Mr. Bryant speaks most favourably of excision in these cases; and I can have no doubt whatever of the conclusion to which that very able and observant surgeon has come on this point; but there are circumstances which give to the caustic method a value which excision does not possess, although I do not claim for it greatly preponderating advantages. There is no loss of tissue by this mode; whereas the whole lip is lost by excision. This might be an advantage; for it may relieve the patient of a part of the face very prone to become the seat of cancer; and to which it may return if the diseased growth alone be removed. On the other hand, the advocates of conservative surgery who with the knife very nearly imitate the caustic, aver that this liability is by no means so great as to constitute an argument in favour of the wholesale, against the limited, removal of the lip-structures.

Again, patients often prefer caustic to the knife; although, as far as pain is concerned, it is, I think, saved by excision.

From epithelial cancer, I go to *lupus exedens*.

Although this affection has been too much looked upon as constitutional, and not admitting of cure but through internal remedies, I can affirm, from repeated experience, that it is curable by means of the solution of the pernitrate of mercury.

The first case in which I tested its powers was that of a lad, aged 18, from Walthamstow, the whole side of whose face had become exceedingly unsightly by this affection in a very severe form. I at first limited the application to a small segment of the ulcer, after carefully freeing the surface of scab and moisture. The effect of the first application encouraged me to bring the whole surface as rapidly as possible under its influence. A few dressings, three days apart from each other, and the intermediate use of a weak solution composed of Condy's disinfectant and opium, were sufficient to bring about reparative action in the sore, and ultimately its complete cicatrisation.

A young woman, aged 24, suffered for years from lupus exedens affecting the skin of the face, over the angle of the lower jaw. I first applied the solution about six weeks since; and have repeated it since about once a week. The sore has cicatrised.

I might give several other cases; but they would be merely repetitions. Suffice it to remark that the remedy does not appear to act in the same manner that it does on epithelial cancer; for it does not destroy the textures with which it comes into contact so suddenly as to produce an eschar. This may, perhaps, be due either to the fact that I have not applied it so vigorously in these, as in the former class of cases; not having found it necessary to do so; or to the greater resistance shown by lupous tissue to the agent; for this tissue differs radically from cancer, in consisting principally of newly formed connective tissue with nucleated cells.

The third form of disease in which I have been in the habit of using this remedy, and that, to a certain extent, satisfactorily, is the *induration of chancre*. I cannot help thinking that the tissue which forms the indurating ele-

ment differs entirely from that in which it is imbedded. The unaided eye will soon learn to distinguish it by its colour and texture; and sometimes by its slightly overlapping the contiguous healthy tissue; but whether it is altogether a new *quasi*-parasitical growth, or is a vitiation of the natural tissues by which these take on new characters and action, it is difficult to say. At all events, there is diseased structure, and its elements have powers of increase and induration.

There is, unquestionably, much difficulty in ridding parts of this tissue. It will sometimes disappear under a course of mercury, whilst at others it will not. Occasionally, it yields without mercury; whilst again, it will resist a long course of this remedy, remain for months after it has been given up (the ulcer having healed), and then melt away as though spontaneously. I have seen one of two indurated masses disappear, as well as syphilitic tubercles, on one side of the face, under treatment, whilst the other nodule and the tubercles on the opposite side have remained without any change. There is scarcely anything more embarrassing than the seeming caprices of this morbid element. The question is how best to get rid of it, *i. e.*, most speedily and most effectually. I have found the pernitrate of the greatest value towards attaining this end. And I cannot but think that at least a plausible inference of some practical value may be drawn from comparing the processes by which epithelial cancer and the induration of chancre are made to disappear before the pernitrate topically applied in the one case, and mercury systematically administered in the other.

In both the effects are, to outward appearance, the same, with the exception of the removal of the chancrous tissue, which is not by masses, as in the case of the cancerous. But this difference does not injuriously affect the analogy; for it may consist simply in that which exists between ulceration and sphacelus; and in this respect the effect of the pernitrate on the chancrous tissue may closely assimilate that which it has upon the tissue of lupus. But, returning from this necessary digression, it may be observed that the effects of the two remedies on the two diseases respectively resemble each other, inasmuch as in both, as the disease yields, so the contiguous tissues make themselves obvious by a ring of healthy redness, which follows close upon the retiring base of the disease, until the latter disappears, and granulation closes up the sore. Does not, then, the mercury as it is brought by the nutritive juices to the comparatively feebly resisting chancre-tissue, act upon it simply as a poisonous agent, destroying it histologically by setting up a species of molecular gangrene; and thus represent the same kind of action as that which is exerted by the pernitrate upon cancerous tissue, only upon a larger scale? And, if so, is not the pernitrate a more ready means of getting rid of the induration than ptyalism? I grant the use of the mercury, both in its curative character, as an adjunct, and as a preventive, so far as constitutional syphilis is concerned, to be most important; but would it not be better, in all cases admitting it, to combine the two? I have repeatedly done so, and with the best effect. But it must be remembered, as the destructive powers of the caustic are made to penetrate deeper and deeper into the chancrous mass, it is not necessary, were it possible, to continue it until the whole of the induration is gone, but only until the wound shows unmistakeable evidence of healing in; for the cicatrix, in these cases, has a peculiar induration which, at least, resembles that of the diseased tissue, and cannot be got rid of.—*British Med. Journal*, *January* 18, 1862, *p.* 59.

10.—CASES OF CANCER SUCCESSFULLY TREATED WITH THE ARSENICAL MUCILAGE.

By Dr. W. Marsden, Surgeon to the Royal Free and Cancer Hospitals.

[The mere recording of operations for the relief of cancer, seems to be of little or no value, unless sufficient time be given to warrant at least a well-grounded hope that a relapse or return of the malady is not likely to occur.

These cases, however, occurred some time ago, and up to the present time there had been no relapse.]

Case 1.—Charles M., from Worcestershire, aged 70, was admitted into the Cancer Hospital, Brompton, on the 13th of October, 1858, and was discharged cured on the 18th of May, 1859. A cancerous ulcerated sore occupies about a half of the lower lip, and the left angle of the mouth. It threatens to proceed upwards towards the cheek and upper lip. It resembles at first sight a warty excrescence, but has a very formidable look; nor can there be a doubt of its malignant character, whatever be the view of pathologists in respect of the true nature of epithelial cancer. The patient had been treated for three months in the Worcester Infirmary. Soon after admission, the arsenical mucilage was applied as a caustic dressing to the parts affected. Under this treatment he rapidly recovered, and continues well to this day.

It may be useful to add, that the disease was of about eighteen months' standing at the time of his admission into the hospital. The arsenical mucilage was first applied on the 10th of Nov., and discontinued on the 16th of December. The progress towards a cure was uninterrupted.

Case 2.—S. G., aged 56, married, and had two children, was admitted as an out-patient of the Cancer Hospital, Piccadilly, on the 15th March, 1859. The history of her case is as follows:—After a fall occurring about a year previously, there appeared to the left of the umbilicus and in the umbilical region a tumour, which, small at first, has now increased to the size of an egg. The tumour is of a deep-red colour, slightly ulcerated over the greater part of its surface. It is firm, and attached to the abdominal walls by a broad base or pedicle. As the means employed seemed to have no influence over the morbid growth, she was recommended to become an in-door patient in the hospital at Brompton. She was accordingly admitted on the 3rd of May, 1859. She enjoys pretty good health, and complains only of a sense of weakness in the limbs. After a few applications of the arsenical mucilage, the tumour sloughed off, leaving a clean surface, which speedily cicatrized, and she was placed on the list of out-patients on the 17th June of the same year. Soon after leaving the hospital this patient lost the use of the lower extremities, but from which she soon recovered. The cicatrix is quite sound, and she continues in good health, and without any relapse, to the present day.

In this case, as in all the others treated in the Cancer Hospital, the application of the arsenical mucilage was rigorously limited to the fungous structure, or, in other terms, to the diseased parts, experience having told me that to extend the application of the medicament to the healthy structures is a proceeding fraught with great danger.

The tumour thus removed is preserved in the Pathological Museum of the Hospital; it is numbered 56. A correct coloured drawing of the tumour, as it appeared when the patient first entered the hospital, will be found in the folio of pathological drawings forming a portion of the Museum; it is numbered 55.

Case 3.—S. F., aged 68, of Calston Colne, was admitted into the Cancer Hospital, Brompton, on Sept. 21st, 1859, for an epithelial cancer (cancroid) of the lower lip, which first appeared about ten months previously. His general health seemed good. The arsenical mucilage was immediately applied twice a-day, and simple soda powders were prescribed to be taken also twice a-day. Under this treatment the fungous growth was speedily thrown off, the secretions from the surface became of a healthy character, and on the 5th of October the black lotion was substituted for the arsenical mucilage. He was discharged cured on the 14th October, 1859, and continues well to this day.

Case 4.—M. N., aged 71, was admitted into the Cancer Hospital, Brompton, on the 20th of June, 1860, for a fungous tumour, of the size of a small apple, seemingly growing from the scalp, close to the fronto-parietal suture, and somewhat towards the right side. To this tumour, which appeared to have no cranial or dura-matral attachments, though strongly resembling such tu-

mours, the arsenical mucilage was applied with great caution; and, when the pedicle, which was of considerable breadth, had nearly sloughed through, it was divided with the knife on the 1st of August. No hemorrhage followed. The bone was found bare and denuded of the pericranium for about the extent of a shilling. Granulations of a healthy character spread over the wound, and a healthy cicatrix ultimately formed. A small red spot marks the original situation of the tumour. On the 12th of October last she had continued quite well, nor has the disease re-appeared.

It is right to remark that the growth resembled very strongly those terrible tumours which, growing from the external surface of the dura mater, terminate by perforating the cranium and spreading over the surface of the scalp.

[The author omitted to give the formula of the arsenical mucilage. The following formula is from a short subsequent note on the subject.]

The following is the formula:—Arsenious acid, gum arabic powder, of each one ounce, mixed with five drachms of water. The part affected to be painted over with the mucilage night and morning, never exceeding one superficial inch. As the part becomes deadened, it must be allowed to slough off, aided by the application of a simple warm bread-and-water poultice. When all the diseased part has been thus got rid of by the repeated application of the mucilage, a carrot poultice should be applied during the night, and a weak black lotion during the day (calomel, one drachm; lime water, one pint), until the part is entirely healed.

Medical treatment.—In persons of general good health, bicarbonate of soda, fifteen grains, night and morning; in persons of leuco-phlegmatic habit, one soda powder every night on going to bed, and the following draught twice during the day:—Hydrochloric acid, one drop; compound tincture of cinchona, one drachm and a half; to one ounce and a half of water. Full diet, with malt liquor, I consider to be absolutely necessary, avoiding spirits and tobacco, but wine I recommend to those who are in the habit of taking it.

It is by this mode of treatment I consider such a change is produced in the constitution as to arrest and even destroy a cancerous action, as I have not yet seen single instances of the disease returning in any of the cases thus treated. There are several cases now under treatment, which can be seen by any medical man who may desire to visit the hospital.—*Lancet, March* 1 *and* 22, 1862, *pp.* 220, 319.

11.—ON ACUPUNCTURE IN THE TREATMENT OF MUSCULAR RHEUMATISM, AND ON GALVANISM IN RHEUMATIC PARALYSIS.

By Dr. Arthur Leared, Physician to the Great Northern Hospital.

The treatment of muscular rheumatism is sometimes so unsatisfactory as to exhaust the patience alike of the sufferer and of his medical attendant. In the following remarks I wish to show the efficacy of two powerful remedial agents—acupuncture and galvanism. The latter is at length beginning to claim the attention it deserves; but acupuncture, though employed by that observant and practical people, the Chinese, from immemorial time, has not been fairly tested in our modern practice. It is not my intention to offer any speculation on the *ratio medendi* of this curious mode of treatment—only remarking that it possibly depends on the production of galvanic currents.

Several years ago I was consulted by a lady for rheumatism, the principal seat of which appeared to be the right deltoid muscle. It was attended by considerable loss of power, and the severe and constant pain had seriously impaired the patient's health. Hearing that a number of approved remedies had been employed without effect, it occurred to me to give acupuncture a trial. Three or four needles were deeply inserted in the affected part, and allowed to remain about an hour. The result was complete cure of the pain, and the full use of the arm was recovered.

Many cases apparently of the same sort of rheumatism came under my treatment when I was physician to the Civil Hospital at Smyrna during the late war. Acupuncture was tried in one case, in which pain was also referred to the deltoid muscle, but it seemed to be of no avail. But while I think it right to mention this, it is to be added that there was too often cause to suspect either that no pain really existed, or that its relief was tardily admitted. The harassing duties of the Crimean trenches, from which these patients had recently escaped, made them sometimes unscrupulous in devising means to delay their return to duty.

In the next instance, the details of which I give from my note-book, acupuncture was quite successful.

A gentleman, fifty years of age, and of strong constitution, consulted me January 20, 1860, for rheumatism, contracted, as he believed, from sleeping in a damp bed while travelling. He had for some time suffered severe pain in the right arm and shoulder, but not immediately in the shoulder-joint. It was evident that the deltoid muscle was the part chiefly affected, but pain also extended in the course of the trapezius muscle to the back of the head. Loss of power over the arm had gradually ensued, and was so complete when I first saw him, that with the scapula fixed he was incapable of raising it in the smallest degree. The temperature of the part seemed to be rather below that of the other arm, and sensation was slightly impaired.

I prescribed the iodide of potassium in full doses, and it was continued until its specific effects were experienced, but no benefit resulted. Colchicum was also fairly tried without any benefit. Tonics and sudorifics in succession shared the same fate. In short, with the exception of temporary relief obtained from opiates, rendered necessary by the suffering and loss of rest, no internal treatment appeared to benefit the patient. In the meantime, external applications were not neglected, stimulating and narcotic liniments, containing ammonia, turpentine, tinctures of opium and aconite, &c., were diligently rubbed in. Sulphur was applied to the arm by means of a flannel bandage. The effect of heat was tried by means of bags of salt as hot as could be borne, and finally, the firing iron of Dr. Corrigan was freely employed. But the benefit afforded by any of these measures was either so trifling or so transitory that I felt almost in despair about the case.

The signal success which attended the use of acupuncture in the case first mentioned, caused me, however, to suggest it to the patient, who consented to have it tried. I now cautiously introduced three stout needles into the deltoid muscle until they touched the humerus, at about equal distances from each other, and the needles were left inserted about an hour. Almost immediate and permanent relief was obtained. The patient scarcely complained from that time of any pain in the shoulder or arm, but the paralysis continued as at first. For this state of things galvanism naturally suggested itself. I began with its milder effects, using the ordinary induction apparatus and the intermittent current, while the force of the shocks was gradually increased. The result was perfectly satisfactory; the patient was galvanised on alternate days for about a fortnight, and every application was attended by an improvement, rendered visible by the increased power of raising the arm. On February 29 (at the end of the time above named), he declared himself quite recovered, with the exception of an aching which occurred in the side of the neck when he suddenly moved it, but this comparatively trivial symptom did not leave him for a considerable time. I ascertained very lately that no relapse had taken place, and all unpleasant sensations had long disappeared.

The foregoing successful cases prove the great value of acupuncture, because in them many other remedies had been previously employed without avail, whilst its speedy action indicated its real efficacy.

The potency of galvanism was also shown in one of the cases, but there is sufficient evidence that it usually acts beneficially whenever, as in this instance, the paralysis is due to a peripheral affection of the motor nerves.—*Medical Times and Gazette, Nov.* 30, 1861, *p.* 550.

12.—ON THE TREATMENT OF RHEUMATIC FEVER, AND ON THE USE OF ALCOHOL IN SERIOUS CASES OF THIS DISEASE.

By Dr. LIONEL S. BEALE, F. R. S., Physician to King's College Hospital.

In the London Hospitals you have many opportunities of studying the course of serious cases of acute rheumatism. Let me recount very briefly some of the principal points met with in examples of the disease which more frequently come under our notice in the wards of this hospital.

The patients are very generally weak, ill-nourished, overworked, and often under-fed, and insufficiently clad, unhealthy people. They are very pallid. The joint-affection varies much in intensity in different cases. There is generally free sweating, or this is easily induced. Pericarditis often comes on very early in the disease. The quantity of lymph effused on the surface of the pericardium is not very great; but there is a tendency to effusion of serum as well; and in many, effusion into the pleura also occurs. It is by no means uncommon to find small patches of solid lung; but the pneumonia generally affects the surface of the lung, and is not extensive. Often the inflammation resolves and recurs in different parts of the lung from day to day. The prostration of strength is considerable, and the pulse is, of course, very quick, and generally weak. If delirium occurs, it depends on exhaustion, and usually ceases after the administration of stimulants. In many cases, we have reason to believe that the recurrence of delirium is prevented by the timely administration of support and stimulants. These cases do well on a supporting plan of treatment, combined with the administration of alkalies and opium, which latter drug may be given in comparatively large doses, frequently repeated, without exerting its ordinary action on the system.

Another class of cases, also complicated with pericarditis and pneumonia, is met with; but examples of it are seldom seen in large towns; and in the course of twelve years, I do not remember to have seen more than two or three instances in the wards of our hospital. The patients are comparatively strong, well-nourished, and sometimes florid people. The skin is dry. Fever runs high. The pulse is quick and bounding. The delirium is violent. The quantity of lymph in the pericardium is often enormous; and I believe death may result from the interference with the heart's action thus produced, and the congestion of the lungs which results. I have had very little experience in the treatment of these cases; but I have seen alkalies, various sudorifics, opium, and support, employed in vain. If I were called upon to treat one of these cases now, I would certainly take blood, partly with the view of relieving the congestion of the internal organs, but mainly in the hope of favouring sweating and the action of the kidneys. In fact, in such a case, I believe the right plan of treatment would be to attempt to modify the changes in such a manner as to reduce it to the character of the cases first alluded to.

I propose to confine my remarks on treatment to the first class of cases. Several of these are admitted into our wards every session; and those of you who practise hereafter in large towns will, no doubt, be called upon to treat such cases frequently. They occur among the better class of persons as well as among the poor. Children, students, and young people of both sexes, are principally the subjects of these attacks; but the disease is not exclusively confined to children and persons under thirty or forty years of age.

The question of treatment is very important; and unless you are well acquainted with the course of these cases, and know the probable and possible complications which you may have to treat, and are familiar with the earliest symptoms of these complications, you cannot undertake such a case with justice to the patient, or satisfaction to yourself. Fortunately the symptoms indicative of serious morbid changes are well marked; and, after having carefully watched two or three of these cases, you will ever after be on your guard, and will not fail to note the earliest symptoms of the heart or lungs being attacked, and modify your treatment accordingly. The *immediate* danger in such cases results from the great liability to acute inflammation of

the pericardium, the pleura, and the lungs; but, supposing the patient to recover from the attack, he may not be as healthy as before, for in too many instances the pericarditis is followed by an alteration in the condition of the valves of the heart. Regurgitant disease of the mitral orifice very commonly results. I have, however, known some instances in which this affection hardly seems to have influenced the duration of life. But among the poor, and people who are compelled to work hard, so that the gradual accommodation which naturally takes place, under the altered condition of the heart's action, in the organs of respiration and circulation, is interfered with, you generally find that the breathing becomes permanently affected; enlargement of the right side of the heart, with weakening of the right ventricle, results; and attacks of general dropsy add to the patient's distress. The case often terminates fatally a few years after the heart affection began.—*British Med. Journal, Jan.* 25, 1862, *p.* 81.

13.—*Indian Remedy for Small-pox.* By Dr. M'WILLIAM.—[It is well known variola carries off large numbers of the North American Indians. During a late epidemic the Indians took a remedy given them by an old squaw, and which was held amongst them to be infallible. Compared to it, the remedies given by the white doctors were considered "no good." From the information gathered from the Indians the following observations have been carefully sifted.]

1. In the case of an individual suspected to be under the influence of small-pox, but with no distinct eruption upon him, a large wineglassfull of an infusion of the root of the plant "Sarracenia purpurea," or pitcher plant, is to be taken. The effect of this dose is to bring out the eruption. After a second and third dose, given at intervals of from four to six hours, the pustules subside, apparently losing their vitality. The patient feels better at the end of each dose, and, in the graphic expression of the Micmac, "knows there is great change within him at once."

2. In a subject already covered with the eruption of small-pox in the early stage, a dose or two will dissipate the pustules and subdue the febrile symptoms; the urine, from being scanty and high coloured, becomes pale and abundant; whilst from the first dose the feelings of the patient assure him that "the medicine is killing the disease." Under the influence of the remedy, in three or four days the prominent features of the constitutional disturbance subside, although, as a precautionary measure, the sick person is kept in camp till the ninth day. No marks of the eruption (as regards pitting, &c.) have been left in cases examined that were treated by the remedy.

3. With regard to the medicine acting (as is believed by the Indians) in the way of a preventive in those exposed to the infection, it is curious to note that in camps where the remedy has been used the people keep a weak infusion of the plant constantly prepared, and take a dose occasionally during the day, so as to "keep the antidote in the blood."—*Lancet, Dec.* 7, 1861, *p.* 550.

14.—ON ANÆMIA.

By Dr. THOMAS K. CHAMBERS, Fellow and Censor of the College of Physicians, Physician to St. Mary's Hospital, &c.

[Anæmia, or "bloodlessness," means in scientific language a deficiency of red discs in the blood. The word has been objected to as not being strictly correct; and "Spanæmia," or "thinness of blood," has been proposed in its stead. Neither, however, is this strictly accurate, as thinness of the circulating fluid, and deficiency of red globules, are not necessarily co-existent. In reality there is no occasion for fault finding.]

Anæmia, without obvious organic lesion, when properly treated, is a very curable condition, and this should still further reassure you, that you miss

nothing by not being able to study its post-mortem pathology. For transitory and curable states leave but little foot-prints behind them for morbid anatomists. In a great majority of cases they depend upon the mucous membrane, of all the tissues in the body the one most affected by mortuary changes.

To the mucous membranes I am disposed to attribute the condition in which we find our present patient. The two circumstances to which I have traced the illness both act directly or indirectly on this tissue. The mental exertion involved in an unusual responsibility thrown on a conscientious person would arrest the action of the involuntary muscles which carry along the mass of food through the alimentary canal. You know well the time your food is in leaving the stomach if you are called to an important midwifery case just after a hearty meal; and several commercial and literary men have complained to me of attacks of vomiting (that is, temporary paralysis of the stomach), when they took dinner alone, and so were apt to let the mind dwell deeply on some interesting subject; and they have told me in wonder that they could dine out and eat and drink all sorts of rich things with impunity. They did not seem aware of the physiological value of frivolous conversation. At the same time that the moral causes thus impeded digestion, the unwholesomeness of the air in the close shop poisoned the mucous membranes, diminishing their vitality and causing them to be abnormally covered with a thick layer of mucus. Remember that, in spite of their name, it is not the business of mucous membranes to secrete mucus; the more perfect is their condition, the more favourable are the surrounding circumstances, the less they do so. From many persons' lungs not a drachm of expectoration is thrown up in a month, and the vast surfaces of the intestines and bladder are equally innocent of even microscopic traces of mucus in the typical health we desire to experience. It is only when the presence of some material agent diminishes their vitality that the mucous membranes exhibit on their surfaces that peculiar substance whence they take their appellation. And the greater the diminution of life, the greater the secretion; a slight cold in the head will be accompanied by slight catarrh, a severe one by excessive catarrh; and the nearer the approach to death, the nearer it is, so that the death rattle, or overpowering collection of mucus in the bronchi, is a popular warning that all is over. Be careful not to look upon mucous secretion as augmental life; it is in fact a partial death.

Well, the poisoning air having covered these slowly moving mucous membranes with a thick tenacious coat, the entrance of alimentary substances into the veins and absorbents was impeded, and our patient starved in the midst of plenty. So all the usual signs of starvation followed. First, hunger,—by no means a constant accompaniment of chronic deprivation of food, yet sometimes present as here; then anorexia, a much more frequent phenomenon; then paleness, languor, weariness, and pain in the stomach; then anasarca, and, in short, the other more marked symptoms of anæmia.

You may observe that the loss in these constituents of the body, which are of a nitrogenous chemical composition, is more marked than that in the hydrocarbonaceous fat. The reason is, partly, that the destruction of adipose vesicles is somewhat concealed by the saturation of the tissue with serum, which gives it a false plumpness—partly, that fat, being absorbable without much, if any, alteration, is easier taken up than fibrin or albumen, which require a chemical solution before they can be absorbed. So that though starved, our patient looks but little emaciated.

All that I have said before, of course, has for its end the treatment. My aim in anæmia is to introduce as quickly as I can the largest possible amount of, 1, nitrogenous food; 2, iron; 3, chlorine. When I say "introduce" I do not mean "throw in," or get swallowed, but assimilated in the system.

As regards the first, it is obvious that if I had written down ever so many "ordinary diets," a patient to whom the very sight of food was an abomination, would have gained nothing by it; she would simply have gone without. I directed, therefore, no meals at all, and no solid food, but a cup of milk with some lime-water in it, to be given as medicine every two hours, and a pint of

beef-tea in small, divided doses during the day. After two days she managed an egg also daily, and after twelve days of gradual additions of this sort, you will find her on full allowance of mutton chop, porter, beef-tea, and milk.

Iron is required to supply the new growth of red discs which we hope for, with their metallic constituent. You cannot get it into the system in any way so quickly as the mistura ferri composita of the London Pharmacopœia. Large doses of more soluble salts have an action on the mucuous membranes which not only prevents them being taken up, but also arrests the digestion of food. Evidence of the latter is found in loss of appetite and feverishness, and of their own rejection in the blackening of the stools much sooner than by the form I have approved of. So in spite of the elegant preparations which are constantly put before us, as recommended by their solubility, such as the chloride, acetate, citrate, phosphate and other salts of iron, I prefer the unchemical mixture. It seems as if the carbonate which is preserved from decomposition by the sugar, and the finely-divided oxides diffused through the thick liquid were peculiarly easy of solution in the water saturated with salts and carbonic acid, which (and not pure water) we must remember is the solvent to be considered.

I have found that some cases which did not improve so quickly as I could wish under the above treatment, made a sudden start of improvement when to it was added the administration of chlorine in the form of warm hydrochloric acid baths. More iron is taken up,—the blackening of the fæces ceases, and therefore perhaps it may be that the presence of more acid in the system attracts more of metal. But in a few cases I tried for experiment the hydrochloric acid baths alone, and even then it was beneficial, seeming to confer muscular strength like what are commonly called tonic drugs. I cannot but think, therefore, that it supplies a distinct want in the system, and that it is a directly restorative medicine in anæmia.

Nor is it difficult to make this empirical observation accord with rational pathology. In anæmia the blood is more watery than natural; the proportion is deficient, not only of organic matters, but of salts. Chloride of sodium is the most important of these, and the supply of one of the constituents of this material we may reasonably imagine is an aid to the renewal of life, which is the end of all medication.

Besides the above-named medicines, you will see, I have ordered Pil. aloes cum myrrha, gr. iv. omni nocte sumenda. Now, do not suppose that this is ordered merely as a purgative, and that any other purgative would do as well. On the contrary, most purgatives do harm in anæmia. Gamboge, castor-oil, sulphate of magnesia, colocynth, mercury, and several others, which produce serous elimination and augment secretion generally, would do harm just in proportion to their activity. It seems established by the experiment of making them act as purgatives when injected into the circulation, that their soluble principles have a destructive agency over the blood; whereas the soluble alkaloid in aloes (aloine) is, in fact, a bitter tonic, and the purgative power of the drug resides in its insoluble resin. Its action is very slightly eliminative—in moderate doses it only slightly augments the solid brown excreta of the colonic glands, and produces fæces feculent in smell and of consistent form; whilst at the same time it restrains, by its bracing bitter, the formation of mucus, as you may clearly see by its action on moist piles, how it dries them up and makes them smart. And by the more vigorous peristaltic action and by the solid mass passed along the gut, the already existing mucus is cleared away. Aloes, therefore, is employed strictly as a clearer of the intestinal, especially of the colonic, membrane. It is joined with myrrh, partly to divide it minutely, and make a small dose go farther, and partly to get the advantage of the extra resin.

November 28. A fortnight ago I lectured about an anæmic patient. She was then showing a tendency to lose her title to the name, and now she certainly cannot claim it, and has earned our confidence in the statement that her natural hue is rosy. She leaves the Hospital to-day, having manufactured enough red discs to colour her blood throughout very sufficiently.

What amount of manufacturing industry does this show? Let us reckon

She weighs 8 stone, or 1792 ounces; of this 2-7ths, or 512 ounces is blood; and of this blood 133-1000ths, that is to say, 60 ounces, should be red globules. Now the analyses of MM. Andral and Gavarret show that in cases of anæmia of at all a marked character (as this was), we may expect, at least, three-quarters of the red discs to disappear, so that when she came into the hospital it may be fairly assumed that she did not possess above 15 ounces; and now I think with equal fairness she may be assumed to have got up to 45, which is conceding that she still wants a quarter of perfect health. By this reckoning she must have made 20 ounces of red blood-discs; that is, the most important organic constituent of upwards of 150 ounces of blood, in a month!

Mark the power of renewal which the human body has under favourable circumstances, and learn from this not only the curability of anæmia when it is a disease, but also the facility of repairing artificial loss of blood when it is employed as a remedy. It has been the fashion lately among certain medical declaimers to paint the physician who draws ten or twelve ounces of blood from the arm as a deadly villain, who necessarily *ex vi termini* takes away "the life," or that which cannot be replaced. Not only pill-dealers and quacks have raised this outcry, but it has been joined in by some whose knowledge of physiology ought to have taught them better. It ought to have taught them the fallacy of the popular notion, and the scientific argument by which to refute it. You will clearly perceive from the calculations through which I have taken you that by proper management no loss is so easily repaired, and that if he saves his patient two nights' sleeplessness or pain, the price of a venæsection is well spent.

Only note this, that *if the loss is to be repaired, the means of repair must be given.* When I bleed, you will observe that I take down the diet card and accommodate it to the circumstances, being very careful that the patient has the wherewithal to replace the globules I am detracting. I supply with one hand what I am taking away with the other. I begin to cure the artificial anæmia, which I feel myself called upon to produce, at the same time that I am producing it. "Blowing hot and cold" you will say. Precisely so—that is what I intend. I blow cold with my bleeding, not for the sake of blowing cold, but because it is the inevitable accompaniment of the remedy. I employ the remedy not to produce anæmia, but for other quite different purposes which I think are worth the cost. And I blow hot to compensate as well as I can for the evil I think it desirable to do, on the principle

"Necesse est facere sumptum, qui quærit lucrum."

I do believe that the sad effects of the excessive venæsection of our fathers, which with justice have been thrown in the teeth of the medical profession, was due quite as much to the starvation as the bleeding. I have a most vivid and painful recollection of seeing, when I was a student in Paris, M. Chomel and others treating pneumonia. I could not at first understand why in France so much more marked, and, in my opinion, so much more deleterious effects were produced by the venæsection than in England. At that period we had at home ample opportunities of seeing blood-letting practised; but I never saw such prostration produced by it at St. George's as I did at the Hotel Dieu. Then I noticed that the order for "*Saignee*" was accompanied by "*Diete absolue.*" I almost doubted my knowledge of French, and was obliged to ask several of the bystanders before I could believe that this meant an *utter deprivation of all food!* There was an instantaneous explanation of the comparative toughness of my countrymen; for never in our worst days did we carry the Sangrado practice so far as that. We did not give food enough, perhaps, but we never commanded that it should be intentionally kept out of our patients' way.

The bad practice of starving and bleeding at the same time, took its rise from the error of Allopathy. In this system a disease is an enemy to be overcome—a something *to be combated by* an agent which is as opposite to it as possible. Bleeding was found by experience to be useful in certain morbid states; therefore it was useful in virtue of its opposite effects. Anæmia and

depression of life, are the most constant effects of bleeding; therefore anæmia and depression are the benefactors to be sought for, and whatever aids bloodletting in producing anæmia and depression, is a good companion to it. It is unnecessary to say, that of course starvation was the first agent thought of, adopted *à l'outrance* by the logical French, and with more hesitation by our fortunately illogical countrymen. The abuse has brought about a reaction; and that treatment which was considered at one time so specific that its gravest faults were viewed as virtues, now runs a risk of being denied *all* virtue because of its avoidable faults.—*Medical Times and Gazette, Jan.* 11, 1862, *p.* 26.

DISEASES OF THE NERVOUS SYSTEM.

15.—ON THE PHYSIOLOGICAL AND MEDICINAL PROPERTIES OF SULPHATE OF ANILINE; AND ITS USE IN THE TREATMENT OF CHOREA.

By Dr. James Turnbull, Physician to the Liverpool Royal Infirmary.

[The artificial alkaloids constitute a numerous class, and from their resemblance to the vegetable alkaloids in chemical constitution, we should expect them to produce powerful, and we must hope, good effects on the animal economy. This, however, remains to be settled by future experiment. Aniline is one of the best known of the artificial alkaloids.]

Aniline is a volatile oily alkaloid which forms crystallizable salts with most of the acids. To the chemist it is a very interesting body, as it forms numerous compounds with other bodies, and its radicle, phenyle, connects it not only with indigo, and its derivatives, but also with carbonic acid, which is the hydrated oxide of phenyle, and with benzoyle and salicyle. There are various processes by which it may be obtained. From indigo it may be obtained by distilling it alone, or with potass; or it may be obtained by heating isatine with potass. It is also present in coal-tar, from which it is now extensively prepared for the purpose of forming certain dyes.

Two circumstances led me to make trial of aniline as a medicinal agent—the fact of its being an alkaloid, from which I inferred that it would act energetically on the animal economy, and, probably, on the nervous system, and the fact of its being present in Dippel's animal oil—an old anti-spasmodic remedy. This oil, Pereira tells us, is undoubtedly a very powerful agent. Swallowed in moderate doses, it stimulates the vascular and nervous systems, and is esteemed anti-spasmodic. It has been used, he states, in hysteria and other affections of the nervous system accompanied with convulsive movements. There are other alkaloids in this oil—pyridine, picoline, lutidine, and collidine—to which its properties may be equally due; but the fact of aniline being the chief of them was one of the reasons that led me to make a trial of the sulphate of this artificial alkaloid.

Chorea is a peculiar convulsive affection of the nervous system, which yields, in the majority of cases, to the means which medical men ordinarily use for the treatment of this disease. Very obstinate cases are, however, occasionally met with which resist all the known means; and an extremely severe one came under my care, where all the usual remedies failed to produce the slightest benefit, and as the disease seemed likely to exhaust the patient, I considered it a fair one to make my first trial of sulphate of aniline. The patient began rapidly to improve, and as she recovered perfectly, I was induced to try it in other cases in which it has proved likewise successful; and I now proceed to give a brief account of six cases:—

Case 1.—*Chorea, with very violent twitching movements, loss of speech, &c.; unsuccessful treatment by purgatives, steel, sulphate of zinc, cod-liver oil, shower-bath, &c.; rapid recovery by means of sulphate of aniline.*—Ann P., a girl aged thirteen, was admitted into the Liverpool Royal Infirmary under my care on the 7th of June, 1860, on account of the usual involuntary twitching

movements which characterize St. Vitus's dance, and which affected all the limbs. She had been ill for three months. She was first treated with purgatives, and then successively with iodide of iron, cod-liver oil, the shower-bath, which is often of great service, and lastly, with the sulphate of zinc. None of these remedies, however, appeared to be of any use. On the contrary, she got worse, and the violence of the convulsive movements became so great, that she was confined to her bed, in which it was a difficult matter to fasten her. She also lost the control of the muscles of the tongue so completely that she was unable to speak. On the 6th of August, two months after admission, and five from the commencement of her illness, the movements had become so constant, tossing her about in all directions, that they threatened to exhaust the vital powers, as sometimes happens in this singular disease, and I thought the case, therefore, a fit one, after having used the ordinary means, to make trial of a new remedy. One grain of the sulphate of aniline was ordered to be taken in solution, with a little sulphuric acid, three times a day. In three days there was a decided diminution in the violence of the movements, and afterwards a gradual improvement. The dose was increased to two grains, and it then caused some depression and a peculiar blueness of the lips, which I have since remarked in several other cases. The medicine was omitted for two days, and resumed in the smaller dose. On the 30th of August she had so far recovered, that she could walk well. She had also regained the control of her tongue, so that she could put it out, and she had recovered her power of speech. On the 10th of September she had entire control of the limbs. They were almost perfectly still, and she was considered cured.

The successful result in this case led me to give it in the next, which was not, however, one of such unusual obstinacy and severity.

Case 2.—*Chorea affecting the left side chiefly; treatment by purgatives and sulphate of aniline; perfect recovery in twenty-one days.*—Mary Ann H., a rather delicate-looking young woman, aged 18, was admitted on the 30th of August, while the previous case was under treatment. She had the movements in all the limbs, but the left side was most affected. She had not menstruated for twelve months, and the bowels were constipated. They were first opened freely by compound jalap powder and calomel, and then a grain of sulphate of aniline was ordered to be taken three times a day. It was afterwards increased to one grain and a half. She recovered rapidly, and the same blue appearance of the lips was developed which had been marked in the preceding case. On the 20th September she had scarcely any involuntary movement, and she was so well, and her hands so steady, that she could knit crochet-work. The catamenia being, however, still absent, decoction of aloes, with steel mixture, was prescribed instead of sulphate of aniline, and she became an out-patient.

Case 3.—*Chorea with twitching movements in the face and limbs, brought on by a fright; treatment by sulphate of aniline, and recovery in fourteen days.*—Martha G., a healthy-looking girl, 17 years of age, residing at Birkenhead, was admitted Dec. 20th, 1860, with very violent twitching movements in all the limbs as well as the face. A month previous she had a fright, which brought on the involuntary twitching at first in the right side. She had been under medical treatment from the commencement of the attack, but had notwithstanding been getting gradually worse. All the functions were natural, the bowels and the catamenia being regular. A grain of sulphate of aniline was ordered thrice a day, as in the previous cases. On the 24th there was less motion, and the dose was increased to two grains. The movements gradually subsided, and on the 3rd of January she had resumed the use of her needle. She then wished to go out, considering herself quite well.

Case 4.—*Chorea affecting the arms, legs, and muscles of the face; treatment by purgatives and sulphate of aniline; recovery.*—Margaret M'C., a stout, healthy-looking girl, aged 11, was admitted on the 27th of April, 1861, with twitching movements in nearly all the voluntary muscles. She had been ill ten days, but had also had a very severe attack about two years previous, for which she had been treated in the infirmary. The sulphate of aniline

was prescribed in the dose of a grain and a half three timesa day. The symptoms did not diminish at first, and on the 5th of May the movements were very violent and uncontrollable, and she was in bed. A purgative powder was ordered. The following day the lips, the face, and even the hands were of a deeper blue colour than I have observed in any other case. This had arisen from double the proper dose having by mistake been administered. There was a diminution, however, in the movements, and she was out of bed and running about. The movements gradually subsided, and on the 6th of June there was scarcely any jerking.

Case 5.—Chorea with the usual symptoms; treatment with sulphate of aniline and a purgative; recovery.—Mary Jane W., aged 13, was admitted on the 21st of May, 1861, with jerking movements affecting chiefly the right arm and leg. She had been ill three months, and had suffered from a similar attack, for which she had been under my care two years before. She was healthy-looking, but the muscular tissue was soft. The tongue was not clean. A purgative powder, with scammony, rhubarb, calomel, and sulphate of potass, was ordered. Two days afterwards the movements had not subsided, though the bowels had been freely opened. Sulphate of aniline was ordered in the dose of one grain and a half three times a day. Under this treatment the movements gradually subsided, and on the 6th of June there was scarcely any twitching. On the 21st she was discharged quite cured.

Case 6.—Chorea with convulsive movements and partial loss of power in the lower limbs; inability to speak; rapid recovery under treatment with sulphate of aniline.—Elizabeth L., aged 16, a thin and rather delicate girl, was admitted on the 2nd April, 1861. The case illustrated the connexion between chorea and rheumatism, for she had suffered from a severe attack of rheumatic fever eight months previous. She had begun six weeks before admission to have involuntary movements in the legs, and during all that time she had been under treatment at a dispensary. She had got gradually worse, however, so much so that she was carried into the infirmary, having lost the power of the limbs so completely that she was unable to stand. She had twitching movements in the legs, as well as the arms, and the face was also very much affected. When she attempted to speak, a peculiar chucking sound was produced, and she could scarcely say "Yes" or "No." The tongue was not quite clean, but the bowels were regular; the pulse was 100. One grain and a half of sulphate of aniline was prescribed three times a day, and a dose of compound jalap powder and calomel. On the 24th she was considerably better, and able to speak distinctly. On the 29th she was able to walk out, and the involuntary movements had greatly subsided. On the 9th of May the movements had almost entirely left her, and she might have been discharged if it had not been considered desirable to retain her, in order that her general health might be improved by good diet and porter, so as to prevent relapse. On the 20th there was scarcely any trace of the original complaint, but she had some rheumatic pain and swelling in one foot and wrist. Quinine with iron was ordered for this, and under this treatment she got quite well.

Sulphate of aniline has been given in two other cases which have come under my observation. One was a case under the care of my colleague, Dr. Inman, where it produced a decided beneficial effect; but the supply of the drug having run short, the cure was completed by other means. The other was a case under the care of my colleague, Dr. Vose. In this case a slow but perfect cure was effected by the remedy.

The beneficial effects of sulphate of aniline have been most apparent in severe cases of St. Vitus's dance, and the first and the sixth that I have detailed were as bad cases as any I have met with where recovery has taken place. The other were cases such as usually yield to the ordinary means at present in the hands of the profession. Chorea is a disease which is generally amenable to the means of treatment we at present possess. Obstinate cases, however, occasionally occur where the disease yields only very slowly, or even baffles our treatment altogether. I have seen it prove fatal in more than one case, and Dr. Hughes, in his "Digest of Cases of Chorea," published in

Guy's Hospital Reports, has recorded sixteen fatal cases. The introduction, therefore, of a new remedy, which appears to have a more direct and powerful influence on the disease than any we at present possess, is calculated to be of use, and I trust that it will be found worthy of the attention of the profession. It seems not improbable that it may prove a remedy of utility in other nervous affections. I have tried it in several cases of epilepsy, and in two cases it seemed to be of service ; but I have not given it a sufficient trial to enable me to recommend it confidently in any other disease but St. Vitus's dance.

We know but little of the *modus operandi* of many of our remedies, and I am not prepared to give any explanation of how sulphate of aniline cures chorea. I have, however, some observations to make on its physiological action, from which we may infer that it has a direct influence on the nervous system ; and also on the remarkable temporary change in the colour of the lips and skin which it produced in most of the cases narrated.

Physiological action of Aniline.—As we know that the alkaloids and their salts are almost indentical in their effects, we can scarcely doubt that the physiological action of aniline and that of the sulphate are the same, or very nearly so. Gmelin states that half a gramme of aniline introduced, together with a gramme and a half of water, into the stomach of a rabbit, caused strong clonic cramps, then laborious breathing, loss of strength, dilated pupils, and inflammation of the mucous membrane of the mouth.

Dr. Schuchardt arrived at the following results from a series of experiments :—Aniline may act injuriously on the animal organism, and in large doses may even cause death. Frogs introduced into a weak solution containing aniline died in periods varying from a quarter of an hour to two hours and a half, and death was also caused by the introduction of aniline into the mouth or into a wound in the back. Rabbits were also poisoned by this substance, a small animal being killed by fifty drops in six hours and a quarter, and a larger one by a hundred drops in four hours. In all the animals experimented upon, violent clonic and tonic spasms ensued after the application of the aniline, and continued almost uninterruptedly till death. There was also loss of sensibility, commencing at the lower extremities and extending to the upper ; the temperature of the body was also reduced. Wherever the aniline was applied locally, as in a wound of the back, on the stomach, on the posterior part of the tongue, or on the conjunctiva, appearances of irritation were observed, which are probably connected with the power possessed by aniline of coagulating albumen. The aniline was never detected in the urine, and it is probable that this substance is eliminated from the body rather by the organs of respiration than by the kidneys.

I gave to a dog about three months old half a drachm of the sulphate of aniline, and the effects produced corresponded in many respects with those observed by Dr. Schuchardt. About two hours and a half after it was given, the animal vomited, and an hour later it was purged. It became dull, weak, and tremulous; the pulse became very rapid ; the pulsations of the heart were counted 148 in a minute, and the breathing was somewhat laboured. The feet were cold, the tongue of a blue colour, and the hind legs almost paralysed. Five hours after, it was very weak, but recovering, and the next day it had recovered appetite and liveliness.

The sulphate of aniline does not seem to have the same local irritating effect that the aniline itself produces. My own observation would lead me to believe that it has a direct action on the nervous system. The most striking effect, however, which it produces is the visible alteration in the colour of the lips and complexion. A peculiar blueness of the lips, the tongue, and the nails, and a dusky appearance of the complexion, have been observed, in a greater or less degree, in most of the cases where a sufficient dose has been taken continuously for any considerable time. It is a temporary effect which has always gone off within twenty-four hours when the remedy has been omitted. In one case, where an overdose of not more than three or four grains was taken, the blueness in the hands extended above the wrists. There has been some depression and headache in some of these

cases, which symptoms have, however, gone off when the medicine has been discontinued. The blue appearance, though like that which occurs in chronic bronchitis, does not seem to arise from any interference with the respiratory function. This change of colour is more interesting from the fact, that we have few examples of parallel effects resulting from the exhibition of medicinal agents. The permanent discoloration of the skin which sometimes arises from the long continued exhibition of nitrate of silver is scarcely a parallel case, for here the oxide of silver is deposited in the skin from the remedy being decomposed by the action of the light; whereas the blueness from the sulphate of aniline appears to arise from a colouring matter being formed in the blood, which is most visible in parts like the lips where there is a thin mucous covering. The effect of madder-root (which is used as a dye) in colouring the bones of animals fed upon it of a red colour, is more nearly a parallel case; but the effect of the carbazotic acid and its salts—which are of a deep yellow colour, and produce a similar colour of the skin, as if the individual were artificially jaundiced—is the example which most nearly resembles that of the colouring influence of sulphate of aniline. This power which carbazotic acid has of colouring yellow, not only the tissues but also the fluids of the body, was discovered by my friend Dr. Moffat; and it is worthy of observation that both aniline and carbazotic acid are derivatives from the radicle phenyle.

If the blue colour produced by sulphate of aniline does not arise from any interference with respiration, we have to inquire from what other cause it may arise. I believe it is from the sulphate of aniline being oxidized in the blood, and producing in it a blue dye. Gmelin states a fact which seems to prove this,—namely, that when aqueous sulphate of aniline is boiled with peroxide of lead, an oxidizing agent, carbolic acid, is evolved, and a blue liquid produced which smells of formic acid and afterwards becomes colourless, and gives off ammonia when heated with potass. Aniline, it has also been stated, is a derivative from indigo; and it has relations to many other dyes, the mauve dye being made from the sulphate of aniline by oxidizing it with bichromate of potass. It is, therefore, probable that the temporary blueness arises from a colouring matter, or dye, being formed from oxidation of the aniline in the blood.

Since this paper was read before the meeting of the British Association for the Advancement of Science, at Manchester, I have received many inquiries, not only in regard to the employment of the remedy in cases of chorea, some of which had been obstinate and of long standing, but also as to how far it might be adapted for the treatment of other nervous affections, more particularly epilepsy, shaking palsy, nervous irritation with jerking of the head, spasmodic asthma, catalepsy, &c. Time and opportunity of administering aremedy in suitable cases are required, in order to reply to such inquiries, and at present I am only prepared to state that I consider, from its action on the nervous sytem, that that it is worthy of trial in chronic nervous affections of spasmodic or convulsive nature. Having now, however, opportunities of knowing the results of its administration in many cases more or less allied to chorea, I may be able, at a future time, to give some information as to its utility in other nervous diseases. Epilepsy is one of those intractable diseases in which it is especially worthy of trial, and I have already alluded to two cases in which it was prescribed with decided benefit. I was consulted by letter in regard to a case of nervous irritation of the head, with frequent jerking, where the remedy was taken with great benefit; and since this communication was written, I have treated other cases of chorea with results so similar to those already given, that it is not necessary to extend this paper by alluding to them at greater length. As shaking palsey is a disease in which there is probably always more or less alteration of structure, we should not expect that sulphate of aniline would be of any service in this disease, or in any other nervous affection arising from an organic change in the substance of the brain or spinal cord.—*Lancet, Nov.* 16, 1861, *p.* 469.

16.—CASES OF CHOREA TREATED BY SULPHATE OF ANILINE.

Under the care of Dr. FRASER, at the London Hospital.

Case 1.—J. W., aged 14, admitted on January 7, 1862, with a mild attack of acute rheumatism. The alkaline treatment was adopted: and in a few days the patient had just got rid of his pains, when one morning he was suddenly seized with twitching of the left side, affecting both the arm and leg. On the following day hemichorea was fairly established. Sulphate of aniline, in doses commencing at a quarter-grain, and going up to seven grains three times a day, was prescribed; in twenty-three days 284½ grains were taken. The mauve colour of the lips became first apparent when the patient was taking three grains three times a-day. As the patient did not appear to derive any benefit from the sulphate of aniline, arsenic was ordered, under which treatment the patient became rapidly better. He is now quite steady, and though still weak, may be considered cured. This patient, whilst taking aniline, did not complain of any pain in the head; an agreeable sensation of warmth at the epigastrium being all that he noticed. It may be remarked here that the colouring of the lips, from the administration of aniline, is of a much lighter tint than the cyanotic hue caused by venous congestion. Whilst the mucous membrane in this case was of a fine mauve tint, a few drachms of blood were drawn for experimental purposes; to the naked eye the colour of the blood appeared deficient in brilliancy, and of a somewhat lighter shade than some blood drawn from a patient not taking the aniline. Under the microscope there was no alteration in the character of the globules.

Case 2.—J. H., aged 19, was admitted on January 9, with severe chorea. The disease was of four months' duration, and affected both sides of the body; the features were much distorted in articulation, and the speech was now stammering. Deafness was present, and he complained of pain in the head. Whilst walking, the patient was constantly seized with sudden clonic spasms of the lower extremities, more especially of the anterior tibial muscles, by which he was thrown forwards on his toes; and whilst sitting, the scapulæ were elevated and the spine suddenly extended by similar spasmodic contractions of other muscles. Chorea electrica would well describe the character of the disease, if that name had not been applied by Dubini to a complaint of a somewhat different nature. This patient was ordered half a grain of the sulphate of aniline with five drops of dilute sulphuric acid, three times a day. The dose was gradually increased, so that on January 30, the patient was taking seven grains three times a day: he continued taking twenty-eight grains daily for ten days, without any improvement. Under these circumstances, on February 10, the sulphate of aniline was discontinued, and the arsenical treatment adopted. His condition is now much improved, and since he was put upon the arsenical plan of treatment he has become considerably steadier, the electrical contraction of the muscles being rarer and less violent. In thirty-four days this patient took 406¾ grains of the sulphate of aniline, and during the last fortnight, whilst he was taking the drug, the deafness and buzzing in the ears were greatly aggravated; he also complained of pain in the frontal region, which reached its acme about an hour after taking a dose. The lips and mucous membrane of the mouth became tinged of a dull mauve colour, when the patient was taking two grains of the sulphate three times a-day; and the colour became deeper as the dose was increased.

[Three other cases are also related. Dr. Fraser then makes the following clinical remarks:]

These cases are not sufficient to warrant a general deduction as to treatment; the result nevertheless points to the inefficiency of the sulphate of aniline in five cases, and is, therefore, *pro tanto*, a clinical fact to be noted. The theory for the employment of aniline in cases of chorea may be founded upon this physiological fact, determined by experiments now being conducted by Dr. Letheby in the Laboratory of the London Hospital Medical

College, that upon the administration of aniline to dogs, rabbits, &c., the functions of the brain proper are arrested, while those of the spinal marrow are highly exalted; as demonstrated by the extraordinary clonic spasms at the time when the animal is entirely deprived of ordinary sensation and voluntary muscular action; for example, a dog under the influence of 3j. of aniline will remain for three or four hours comatose, while the limbs are in a state of constant motion, as if in the act of running. Hence it might be inferred that the abnormal muscular movements in chorea may, by the action of the aniline, be arrested, and give place to the normal voluntary movements; but so far as these cases go there has been a signal failure. A curious chemical question arises from the fact of the characteristic mauve hue being produced on the lips and buccal membranes. The production of the mauve colour from aniline is proved, by the published results of Dr. Letheby, to depend upon oxidation, and he has shown also (vide Chemical News, Feb. 15), by means of the oxygen from the positive pole of a galvanic battery, that aniline is changed into mauve. It would seem, therefore, that aniline, while circulating in the system, receives oxygen from some source and acquires its mauve colour.—*Med. Times and Gazette, March* 8, 1862, *p.* 239.

17.—CASES OF SEVERE NEURALGIA TREATED BY VALERIANATE OF AMMONIA.

Under the care of Dr. O'Connor, at the Royal Free Hospital.

The following cases of neuralgia, extracted from the case-book of Dr. O'Connor, are representatives of a large class of a similar character which have come under his care. They illustrate the success attending the use of the valerianate of ammonia as a remedy in the severer forms of this malady. Dr. O'Connor informs us that this drug, if retained in a state of crystallization, rapidly decomposes, and is uncertain in its action. It should be kept in solution, and the smallest dose he is in the habit of giving of the latter is equal to twenty grains of the crystal.

Case 1.—J. R., a man aged 46 years, a porter, unmarried, was admitted Nov. 9th, complaining of excruciating pain in the right side of the face, commencing near the malar bone, from thence extending to the nose, over the whole of the upper, and then to the lower jaw. He has been subject to the pain for ten years; it is generally most severe about eight o'clock at night, but he is never totally free from it. For the last week it has been most excruciating, and he has had scarcely any sleep. He has been under the care of many medical men, and sought advice at various institutions, without benefit. Dr. O'Connor ordered him a dose of compound powder of jalap to be taken directly, and a draught of three drachms of Bastick's solution of the valerianate of ammonia in infusion of calumba every three hours.

On Nov. 16th he again presented himself at the hospital, when he said that after taking two doses of the medicine the pain was considerably relieved, and that he had no occasion to have recourse to the remedy after the sixth dose. He is now well and free from pain.

Case 2.—P. B., a married woman, aged 31, admitted same day as the foregoing; has two children. For a number of years she has been subject to attacks of neuralgia, which come on generally about five o'clock of an afternoon and continue with great severity for five hours. The pain seizes her first behind the right ear, extends to the nape of the neck and back of the head, and frequently shoots with great rapidity along to the malar bone, then to the lower jaw; and she is suddenly seized with pain at the region of the heart. She is suffering from aortic valvular disease. At times the pains are so severe that she cannot bear the slightest blast of wind, and is obliged to roll herself on the floor. She was ordered three drachms of the solution of the valerianate of ammonia in infusion of calumba every three hours.

On Nov. 13th she was very much better; has had only three attacks since the 9th. The medicine to be continued.

23d. Has had no pain since the 18th.

Case 3.—R. P., a married woman, aged 42; has had eight children; works as a laundress; has suffered from neuralgia of the fifth pair on the left side for nearly twenty years, and has sought advice at many hospitals with only temporary relief. When the paroxysm is most severe the pain extends to the side of the neck and shoulder: it also causes deafness. She is obliged to go to bed, and cannot take any food, the slightest motion causing intense agony. For ten years back the paroxysms have been more severe and of longer duration. She was admitted under the care of Dr. O'Connor, on the 13th of November, whilst in severe agony; had no sleep the previous night; her health is otherwise good; and the teeth perfectly sound. She was ordered four drachms of the solution of valerianate of ammonia, in infusion of calumba, to be taken every three hours; a dose of compound powder of jalap directly.

On the 27th she states that after a few doses of the medicine the pain diminished, and on the following day it entirely disappeared.

Jan. 8th, 1862. This woman became an out-patient from an attack of influenza. She states that since the 28th of Nov. she has not had any return of pain.

Case 4.—L. C., aged 49, a married woman, admitted on the 20th of Nov. She has had nine children; has been for nearly ten years subject to severe attacks of neuralgia of the right side of the face, right eye and the tongue. These paroxysms came on generally at eight o'clock in the morning and four in the afternoon, and as soon as she puts anything into her mouth she describes the pain as insufferable. Her health is otherwise good, and her teeth are all sound. She was ordered a draught composed of four drachms of the solution of valerianate of ammonia, in infusion of valerian, to be taken every two hours, and to be repeated oftener if the pain continues in its severity.

Nov. 30th. She states that since the 27th she has had sound sleep every night, and the paroxysms of pain only come on once a-day, and then of much milder character. The medicine to be taken every six hours.

Dec. 14th. Has had no return of pain since the 1st, and now feels quite well.—*Lancet, Jan.* 18, 1862, *p.* 68.

18.—BROMIDE AND IODIDE OF POTASSIUM IN EPILEPSY.

Cases and Clinical remarks by Dr. WILKS, Guy's Hospital.

[The efficacy of the iodide of potassium in cases of epilepsy arising from syphilis is not singular. Most remarkable success sometimes follows its exhibition.]

The bromide was recommended by Sir C. Locock as a remedy having some influence over the ovary in females, and therefore curative of those epileptiform affections which might be due to the irritation of this organ. Although this theory was questionable, yet if it were founded on any facts showing the efficacy of the remedy, this was an additional reason for adopting it; and we thought equally good for all cases, whether men or women. Since using this remedy, Dr. Wilks added, now for two or three years, and in a large number of cases, he had undoubtedly had more success than heretofore, being fully aware, he said, of the remarkable fact that epileptics generally seem to be better on any new remedy, whatever its nature may be, and, also, that should it exert a real influence the benefit is often only temporary. The *rationale* of this success is connected, we imagine, with the remarks just now made, that many cases of epilepsy depend on a local affection of the bones or membranes, which the remedy removes by its absorbent powers.

Not that we can prove the existence of any such cause in most instances, but knowing the value of the medicine where such cases do exist, we have

explained its *modus operandi* by its selecting out these cases for cure. That epilepsy may exist for years, and only at death a local cause be found, we know from several instances, and in such there has been no difficulty in discovering a history of injury at an early period of life. Another explanation for an occasional cure by this remedy may be found in the fact, that fits arise sometimes from lead poisoning, and that the iodide or bromide of potassium is instrumental in eliminating the poisonous metal from the system. In two or three cases we have seen epileptiform fits arising from lead poisoning, and in which iodide of potassium was used with success. In this way it may sometimes have been supposed to have cured epilepsy.

Knowing how apt we are to be deceived by the operation of a new remedy, I do not speak more peremptorily on the subject, but add a few cases; only stating that I continue to use it in all new cases which come before me, believing it to be wise to adopt some such remedy in the first instance.

Case 1.—Cecilia D., aged 22, came to the Hospital in April, 1860. Her mother accompanied her, as she was unable to find her way and was childish in her manners. The mother, indeed, never left her night nor day, as she generally had two or three fits a-day. They had existed many years, but for two years had occurred daily. She had more than once fallen into the fire and burned herself. She was ordered five grains of bromide of potassium three times a-day. The fits soon began to grow less in number, but the improvement was not very marked until her visit on June 6, when she had no fit for three days, and in the following week, the 13th, had had none. After this they occurred at long intervals. She persisted in the medicine, with the exception of a week or two, and the report was, on October 24, "No sign of a fit for two months." At the end of another month, there having been no attack and her health improved, she ceased to attend. On February 13, 1861, she came again to the hospital; she had remained well ever since (nearly six months) until a few days before, when, having lost a brother suddenly, she was seized with four fits in succession. She commenced the same medicine again and persisted in it for six weeks, until her letter was out, when having had no return of the fits, she was dismissed. This girl thus not only lost her fits, but became more robust and in better health, a fact, Dr. Wilks said, he has noticed in similar cases, that as the disease recedes so does the health improve. In this particular case it might be thought that with such a striking result some syphilitic taint was present, but none such was discoverable.

Case 2.—Amy M., aged 13, came as out-patient with fits in March, 1860. She took bromide of potassium in three-grain doses, and the fits soon ceased. At the end of three months none having occurred, she left. This case is unsatisfactory, as a long interval sometimes intervenes between the attacks.

Case 3.—Alfred C., aged 36, a worker in lead, came to the hospital on June 13, 1860. He was a very feeble man, and had a remarkably sallow or waxy complexion, and had a blue line on the gums. He had fits and some hesitation in his speech. He took the iodide in three-grain doses, and left at the end of two months in much better health, and with a cessation of the fits.

Case 4.—Francis K., aged 34. For two years he had been subject to fits. At first they appeared at long intervals; afterwards, about once a month, subsequently about twice a week. He stated that he fell without any notice, and struggled violently. He was ordered the bromide, and when he left, in December, 1860, he had had none for ten weeks. He was seen some weeks afterwards, and he stated that he had had a slight warning twice, but never a decided paroxysm.

The following case affords a good example of the advantages of accurate diagnosis. It might, Dr. Wilks said, be regarded as a typical case of syphilitic epilepsy cured by iodide of potassium:—

Robert C., aged 36. He was a carpenter, but formerly had been a soldier in India; was invalided, owing to rheumatism or pains in the limbs. Two months before admission he had a fit whilst walking in the street; and on recovery he felt his left arm and leg numb and weak. He has had about a

dozen fits since and in some of these he has not lost his consciousness but he foamed at the mouth, and bit his tongue. Two days before admission he had a fit, followed by a great loss of power of the left arm and leg. On admission he was exceedingly ill, complained of great headache, and had partial paralysis of the left side, the arm being almost powerless, but the leg he could move a little. He soon after had three fits, in which he was convulsed all over, and screamed out. This he had done in previous attacks, and it was said "acted like a madman," although in the slighter attacks he had retained his consciousness. In the intervals he complained of pain in the right side of the head and neck, and was so weak that he could not move his head from the pillow. He was almost totally paralyzed on his left side: the weakness increased since admission; sensation perfect; no paralysis of face; eyes unaffected; pupils natural; also great difficulty of swallowing, and mouth apthous. His wife was sent for to sit up with him, as it appeared scarcely possible that he could survive long. As the patient had never been in a condition to give a good history of his case, the wife was questioned and she said that he had a fall two or three years before, also that he had long suffered from pains in the limbs, and that she had had several miscarriages and dead-born children. The patient was then again examined, and it was found that one clavicle was enlarged, as well as os femoris on one side. All these circumstances suggested syphilis, and therefore iodide, in ten-grain doses, was given. He began to improve in a most remarkable manner; only one or two more fits occurred; the paralyzed limbs began to get stronger, and complete consciousness returned. His general condition also improved, and at the expiration of three weeks he was able to leave his bed and walk about. About the end of another week he had only a slight dragging of the foot, and he left the hospital convalescent at the termination of not quite a month after the commencement of the medicine. This case afforded the most remarkable recovery we have ever witnessed from a disease of this severe character.—*Medical Times and Gazette, Dec.* 21, 1861, *and Jan.* 4, 1862, *pp.* 635, 7.

19.—EPILEPSY SUDDENLY AND REMARKABLY RELIEVED BY THE USE OF ARSENIC.

Case under the care of Dr. Bristowe, at St. Thomas's Hospital.

The patient is a shop-boy, 14 years of age, who has been subject to epileptic fits for upwards of two years. They came on without obvious cause, and at first were always nocturnal. They gradually increased, so that after a time he generally experienced two or three every night, and often one or more in the day. In May last he was admitted into the hospital, where he remained four months; and under the treatment there so far improved that during the latter part of that time, and for the following three months, the fits recurred only about once a month. A week before his last admission, however, they came on again suddenly, and with great severity. For the first three or four days of this time the fits followed one another night and day with great rapidity, but he had intervals of consciousness. For the last three days, however, the fits were still more frequent, and he continued between them in a state of insensibility. He was admitted on November 13 in the condition just described, but in so utterly prostrate a condition that his speedy death was anticipated. With the aid of a purge and of a blister to the neck he emerged from this critical state, and became quite conscious. The fits, however, though rendered less frequent, did not cease, and he was ordered to take two grains of sulphate of zinc in infusion of valerian three times a day. From this time up to December 7 the above line of treatment was pursued, the dose of sulphate of zinc being gradually increased to ten grains. During the whole of this period, however, his condition was a peculiar one. His lower extremities, which previously had been healthy, were feeble and uncertain in their movements, like those of a paraplegic patient, so that he was unable to walk; but there was no anæsthesia. He was there-

fore in bed, and for the most part in a semi-recumbent posture. But it was observed that whenever he tried to turn or otherwise moved his legs, and especially, therefore, if he were made to get out of bed, a peculiar numbness or sensation of "pins and needles" crept from the toes upwards, and was followed in a few seconds by a fit,—the fit lasting for about half a minute only, and being preceded by a prolonged and distressing scream. The truth of the statement just made was repeatedly verified by Dr. Bristowe. It was ascertained, however, that after a fit had been thus produced he enjoyed a perfect immunity for about a quarter of an hour, during which time he might move about as he pleased without any ill results; and it was further ascertained that by careful management he could, and usually did, prevent the occurrence of any fit during the day. They took place, however, frequently in the night, always preceded by a scream, and they increased in frequency nightly, so that on the night of December 6 they were so numerous that his constant screaming prevented any of the patients in the ward from getting rest.

On the next morning (the 7th) the medicine he had hitherto taken was ordered to be omitted, and five drops of liquor arsenicalis with compound infusion of gentian thrice daily were prescribed. He had no fit the following night, and none during the succeeding eight days. For two or three days he still suffered from occasional attacks of numbness in the leg; these feelings, however, quite disappeared, and he recovered completely the use of his lower extremities. On the afternoon of the 16th he was visited by his mother and several of his friends who remained with him for some time. Almost immediately after they left him, the fits returned, and between five and ten p. m. came on every ten or fifteen minutes. During the night he had two; and during each of the two following nights he also had one or two attacks. From the 18th, however, up to the present date (the 31st) he had continued entirely free, and apparently quite well.

There are a few particulars to be added to the case. The boy was, and is, pale and anæmic-looking, but his digestive functions seem naturally performed; he has no cardiac or pulmonary symptoms. He is quite intelligent, and so far as can be ascertained has no spinal or renal disease. The mode of access of the fits, the way in which they could be produced at pleasure, and the way in which their occurrence could during the day-time, be prevented, are all interesting, but do not perhaps call for special comment. The effects of the arsenic were, however, so sudden and so remarkable, that in order to show they were not fallacious, a few words seem necessary. It will be observed that the fits were nocturnal, and that they increased in severity up to the very night before the change of medicine was made. The change of medicine, further, was effected without calling his attention to the fact, or to the probability or possibility of any special effect from it; so that the sudden disappearance of the fit could scarcely have been due to any mental impression, and could scarcely have been a mere coincidence. It will be observed, too, that though the fits themselves ceased at once, the aura persisted for a time, and further that even after the latter had disappeared, the liability to have fits from any excitement continued: both of which points are quite compatible with the view that the amendment in this case was due to the use of arsenic, and indeed favour it.—*Med. Times and Gazette, Jan.* 11, 1862, *p.* 32.

20.—DIGITALIS IN DELIRIUM TREMENS.

By Dr. Robert Duchesne, London.

[The following case is interesting, as the treatment of delirium tremens by digitalis is still *sub judice*. It tends to point to those cases in which it may be expected to prove especially useful.]

W. A. H., aged 35, of small stature; in good condition of body; fair; of excitable temperament; a small draper or tallyman; of sober habits. Has for the past week taken more spirits than usual, and on December 25th in-

dulged very freely, eating almost nothing. First seen on the 28th of December; usual symptoms.

Treatment.—Opium and chloric ether; then morphia, commencing with a quarter of a grain and increased at last to one grain every four hours, with three ounces of whiskey—the *fons et origo mali*—and one pint of beer, with milk and beef-tea, up to January 3rd, the day on which I saw him. His condition was this:—Pulse 84, soft and full; secretions arrested; pupils contracted; head hot and heavy, dull pain suboccipitally; skin perspiring very freely. Slept three hours last night, three this morning, and has had sleep during the treatment. He had nausea, felt muddled, and in fact was narcotised; the delirium persists. To discontinue the morphia, and take effervescing ammonia mixture with Scheele's acid. 11 p. m. Has passed urine, but continues very restless and rambling: has taken his milk and beef-tea, with some whiskey and beer. To take at 3 a. m., ʒ ss. tinct. digitalis, aquæ ad ℥ jss; spirit lotion to head.

4th, 8 a. m. Patient would only take half the draught; no effect. To repeat the draught. 2 p. m. Slept from 9.30 a. m. to 1.30 p. m. Took some beef-tea, and is now asleep. To take ʒ ij. tinct. digitalis, aquæ ʒ vj. every six hours; milk and beef-tea; beer, no whiskey. 10 p. m. Still asleep; two doses taken.

5th, 9 a. m. Asleep. Woke up at 8, and had some toast and coffee with milk. 9 p. m. Has slept the greater part of the day; has eaten three mutton chops; no more medicine taken, but bowels have been very freely relieved both yesterday and to-day; urine normal; no delusions; is convalescent.

On the following day I put him on citrate of iron and quinine, with nux vomica, and I am about now to take my leave.

It will be remarked that this was a primary attack, occurring in a young man of tolerably regular habits, although of weak constitution and nervous temperament: that the worst day of the attack was that on which the effects of morphia had become fully developed, and that the effect of the digitalis was most marked and rapid, the pulse becoming firmer, the sweats ceasing, and most comfortable somnolency being immediately induced. It had also an equally-marked action on the intestinal canal. In all, one ounce of the tincture was taken.—*Medical Times and Gazette, Feb.* 1, 1862, *p.* 120.

21.—*Capsicum in Delirium Tremens.* By CHAS. FERNELEY, Esq., Grantham.—[Mr. Ferneley's attention was first directed to this treatment of delirium tremens by Dr. Foss, principal medical officer of the garrison at Cork. He says:]

I have used it on several occasions both in public and private practice with success, its administration having almost invariably been followed by more healthy perspiration and refreshing sleep, from which the patient has awoke quiet and relieved from the nervous excitement. The way that I have used it has been in the form of punch, first making a moderately strong infusion, two scruples of Cayenne pepper to a pint of boiling water, straining it when cool, and adding sugar and citric acid to suit the taste. It is very palatable, and will be taken by the patient *ad libitum.*—*Lancet, March* 15, 1862, *p.* 287.

DISEASES OF THE ORGANS OF CIRCULATION.

22.—ON REGURGITANT AORTIC DISEASE OF THE HEART.

By Dr. HYDE SALTER, F.R.S., Assistant Physician to Charing Cross Hospital.

[The patient, a bill-poster, a man of forty years, was not exactly to be called a temperate man. He had an attack of rheumatic fever three years ago, and was left with palpitation and embarrassed respiration. These symptoms have increased up to the present date.]

The first thing that would strike you on looking at this man would be his dyspnœa. There are great differences in dyspnœas, according to the different causes that produce them, and which suggest many interesting physiological questions, into which, however, I must not enter. Now this patient's dyspnœa is eminently *cardiac*. The peculiar characteristics of cardiac dyspnœa are these:—It is rather a breathlessness than a difficulty of breathing; it has a panting, gasping character; the sensation complained of is that of oppression, not tightness, and has a strangling, choking-throat feeling about it. The breathing is always greatly accelerated; many dyspnœas are not—laryngeal dyspnœa, for instance, which is often slower than natural; but cardiac dyspnœa always is. This man is breathing forty-six respirations in a minute.

Another peculiarity of heart dyspnœa, well illustrated in this case, is its great intolerance of movement, or any exertion whatever. Very often, in heart dyspnœa, exertion is the only circumstance under which the dyspnœa is felt. As long as the patient is at rest there may not be the slightest sense of difficulty of breathing, and the respiration may not be distinguishable from that of a person in ordinary health; but the moment he exerts himself, especially if ascending a hill or going up stairs, his breath is gone. So it is with our patient. Although his breath is always short, even when perfectly at rest, it becomes frightfully difficult if he attempts to move. He declares that merely turning his head brings on the dyspnœa; speaking is almost impossible; he cannot rise from his bed, or move two steps, without being completely *hors de combat*.

Another peculiarity of the dyspnœa of heart disease is its intolerance of the recumbent posture; and this peculiarity has given its name to one of its most characteristic forms—*orthopnœa*. The word really signifies merely "upright breathing"—compulsion to assume the erect position in order to breathe; but clinically it stands for a good deal more—for a recognized and well-marked group of respiratory phenomena. Our patient is an example of it in its severest form; he has not laid down to sleep for ten days and nights; and if he momentarily dozes in even a semi-reclining posture, he is instantly awoke by impending suffocation.

There are three forms, or rather degrees of orthopnœa: the first, in which the patient may suffer little or no dyspnœa when awake, even when exerting himself moderately, or ordinarily when sleeping in the recumbent posture. He lies down and goes to sleep as if in health; but occasionally, in the middle of the night, he is suddenly awoke with a strangling, gasping breathlessness; starts up, flings the clothes from him, and struggles for his breath as if he was dying. Very commonly the patient jumps from his bed in the effort to get more air, and sits gasping on the side of it, or leaning on a chair. It is often a quarter or a half a minute before he can "turn his breath;" and, when he does so, his respiration is hurried and panting. As the circulation through the lungs becomes freer this gradually subsides, and the breathing becomes as tranquil as ever. This may happen at long intervals, or frequently; very often it occurs regularly every night, and sometimes many times in a night. The fear which patients exhibit, after such a paroxysm, of going to sleep again, lest another should come upon them, shows how terrible is the sensation that accompanies the struggle.

Another form is that in which the patient can breathe well in any position as long as he is awake, and can sleep perfectly well sitting up, but cannot sleep lying down for a moment, the attempt to do so being immediately attended with the characteristic paroxysm. In these cases the patient is obliged to sleep in the erect posture, and in that way gets pretty good nights.

The third and most intense form is that in which sleep is altogether impossible in any position, even sitting: the moment the patient dozes, on comes the struggle; he is wide awake in an instant; sleep is hopelessly banished from him. If you stand by and watch a person in this state, which is a most painful thing, you see his drowsiness gradually overpowering him; his eyes close; his breathing seems momentarily more tranquil, and you would fancy he was just falling off into a refreshing slumber; but, a moment after, he starts

up with a look of horror, and his quiet breathing is instantly exchanged for the orthopnœal struggle.

This last is the form of orthopnœa from which our patient suffers; he has not had ten minutes' sleep, night or day, for nearly a fortnight; he is dying with sleepiness, but dares not yield to it; and, if it overcomes him, he is unconscious but a few seconds before he is broad awake in a death-struggle for breath. Such cases show with how little sleep a person can go on living. I think it is impossible, in the whole range of the incidents of disease, to imagine any suffering that can compare with this: the thirst of Tantalus could not be worse. It is not wonderful that the sufferers long for the sleep of death.

And this leads me to speak of what is the absolute nature of heart-dyspnœa.

The essential condition of tranquil respiration is a free renovation of the two fluids, air and blood, in the lungs; the essential condition of dyspnœa is an impeded renovation of one or both of these fluids. In lung-dyspnœa, the fluid whose renovation is impeded is the air—as, for example, in bronchitis, asthma, emphysema; in heart-dyspnœa, it is the blood. The immediate cause, I believe, of the sensation of want of breath, in any form of dyspnœa whatever, is congestion of the capillaries of the lungs, dependent generally on the presence of venous blood in those vessels. How this is produced by shutting off the air we understand perfectly well; the oxygenating agent is not supplied; the appropriate changes do not take place in the blood; and the capillaries, exercising upon it some obstructive force the exact nature of which we do not know, refuse to give it free transit, and it accumulates in them and in the pulmonary veins behind them, constituting the congestion of asphyxia. In the lung-congestion from disease of the left-side of the heart the sequence of events is quite different. The point of obstruction here is not in the capillaries of the lungs, but at the heart; so that the whole of the pulmonary circulation, veins as well as arteries, is affected by it. The capillaries become loaded, not from the blood being in a state unfit to pass from them, and from an opposing action to its free transit exercised by themselves, but from finding in front of them loaded pulmonary veins. Moreover, in cardiac congestion of the lungs, until ulterior changes take place, the access of air to the pulmonary capillaries is free, and the blood in them therefore arterialised. Thus, in the congestion of asphyxia, the capillaries are loaded with venous blood; in that of heart disease, with arterial. But we know that impediment in the capillary circulation of an organ as inevitably involves suspension of its function, as suspension of its function involves impediment in its capillary circulation. We know, for instance, how mechanical congestion of the kidney diminishes the quantity of the urine. And, although the blood in the congested pulmonary capillaries in heart-disease may be arterial, yet being itself unable to pass on, from the obstruction in front, it prevents the venous blood behind it reaching the seat of arterialisation, and so suspends the function of the lungs, and loads the branches of the pulmonary artery with venous blood, as much as in true asphyxia. And it is not long before the congested capillaries relieve themselves by serous transudation, giving rise to vesicular and interlobular œdema, and other ulterior changes, so that respiration becomes directly impeded. Nevertheless, there is at first that difference between cardiac and asphyxial lung-congestion which I have indicated; and the points of contrast may be tabularly represented thus:

Pulmonary Congestion of Asphyxia.	Pulmonary Congestion of Heart-Disease.
Suspended function producing capillary stasis.	Capillary stasis suspending function.
Point of obstruction the pulmonary capillaries.	Point of obstruction the heart.
Pulmonary veins emptier than usual.	Pulmonary veins loaded.
Blood in pulmonary capillaries venous.	Blood in pulmonary capillaries arterial.

Since, then, in the first stages of cardiac dyspnœa, the blood in the capillaries of the lungs is arterial, the presence of venous blood in them cannot be necessary to the sensation of want of breath: impeded transit of blood through

the lungs, and a loaded state of the pulmonary circulation, would appear to be all that is necessary.

The extreme dyspnœa from which our patient suffers suggests the question as to what form of cardiac disease the most embarrasses the breathing; for we often see patients dying of heart-disease with much less dyspnœa than we see in this poor man at the present time. This question I am not prepared to answer. I should have imagined that mitral regurgitation, constituting an obstruction that the most directly tells back upon the pulmonary circulation, would have the greatest tendency to engender dyspnœa, as it certainly has to produce pulmonary hemorrhage; yet here we have what we may call acute orthopnœa, easily induced by a valvular derangement from which the lungs are protected by the intervening healthy mitral valve. I profess myself, then, unable to answer this question; but it is one well worth thinking of, and well worth making the subject of observation.

I have detained you thus long in analysing one of this patient's symptoms—the dyspnœa—because it is, as I hope I have shown you, so full of instructiveness, physiological as well as pathological; but there is another symptom that would strike you as immediately as the dyspnœa—the *cough*. This is the true cardiac cough—a single hacking cough, not in paroxysms but iterated, keeping on; never urgent, yet not passing half a minute without occurring; feeble, with a certain throat-hoarseness about it, having rather a choking character, and with no expectoration as its result. When I say no expectoration, I should say there *was* no expectoration; but a little is now appearing.

In prosecuting our inquiry into our patient's symptoms beyond what superficially manifests itself, the first thing that would strike us would be his *pulse*. Now, this pulse is utterly peculiar; it is diagnostic of aortic regurgitation; in no other state whatever could it occur; it points to that one thing alone; and any one of you might on the strength of it, and without making any examination of the heart, safely diagnose the valvular lesion. When I first listened to this patient's chest, the only sound that I heard was at the base, with the *first* sound (systolic aortic), and I therefore pronounced the disease to be aortic obstruction; but, the moment I saw the pulse, I knew there must be aortic regurgitation, and, on listening again, plainly heard, at the same situation, a bruit with the second sound (diastolic aortic); and we have heard it ever since, much more plainly than the systolic; and how it was it escaped me at first, I am at a loss to explain. I need not tell you that the peculiar character of the pulse to which I refer is a conspicuous leaping jerk, not only perceptible to the touch, but visible to the eye; carotids, temporals, brachials, radials, tibials—in fact, all the arteries wherever superficial, and where not ordinarily visible, are plainly seen leaping forth at each beat of the heart; and the pulse may be counted in all of them, as well by the eye as by the touch. The arterial system seems alive; and, whenever the arteries are tortuous, a peculiar sudden writhe is visible, like a centipede when touched. This pulse has been called a "jerking" pulse; I think a "leaping" pulse would perhaps be a better name. It was called by Dr. Todd, a "locomotive" pulse—an injudicious name, I think; for *locomotive* is just what it is not: it is motive, but not locomotive.

The way in which it is produced by the lesion which it implies is sufficiently clear. When the aortic valves are sound, the complete and instantaneous closure of the aortic orifice maintains the tension of the arterial system which the additional volume of blood sent into it by the systole has just carried to its maximum. The systole *makes* the arterial system tense, the valves *keep* it tense, and therefore the next systole *finds* it tense; but where, from insufficiency of the aortic valves, the blood largely refluxes into the ventricle, the tension of the arteries is suddenly lost, their tendency to contract on their contents is unresisted, and they instantly collapse. The state of the arterial system at systole and diastole is, in the one case *full—kept full; full—kept full:* in the other it is, *full—empty; full—empty*. It is this alternation of a condition of fulness and emptiness, instead of the maintenance of a nearly uniform fulness, that mainly contributes to this jumping

character of the pulse. I am accustomed to compare the natural pulse to the dead beat of a chronometer, and the regurgitant pulse to the beat of the second-hand of an ordinary clock. In the one, for every second moved, the hand moves forward a second, and then holds its own: in the other, there is a back stroke of as much as half a second; so that for every beat the second-hand moves forward a second and a half, and back half a second. Which looks the most "lively" and mobile, all will know who have watched the two methods of beat: in fact, one goes over just twice as much ground as the other in the same amount of progress. The analogy of these two beats to the natural and jerking pulse is, I think, very close. Moreover, a ventricle emptying itself into a collapsed arterial system would probably do so in a more jerky and sudden way, than if driving its contents into an already tense and preoccupied aorta. Moreover, the more tense the arterial wall, the greater and the more sustained is its reaction upon the contained blood, and the more complete that conversion of the systolic pumping into the even flow characteristic of the circulation in the smaller vessels, which it is the purpose and effect of this elastic reaction to produce. The sudden loss of arterial tension on the reflux of the blood into the ventricle must almost destroy this conversion of the intermittent systole into the sustained *vis a tergo* of the more distant arterial streams; and thus we see, in these cases, the smallest arteries beating with almost as much jerk as the largest. Moreover, at each diastole, the blood in the arteries must, comparatively, come almost to a stand-still; therefore the next jerk finds a too stationary blood in front of it—a blood, therefore, more than usually antagonising the onward flow of the fresh instalment from the ventricle. Now, we know that the force of the ventricle is partly spent in driving the blood onwards; and partly, in consequence of the resistance that it experiences in this direction, that it acts laterally on the arterial walls, and distends the arteries. The greater, therefore, the resistance that it experiences in front, the more it is spent laterally.

In all these ways, aortic regurgitation conspires to produce the excessive arterial pulsation that presents so remarkable an appearance in our patient.

On examining the heart, we find, besides the sounds I have described, that it is beating with a too strong and heavy impulse, that the apex beat is too low, and that the heart altogether is greatly displaced downwards. Now, I am sure it is a mistake to regard this downward displacement as due only to hypertrophy. Three causes conspire to produce it:—one, the hypertrophy of the organ; another, the dyspnœal inflation of the lungs; and another, the flattening of the diaphragm, which always exists where there is an abiding source of dyspnœa, where there is an unsatisfied and unsatisfiable demand of the conditions of respiratory balance. This descent and flattening of the diaphragm is, in fact, a part of that chest-distension which immediately supervenes whenever there is a condition of respiratory arrears; it is the instinctive and direct effort to set things right, and restore the balance, by getting more air. If you examine the heart of a person panting from exertion, you will find it beating considerably lower than usual; the moment a paroxysm of asthma comes on down goes the heart from this descent of the diaphragm, and may be plainly felt beating in the scrobiculus; in fact, in all dyspnœas this is the case, and the dyspnœa of heart-disease is not an exception. For although in heart-disease, the disturbance being on the side of blood-supply, and not on that of air-supply, the introduction of an additional quantity of air does not better the state of things; it, nevertheless, instinctively occurs. It is, therefore, a great mistake to imagine that the amount of hypertrophy may be measured by the amount of downward displacement of the cardiac dulness. A striking confirmation of this is shown in the fact that while, in cases of undoubted hypertrophy, there is great downward displacement of the heart's dulness, there is often no upward extension of it at all; nay, that will not extend as high as usual. The fact is, that with the dyspnœal chest-distension there is dyspnœal lung-inflation, the inflated left lung overlaps the heart in an excessive degree above and to the left, and thus we get, to a certain extent, the same state of things as in emphysema. The unduly expanded left lung may also be said to thrust the heart downwards and to the right. In a temporary source

of dyspnœa, as asthma or violent exertion, this state of things is essentially temporary; and as soon as the breathlessness is over the heart returns to its normal position, and its dulness to its normal limits. But in such a case as our patient's, this condition is permanent, and we see it in him very strongly marked. The dulness involves the ensiform cartilage and the sternal extremity of the eighth, and even the ninth rib; while above the cartilage of the fifth and corresponding portion of the sternum is resonant.

We must, therefore, be on our guard, on the one hand, against imagining that the whole of the downward extension of the heart's dulness is to be explained by the hypertrophy; and, on the other hand, that because there is no upward extension of dulness, or even less than usual, there is, therefore, no hypertrophy at all; for you may have considerable downward extension of dulness, and yet there shall be no hypertrophy; you may have no upward extension, and yet hypertrophy shall exist.

And let me take this opportunity of earnestly impressing upon you the importance of bearing in mind this axiom (if I may so call it) in auscultation, that any source of dyspnœa induces a condition of hyper-distension of the chest, temporary if the source of dyspnœa is temporary, abiding if abiding. Unless you bear this in mind, you will be in constant danger of drawing erroneous conclusions as to the existence of emphysema, heart-disease, and various other conditions. I the more earnestly impress it upon your attention, because I am sure it is a fact that has been very much overlooked, and to which sufficient importance has not been assigned. I have sufficiently explained it in my work on *Asthma* and elsewhere; and, therefore, I shall not now detain you by entering into its rationale.

Let me direct your attention to this patient's *sputum*. You may remember that I told you just now that his cough was at first quite dry, but that now some expectoration is appearing. Now, why should this man spit? He has had no bronchitis; he has had no pneumonia; his respiratory mucous tract has not been affected by any of those conditions which commonly give rise to fluxes and exudations from mucous surfaces. Where does the sputum come from, and why does it come? It comes from the only place from which mucous sputum can come—from the bronchial mucous membrane; and it comes because that membrane is congested. There are evidently two conditions in which mucous surfaces pour forth an inordinate secretion; one, active inflammation; the other, passive congestion. In fact, any hyperæmia of a mucous surface, whether active or passive, relieves itself by an increase of the secretion of that surface. But there is, I think, this difference, that while the secretion from an inflamed mucous membrane is often highly purulent, that from a mucous surface merely congested, is either not at all so, or very slightly; I am inclined to think, not at all. Now if you look at this sputum you will see that it has no admixture of pus; that it consists of a grey gelatinous mucus, not opaque, and with no trace of yellow in it. It is very like the sputum of asthma, and is produced in the same way—poured out to relieve a state of congestion engendered by embarrassment of the pulmonary circulation. In pure asthma you have no inflammation of the bronchial mucous membrane, and yet it is very rare for an attack of asthma to pass off without the occurrence of this mucous exudation; never, I think, unless the attack is exceedingly transient. This is, in my opinion, an important distinction; and I believe that the presence or absence of pus will always enable us to diagnose whether the mucus is the result of an inflamed, or a merely loaded condition of the bronchial mucous membrane; at least, the presence of pus will always enable us to say that there is something more than mere congestion, although, in the very earliest stages of inflammation of the air-passages, the absence of pus will not prove that such inflammation does not exist.

And now, in conclusion, a word or two on treatment. What is to be done in a hopeless case like this, rapidly running to a fatal issue? How can we better this poor man's condition, incurable and unreachable as the organic foundation of it is? In one way, and only in one way, that I know of. His disease is essentially mechanical, and in a mechanical and material direction we must aim at its relief. The machinery of his circulation is out of gear

Now, in the circulation of the blood, there are three things concerned—the active organ, the heart; the passive organs, the sanguiferous vessels; and the fluid to be circulated, the blood. In this case, the passive organs and the fluid to be circulated are as they should be; the active organ is spoiled, and by this spoiling is no longer equal to its work. How, then, can we bring about some approximation between the work to be done and the power to do it? The heart we cannot reach; and if we could, its derangement is essentially irremediable; the passive organs we cannot influence—we cannot shorten the distance the blood must reach, nor supplement the heart's deranged machinery by any fresh force. There is only one thing left, to diminish the quantity of the fluid to be circulated. We cannot raise the machine to its work; we must lower the work to the machine. If we can do this, we may hope, *pro tanto*, to disburden the embarrassed heart, and to relieve the choked and loaded vessel.

There are two ways in which this may be effected. Blood may be directly abstracted. And there is no doubt that immense relief would immediately follow venesection in such a case. But we know from the rapidity with which some elements of the blood are restored, and the slowness with which others are, that such a quantitative reduction of the blood is speedily followed by a qualitative deterioration of that which is left. We do much better, then, if we can select what elements of the blood we will withdraw, and leave behind those which are the most valuable and of the slowest repair. Diuretics and hydragogues enable us to do this. But I have found, in these cases, direct diuretics peculiarly inoperative; I believe them to be almost worthless. When the mechanical embarrassment of the circulation is extreme, and the quantity of the urine greatly reduced (as it almost always is) by the slow renewal of the blood in the kidneys, you may give diuretics for ever and get no results. I believe the blood in the Malpighian capillaries is in a state almost of *stasis*, and having parted with its water, is not promptly succeeded by relays of fresh blood with a *plus* quantity of water, as is the case in health. To give diuretics in such a state of things is beginning at the wrong end; the renal circulation must be disembarrassed before the kidneys can filter away much water. Hydragogues, then, alone remain to us. Of these we have many; our choice is large—jalap, salines, and a long list of purgatives. But by far the most powerful and satisfactory is elaterium. And I think, if I could show you some cases that have recently come under my observation, you would be disposed to admit with me that the results attained by the administration of elaterium in cases of cardiac dropsy are among some of the most striking—I may say the most startling—triumphs of therapeutics. Floods of water are draughted away from the bowels, the dropsy vanishes, the breathing becomes unembarrassed, and the patient is restored, for a time, to a condition of very tolerable comfort. This is no exaggeration. Only about a month ago, I saw a very striking case. I was called to see a man at Camberwell, who was supposed to be, and really was, *in extremis*. I found him distended with dropsy; his legs like two Doric columns; and he had not been in bed for many nights, owing to the urgency of his dyspnœa. I heard, about a fortnight afterwards, that his improvement had been so rapid and so great under the use of elaterium, that he had returned to active business in town, and had resumed his employment as a traveller for one of the principal brewing firms in this city, and was at that time travelling in Northumberland. In fact, I am now never surprised at any amount of improvement that follows the use of elaterium in these cases. The only disadvantage that it possesses is, that the patients are often in a state of extreme exhaustion, and the action of the drug is frequently, in a high degree, depressing. To obviate this evil as much as possible, I allow sufficient time between the doses for the patient to recover himself, and at the same time, freely support with stimulants and tonics. If the elaterium is given at long intervals the dose must, of course, be proportionably larger, and I find that the patient is less exhausted by a good dose every other day, than by smaller doses more frequently repeated. My plan is to give a third of a grain, or even half a grain, about five o'clock every other morn-

ing. By ten or eleven o'clock the whole thing is over; immense watery defluxions have taken place from the patient's bowels, leaving him wonderfully lightened and relieved, but much exhausted. (I generally begin, I ought to mention, with the sixth or eighth of a grain, and increase the dose as I find my patient will bear it. It is a dangerous thing to start with a large dose of elaterium at first; partly because different individuals tolerate it so differently, and partly on account of the unequal strength of the drug.) At the same time, I give some such mixture as the following, in a wineglass of water, every three or four hours:—

℞. Liquor. cinchon. (Battley) ℥xx; spirit ammon. arom. ʒ ss; ætheris sulph. co. ʒ ss. M.

Under the influence of this, with the aid of the intervening recruiting day, patients will bear the quantity of elaterium I have mentioned for an indefinite time, and even gain strength under it, or rather, in spite of it. A fortnight of such treatment is often sufficient entirely to transform the patient's condition. One important result of this treatment is that, with the disappearance of the rest of the dropsy, the œdema of the lungs also vanishes; thus, one source of the dyspnœa is removed, and by the free admission of air into those portions of the lungs previously occupied by the serous infiltration, the asphyxial element of the pulmonary blood-stasis is cleared away, and one additional source of heart embarrassment removed. I think this is one of the most important results of this hydragogue treatment: the other results relieve the breathing indirectly, this directly. It is a most interesting thing to watch, day by day, under these circumstances, the gradual descent of the signs of the œdema at the back of the lung till not a trace of it remains.—*British Medical Journal, Feb.* 1 *and* 8, 1862, *pp.* 109, 137.

DISEASES OF THE ORGANS OF RESPIRATION.

23.—ON PNEUMONIA.

By Dr. C. J. B. Williams, F. R. S., Consulting Physician to the Hospital for Consumption and Diseases of the Chest, Brompton.

This disease I regard, not as a local inflammation, but as a disease affecting that vascular plexus which conveys all the blood of the body—as an inflammatory disorder of the lesser circulation of the body. It is not a local, but essentially a blood disease, and brought about by agencies affecting that fluid. Then there are certain poisons which, if introduced into the blood, produce pneumonia. The bite of a rattlesnake has this consequence; the injection of phosphorus into the veins; the slow absorption of arsenic through a wound. So also with certain fevers, which, besides a tendency to affect the whole frame, tend specially to excite pneumonia. Asphyxiating influences produce pneumonia. Further, any influence which affects violently the whole circulation is very likely to end in pneumonia; especially that great cause, cold, and most strongly when combined with fatigue. It is not so much ordinary cold which excites this disease: that produces catarrh and rheumatism. But if a man allow himself to be thoroughly chilled—lie out all night, and be paralysed and chilled—most probably the reaction will bring on pneumonia. Pneumonia is under the influence of the secretions. It is characterised, as we all know, by an absence of the chlorides and sulphates in the urine; and correlative with this suppression of the chlorides in the urine is the fact that in cases of pneumonia they are found in excess in the lungs.

The general view, then, which I take of pneumonia points to its classification as a blood-disease—one which affects an important part of the vascular system, and of the blood circulating in that system. In this view the suppression of the chlorides must also be kept in mind.—In the first stage of pneumonia—which we consulting physicians in this

day have little opportunity of seeing—I regard bleeding as the great remedy. I have seen nothing lately in the experience of disease which induces me to modify that opinion, or to accept the uncompromising theory of stimulation which is in favour with the school of my late and deeply lamented friend, Dr. Todd. I have seen several cases—not in the period from which I am now selecting examples, but in my earlier practice—of pneumonia in the first, inflammatory stage, where hot skin, crepitation, and strong and frequent pulse have been present, and where bloodletting has arrested all these symptoms, and in the course of a few hours vanquished the disease; and of such cases I cannot drop the recollection. So of other antiphlogistics in this stage, particularly the potassio-tartrate of antimony, a valuable remedy with or without bloodletting. My experience with respect to the manner of its use differs, however, from that of my illustrious preceptor Laennec, who gave heroic doses. I have found doses of from one-eighth to the half of a grain as useful as the larger doses of several grains, which he was accustomed to administer. Saline diuretics are also very useful; and for this purpose we may prescribe a fixed alkali in combination with nitrate of potash, chlorate of potash, or tincture of squill. This is the treatment which I think appropriate to the first stage of inflammation and congestion before the occurrence of *solid effusion*.

In proportion as solid effusion takes place we want other remedies. The pulse alters, the skin cools, and the febrile orgasm subsides. At this stage general antiphlogistic treatment fails. Local depletion by leeches or cupping is very useful, and so also is counter-irritation by blistering. Here tartarised antimony fails, and mercury begins to be useful. It is not till the cholagogue action of mercury is obtained that its full benefit is felt; but the value of this remedy, carried on to the production of this effect, has been very apparent to me, during my experience, in the stages of effusion and hepatisation. But as the circulation flags, you need other aids; or, indeed, if from the first the disease be of an asthenic or typhoid type, you need stimulants, which, when judiciously employed, assist in the resolution of the solid effusion.—*Lancet, April* 5, 1862, *p.* 346.

24.—THE CHRONIC RESULTS OF PLEURISY AND PNEUMONIA.

By Dr. C. J. B. Williams, F.R.S., &c.

The Chronic results of Pleurisy and Pneumonia are oftentimes of very serious import, and require to be carefully distinguished. The most common result is that, the consolidation not being cleared away, deposited matter is left behind, which ultimately undergoes tubercular degeneration. In other cases the result is condensation of the vesicular lung-tissue and dilatation of the bronchi. Owing to the condensation of the vesicular tissue, the air does not enter the cells with facility; but the ribs, muscles, and motory apparatus of the parietes continue to act with vigour, driving the air into the impermeable lung-tissue. The necessary mechanical consequence of this is that the air, acting strongly upon the walls of the tubes into which it is impelled, produces dilatation of the bronchi. This condition of condensation of the vesicular tissue, with dilatation of the bronchial tubes, is apt to be mistaken for phthisis. I have formerly pointed out this result of pleurisy and pneumonia, and explained the manner of its origin and its diagnosis.

It is produced, as I have said, in a purely mechanical manner by the action, on the walls of the tubes, of the air which is driven into them, but cannot penetrate the impermeable lung. The physical signs resemble in many respects those of phthisis. There is the dense, dull lung-tissue, absence of vesicular breathing, pectoriloquy, and tubular, or even cavernous breathing.

The diagnosis is difficult, but may be aided in the first place by observing the situation. The signs are not at the upper, but at the lower and central, parts of the lung, or at the side of the thorax; and the dulness is very considerable. The opposite conditions prevail in phthisis. Then, as to the

general symptoms, there may be with all this a good deal of *embonpoint*, and a ruddy aspect of health, which do not comport with so considerable a degree of phthisical disease. Also, in watching the progress of the case, you may observe that the malady does not seem to advance and affect the constitution as in tubercular disease. Such a case, in my notes, I saw first in December, 1848:—A gentleman, aged seventy-one. He had a cough of two years' standing, which had set in after a sharp attack of inflammation. It was supposed to be phthisical. However, he was a portly man, of healthy aspect. Amongst the physical signs were dulness in the lower part and up to the centre of the right lung, and signs of cavities; sometimes the dry signs, and sometimes moist gurgling. He expectorated purulent matter; but he lived on, and died last month aged between eighty-five and eighty-six, and on examination there was found the anatomical condition which I have described as giving rise to these symptoms—condensation of the lower part of the lung, with dilatation of the tubes.

But I should say that I have seen the consolidation in such cases undergo tubercular degeneration, and go on to phthisis, ending fatally. Then, too, when the deposit is considerable and unhealthy, and undergoes retraction to a great extent, it may, under a generally cachectic state of the system, end in death, commonly immediately due to dropsy.

The expectoration in these cases of condensed lung with dilated air-passages is very commonly offensive. This is owing to the subversion of the process by which nature provides, in the healthy state, so beautifully for the immediate passage upwards, and expulsion, of the secreted matters. It is due to the retention of the matter in the dilated and weakened tubes: just as in ozæna the most offensive odour is generated, not always from an actually diseased condition of the parts, but from the mere retention of a plug of muco-purulent matter, which undergoes decomposition. This fetor may be corrected by inhalations.—*Lancet, April* 12, 1862, *p.* 374.

25.—ON GANGRENOUS ABSCESS OF THE LUNG.

By C. J. B. Williams, F. R. S., &c.

Gangrenous Abscess of the Lung is one of the most serious consequences of inflammation. Contrary to Laennec, I consider it due to inflammation producing an obstruction in the circulation and nutrition of some parts of the lung. This I take to be the cause of this gangrenous destruction, and not the existence of an intense and peculiar inflammatory action. I have here the notes of the case of a gentleman who was swimming in the Thames, and got beyond his distance. He became exhausted and severely chilled, and was attacked with pneumonia. When I saw him there was a marked degree of fever and exhaustion, and fetid expectoration. This is a remarkable symptom, in respect to which there is an observation which I wish to make on a point of diagnostic value. When first expectorated, the matter expelled is abominably offensive; but when it has accumulated for a short time in the sputorium it acquires rather a pleasant odour. I have often pointed this out to students. The narrow boundary which divides perfumes from the most unpleasant effluvia is very well known, and some choice and agreeable scents are manufactured by inducing chemical change in very rank and foul-smelling substances. The peculiar and pleasant smell which this expectorated matter acquires after a short time is in no small measure diagnostic, and in itself points to the character of the disease; the odour is very nearly that of cowslips. Sometimes sloughy matter is expectorated—actual gangrenous *débris* of lung-tissue. This was the case with the above patient, who made a good recovery, however, by the end of about six months.

Another symptom worthy to be noted in the cases of gangrenous abscess of the lung is the violently convulsive cough. This cough is comparable in its violent convulsive efforts with that of whooping-cough, and I may say that the

parallel is one which may be borne out. I believe it also to be the result of a poison, and that the disease is best treated by remedies which attack the poison where its energy is located—in the trachea and air-tubes. So in the intestinal tube, an analogy may be observed if we refer to the case of dysentery. A characteristic feature is the excessive and violent straining of the intestinal canal to get rid of the poisonous matter.

The treatment of gangrenous abscess of the lung may be antiseptic, in order to counteract putrescence, and directed to allay the violence of the cough. I have frequently employed the dilute nitro-muriatic acid in doses of from forty to sixty minims several times a day, with the effect of counteracting and preventing the spread of the fetor. It is better to give the acid in quite full doses, and larger doses than the above may be given by combining it with glycerine, which has the effect of sheathing the acid, and is itself antiseptic. Fumigations may be employed with advantage, and especially of creasote, or the inhalations of creasote and chloroform. It is necessary to keep up the nutrition of the system.—*Lancet, April* 12, 1862, *p.* 373.

26.—ON PNEUMONIA.

By Dr. Edward Copeman, Norwich.

In the sthenic form, pneumonia attacks for the most part those who *seem* to be in the enjoyment of good health, but who are plethoric, full of blood, and in fact verging upon any disease of which hyperæmia would be a predisposing cause. A person in this condition is exposed to some exciting cause of disturbance to his lungs, probably either cold or over-exertion. If the chest be long exposed to the direct influence of cold, the blood will be driven from the surface and accumulate in the internal organs; if over-exertion be the cause, the lungs will be overfilled with blood by increased action of the heart. This active congestion of the lungs produces more or less of shock to the system, and impedes the ready flow of blood through the vessels of the organ affected. This impediment is increased by the tenacity of the fibrine which such blood contains, and a free circulation cannot be again established until the blood is relieved by a deposit of some of its fibrine and an increase of its watery constituents. Thus fibrine is thrown out by nature to relieve the obstructed circulation, producing consolidation and if the vis medicatrix be powerful enough to bear this temporary interruption to the vital processes, the deposit will be reabsorbed when the congestion no longer exists. Of course a good deal of agitation of the system will have accompanied these changes; and probably a natural diaphoresis or expectoration, or flow of urine, or diarrhœa (though in these cases seldom the latter), will materially assist the power with which our bodies are so mercifully endowed, that of constantly tending to restore the healthy balance when disturbed by disorder or disease. Now in such a case as this, nature will often accomplish a cure with the aid simply of diluent drinks, avoidance of heavy food, and keeping the surface at a uniform and perspiring temperature. But on the other hand, the cure would be much accelerated, rendered more safe and certain by bloodletting at the outset; or by antimonial diaphoretics, and nitric ether to excite the action of the kidneys, or both these combined. The former at once diminishes the column of blood, and, if adopted quite early, anticipates the effusion into the air-cells by rendering it unnecessary; the latter accomplishes the same purpose indirectly by means of the skin and kidneys, though not always in time to prevent a certain amount of deposit of fibrine in the lung. However, even in this case, provided the congestion is relieved, there will be no increase of effusion; and what has already taken place will either be naturally absorbed, or its absorption may be hastened and secured by the aid of mercurial action. Here again we arrive at a point in dispute at the present day, viz., the action of mercury in the treatment of inflammation. But setting aside theory for a moment, does any practitioner of experience doubt the power of mercury to remove the products of inflammation? Can

any one question the fact of its power to remove deposits of lymph and pus, who has witnessed its effects in iritis and hypopium? Here its effects are visible, definite, unmistakable. Can it be doubted that it produces the same effects in other organs? No—in these matters, experience must guide us; and I will fearlessly undertake to say that in cases of sthenic pneumonia, where the fibrinous deposit in the substance of the lungs requires to be quickly removed, the object will be obtained most effectually (if it can be obtained at all) by a discreet employment of mercury; and this, with less detriment to the constitution than would arise from the prolonged continuance of the pathological condition for which it is employed.

But how are we to deal with other kinds of pneumonic inflammation—asthenic—secondary—recurrent—lobular? For these occur usually in the course of fevers, or other diseases of debility, and do not depend upon hyperæmia, or upon a too highly fibrinated condition of the blood; they are, however, like the sthenic, dependent upon *retarded circulation* through the lungs, or, in other words, *congestion*.

In many instances, as in fevers, the blood contains poisonous ingredients which diminish nerve force, and deprive any organ of the power of resisting exciting causes of disease.

With respect to the lungs, for instance, if cold be taken, there may not be nerve force enough to resist its effects, and congestion will be the consequence; but is this a kind of inflammation to require loss of blood? The quantity of blood is not in excess, but it lacks the healthy condition by which it stimulates the heart and arteries to propel it equally through the body; and the best way of relieving these asthenic local congestions is to excite additional nerve force by internal stimulants, such as ammonia, brandy, &c.

But there are also intermediate cases in which pneumonia is neither dependent entirely upon hyperæmia, nor upon poisoned blood; but in which the constitutional power is so slight and the natural resistance to the causes of disease so imperfect, that, on exposure to exciting causes, congestions and inflammations may take place simply for want of nervous power to resist them. In these cases, the blood, although not diseased, as in fevers, is neither superabundant, nor does it contain an inordinate quantity of fibrine, so as to require either blood-letting or the cutting off of supplies; and in such cases a combination of a diaphoretic treatment to relieve the pressure upon the lungs, and stimulants to force the blood through them without delay, will, I think, be found the most efficacious. I also believe that these last cases are less likely to recover of themselves, than the first I mentioned—the sthenic—for want of that natural vital power without which no severe diseased action can be overcome. In all three of these conditions—the sthenic, the asthenic, and that consequent upon diseased blood from fevers—the blood, when retarded in its circulation through the lungs, produces the same physical signs and symptoms: transudation into the air-cells, cough, dyspnœa, pain in the chest, pyrexia, coloured expectoration, and, at last, consolidation of the lung. But, although the symptoms are very similar, in each case enabling us to diagnose pneumonia, yet, as the causes of them and the early pathological conditions vary in each, so we may reasonably expect the necessity of varying the treatment accordingly. It would be taking a very narrow view of the question to say that, because the disease is *pneumonia*, we must therefore treat it by blood-letting, or we must *not* treat by blood-letting, or it is better to let it alone, for it will get well of itself. We can have no such definite rule of practice as this in our profession; we must be guided in our treatment by well grounded general principles; and the great art we have to learn is to be able to modify those principles in detail to suit the varying exigencies of those pathological conditions included in the general name of the disease.

If we are called early to a case of pneumonia, when the attack is sudden and severe, the patient young, plethoric, and not the subject of any constitutional disease, we should best relieve the patient, and save him the ulterior changes belonging to the disease, by early and decided blood-letting.

In asthenic cases *uncomplicated by other disease*, I have often seen great relief afforded in the early stage by local blood-letting aided by diaphoretics;

taking care at the same time to give either tonics or stimulants, or both, should the *general* condition of the patient seem to require it.

In asthenic cases occurring in persons *labouring under strumous and cachectic states of system*, even local bleeding may be dangerous, and will be seldom if ever required; for, instead of being in excess and highly coagulable, the blood is poor and watery; and although it readily exudes, the consequent filling up of the air cells is less dangerous, and expectoration more easily takes place; we may safely take more time about the case, the patient will not be so early threatened with apnœa, and our surest remedies will be those which give tone to the general system, aided by counter-irritation and medicines of diaphoretic and diuretic properties. In these cases, as well as in those which occur in fevers, there is less need of mercury to effect the absorption of the effused matter; for, containing less plastic material, it is more freely expectorated during the attack, and more easily absorbed afterwards, provided we have been able not only to relieve congestion but to give increased tone and nervous power to the whole body.

Where pneumonia has arisen secondarily in the course of typhus and other fevers of a low type, I have always found it the most successful practice to trust, as far as medicines are concerned, to the appropriate treatment of the fever generally; and to limit the special treatment of the local disease to fomentations and counter-irritation by turpentine, blisters, or other stimulants. —*British Medical Journal, Feb.* 1, 1862, *p.* 113.

27.—ON PLEURISY.

By Dr. C. J. B. Williams, F.R.S., &c.

The essential distinctions between pneumonia and pleurisy I need hardly dwell upon. Pneumonia I have regarded as a blood disease; pleurisy is essentially a local disease. It is dangerous, not from its affecting the blood generally, but from the effects of the local effusion oppressing the lung and otherwise interfering with the respiration. The dangers from effusion are not much to be dreaded if that effusion be not very copious, or if, being copious, it is simple; so that we are able to regard pleurisy, under ordinary circumstances, as one of the diseases over which we have a great command, and in which we commonly secure the most complete form of success. At the outset of the disease, bleeding will sweep it away; it will arrest the incipient inflammatory action, cut down the pain, and avert the maturer stages of the attack. But after a few hours, when effusion has set in, the case is altered. This requires the most elaborate treatment, both local and general. The general treatment involves an appeal to all the emunctories, the free use of diuretics, and the administration of mercury. It is important to keep in view the effects of considerable effusion, not only in oppressing the lung and interfering with its expansion, but in making pressure on the heart and great bloodvessels. I have just come from a child, where, from the quantity of fluid effused, the heart is pushed over and displaced, and the vessels at the root of the neck are evidently seriously pressed upon. I consider that child in danger from the mere quantity of the effused fluid. The quality may be a source of danger as well as the quantity. Where the effusion is of a purulent character there are several dangers. There is the chance of purulent absorption; the depressing influence of pent-up matter on the system; the tendency of the collection to penetrate and make its way through various organs. Other evils may flow from the effusion of plastic lymph. The formation of supple adhesions may not be practically very troublesome; but where cacoplastic lymph is thrown out contraction occurs, and often gives rise to displacement, obliteration of vessels, and other lesions of serious and inconvenient character. So that here also we must make a division into complete and incomplete recoveries.

Complete recovery is the most ordinary and most frequent result.—*Lancet, April*, 12, 1862, *p.* 374.

28.—ON THE TREATMENT OF TUBERCULAR HEMOPTYSIS.

By Dr. WILLIAM BRINTON, Physician to St. Thomas's Hospital.

Of course pulmonary hemorrhage has the usual long list of specifics, each of which, in the eyes of its partizans, is so efficacious that one can hardly avoid wondering anybody should be so foolish as to die of hemoptysis at all. Alum, turpentine, mercury, the mineral acids, acetate of lead, sulphate of copper, tannic acid and a host of analogous astringents, ipecacuan, tartar emetic, the external application of ice, figure amongst the favourite remedies of this kind. The variations of diet have been scarcely less striking than those of drugs; the patient who would have been nearly starved twenty years ago to favour coagulation of the blood in the ruptured vessels, being now sometimes crammed with food, and drenched with stimulants, at frequent intervals, in order to "keep up his strength."

Warned by such discrepancies, I think we are justified in regarding as inconclusive what are often termed the "teachings of experience"—namely, the mere events which follow the adoption of this or that drug or dietary in the treatment of the malady. Here, as elsewhere in the study of therapeutics, it is our first duty to check the uncertainty or bias which must sometimes beset the conclusions of the most judicial intellect, by a study of the nature and history of the disease, and by the physiological action of the remedy. What *is* hemoptysis, on the one hand? What does the particular drug do to the healthy body, on the other? Unless we can at least partially answer the first question, it is difficult to see how we can conclude any remedy really useful. Unless you can answer the second, and point to at least an analogy between the effects of the drug in health and in disease, it is nothing short of a very complete and large account of the natural course of the malady that would enable us to accept any recovery as a cure. True it is that quinine cures most forms of ague, and mercury some forms of syphilis, by agencies which are quite inexplicable from the mere physiological effects of either drug respectively. But here our knowledge of the course of the two maladies, unmodified by their respective specifics, proves the reality of the cure. It is not so much a multiplication of coincidences, as an asseveration of cause and effect, which is thus afforded us.

Looking back to the variations of hemoptysis already alluded to, and eliminating the casual bleedings of pleurisy, and the more essential, but dilute, hemorrhages of pneumonia, you will remember that we noticed the hemoptysis of pulmonary tubercle as presenting itself in three forms of very different frequency and gravity. The afflux, or active congestion, of the first stage of tubercular deposit, and the obstruction and erosion in the second stage, affect chiefly the smaller vessels of the diseased organ. The large ulceration of the third stage occasionally lays open a blood-vessel of considerable magnitude. The first two are of frequent occurrence; indeed, the second is, in greater or less extent, almost a necessary incident of the malady. The latter is fortunately rare.

Now, of these two commoner varieties of tubercular hemoptysis, it may be unhesitatingly affirmed, that in a very large per-centage of their cases the bleeding would cease spontaneously. Indeed, inquiry amongst the poorer classes who form the out-patients of our hospitals shows that hemorrhage of this kind commonly ceases in a day or two: not only without medical advice, but often in defiance of the want of that perfect rest and unstimulating diet which, apart from drugs, constitute the best means of favouring its arrest. And I need hardly say that, in such a process, Nature is only stanching an internal bleeding by the same means which she ordinarily employs to arrest an external one; means which, though in the case of these pulmonary vessels they have to fight against additional obstacles in the shape of the alternate suction and compression of breathing, and the agitation of cardiac movement, are yet in the main sufficient,

Hence to assert that these frequent and scanty hemorrhages generally cease after the administration of any of the above drugs is to state a fact

which, the moment it is made to sustain an assertion of their specific operancy as styptics, may be challenged as an instance of the fallacy "*post hoc, ergo propter hoc;*" for the fact is utterly insufficient to sustain any such conclusion. The bleeding may have ceased spontaneously; a recovery which, supposing the instincts of the patient have been allowed full play in the matter of rest and diet, is even additionally explicable. Strictly speaking, the mere coincidence scarcely entitles us to affirm more of styptic power in the drug than if the bloodvessels concerned had been those of a cut finger.

In all cases of hemoptysis, the first indication of treatment is suggested by the pathology of these, and indeed most other, morbid hemorrhages. That the bleeding is generally preceded and accompanied by great congestion of the bloodvessels nearest the lesion is a fact about which there can be no doubt whatever. And that the removal of this condition can often be effected by smart aperients is another fact which may be as unhesitatingly asserted. Nowhere are these statements better verified than in the digestive canal; where, for example, you may sometimes see in the submucous areolar tissue that the nodule of deposit which has excited a copious hemorrhage, is surrounded by an inch or two of deep velvety congestion with a well-defined margin towards the neighbouring healthy structures; and where the virtues of drastic purgatives in melæna constitute an experience as old as the days of Hippocrates. And of all purgatives which one may give with this purpose, the old-fashioned dose of blue-pill or calomel, followed by a black draught, seems really the quickest and most efficacious. Under all ordinary circumstances its administration, and, if needful, repetition, should be a prominent feature of the treatment.

Then as to the drugs for checking hemorrhage, by their astringency or otherwise. There seems to be a kind of notion in the minds of some therapeutical writers, that such drugs as turpentine, alum, gallic acid, acetate of lead, sesquichloride of iron, &c. &c., when taken into the stomach, are received into the blood in quantities sufficient to communicate to this fluid those styptic properties which they undoubtedly possess when applied to an external wound, or to a bleeding stomach, or perhaps (through the urine) to a bleeding bladder. But I know of nothing to confirm such a notion. And I think that those who would look the facts of therapeutics sturdily in the face (which, not wishing to die a homœopath, I intend to do), would scarcely think twice about the matter. At any rate experiment would soon afford some analogical answer. Stir up ten grains of gallic acid, or ten minims of tincture of sesquichloride of iron, in ten or twenty pints of blood, and I think the resulting solution would be a styptic of a somewhat Hahnemannic degree of diluteness; not nearly so potent, one may anticipate, as the compound tincture of benzoin which, forty years ago, was the favourite application of some old-fashioned country surgeons to stanch the bleeding of a superficial wound, and which, in its turn, modern surgeons have practically superseded by a little patience and cold water.

Of other remedies for hemoptysis one may speak more respectfully, though with some qualifications. Ice, for example, seems occasionally of service. But the internal congestion, invariably produced by great external cold, suggests that, save where the source of the hemorrhage is near enough to the surface to be directly affected by it, its benefits may be more than questionable. And of ipecacuan, which many excellent authorities think highly of, in small and frequent doses, (a grain every ten or fifteen minutes,) I can only say that I have known its use followed by great exhaustion and nausea, and a very protracted convalescence, but, (what is more to the purpose,) that over and over again I have known it utterly fail in any degree to moderate such a hemorrhage.

In short, all else that I have to say of such remedies might almost be summed up in the statement, that it is in the terrible hemorrhage of larger eroded vessels, and therefore of what is often advanced phthisis, that we have the most searching test of the value of any drug of this kind. Presuming we have such a means, we must very frequently use it; for it is often impossible to tell beforehand whether a slight oozing is not the commencement

of a more considerable hemorrhage, such as (directly or indirectly) endangers life. And I believe that we have such a remedy, for the majority of patients, in *digitalis*.

The grounds for this strong personal conviction I find chiefly in the fact, that I have repeatedly seen profuse and alarming hemoptysis, which had resisted many other means of treatment, including a most judicious diet, and all the other drugs above enumerated, yield to this active remedy, given so as to produce its well-known specific effects on the pulse. And not only so, but, in several cases, the interruption of the drug seems to have permitted the return of the hemorrhage, to be again arrested by the resumption of the digitalis. Our patient in bed No. 9, of Jacob's ward, so far illustrates this point as that, thrice over, the gradual disuse of the digitalis, at intervals of two and three weeks from the previous cessation of the hemoptysis under the use of the drug, has been followed by fresh attacks. And the very same physiological effects which confirm the efficacy of digitalis may suggest the details and qualifications of its use. Pray remember that it is too much of an edged tool to be carelessly handled; and hence that, if you cannot or will not take the trouble to watch over its administration, you had best not give it at all. From thirty to ninety minims of the officinal tincture daily, in four or six doses, is as much as is generally requisite; and, (as has been pointed out by Sir Henry Holland, in that chapter of his classical work devoted to the consideration of this drug,) it is often not the robust, strong man who bears or demands the largest dose. Keep the patient quiet, and in the recumbent posture—an attitude for which the drug is a double argument in this malady. Judge, by repeated inquiry, how far, or how quickly, the digitalis is acting on the pulse; and, as a rule, reduce the dose as soon as the pulse has dropped to the slow rate, (30 to 50 or 60 per minute,) which is the usual result of its action both in health and in disease. Lastly, for those who like polypharmacy, the drug combines well enough with dilute sulphuric acid during the attack, and with sesquichloride of iron during the convalescence, without substantial detriment to its own much greater virtues.—*Lancet, Nov.* 30, 1861, *p.* 516.

29.—ON THE PATHOLOGY OF ASTHMA.

By Dr. Hyde Salter, F.R.S., Assistant Physician to Charing-Cross Hospital.

In the Dublin Quarterly Journal of Medical Science for May, 1861, there is an able article by Dr. Kidd, of Dublin, on the Pathology of Asthma, in which he advocates a theory of the disease which is so plausible, and with which so many of its phenomena might appear to be consistent, but which is in my opinion, at the same time, so erroneous, and would be so obstructive, if adopted, to the progress of our knowledge of the disease and its rational therapeutics, that I should very much regret if the arguments in support of it were considered unanswerable by the numerous readers of that journal, and should very much wish to expose, in the same candid and courteous way in which Dr. Kidd's paper is written, the untenability of the views he advocates.

Dr. Kidd's theory is, that asthma essentially consists in an irritation of the cerebro-spinal nervous centres, and that its phenomena immediately depend on a state of tonic contraction of the muscles of inspiration. This is a theory that I have mentioned and controverted in my book; but the theory there referred to differs from that of Dr. Kidd, in that he includes, among the muscles of inspiration, the bronchial muscle. Dr. Kidd's statement of his views, which, at the cost of a little length, I think it better to give in his own words, is as follows:—

"The circumstances to which I would draw particular attention are, that instead of the thorax being constricted, and the diaphragm and abdominal viscera being drawn up into the chest, causing a hollow at the epigastrium generally described as occurring during the fit of spasmodic asthma, it is found, on careful observation, that the thorax is distended to the greatest ex-

tent,—that it measures from two to three inches more in circumference than during the interval,—that the intercostal spaces are widened,—that the diaphragm is pulled down to the fullest extent, so as to produce great fulness of the abdomen, and cause the heart's impulse to be felt at the scrobiculus. The sounds, moreover, afforded by percussion over the thorax, instead of being dull are found to be clear, and it is as evident that the whole chest is full of air; and on stripping the patient, it is seen, that not only are the muscles of ordinary inspiration in a state of violent action, but that those of forced inspiration are also called into requisition, distending the thorax to the utmost."

After quoting some passages from my book in confirmation of the above description, and remarking that, in consequence of adhering to the exclusive and isolated principle that asthma depends altogether on the spasmodic action of the bronchial muscles, I do not recognise the teachings of my own clinical observations, Dr. Kidd thus proceeds:—

"In birds, who have no diaphragm, and who have the ribs connected by osseous prolongations with a sternum so large as to cover almost the entire front of the body, the natural state resembles closely that of the human subject when labouring under asthma. 'The natural state of this bony framework,' says Dr. Carpenter, 'is such, that when no pressure is made upon it the cavity it encloses is in a state of distension; and the state of emptiness can only be produced by a forcible compression of the framework, through an exertion of muscular power. In the state of distension, therefore, which is natural to the cavity of the trunk, the lungs are expanded, and fill themselves with air, which they draw in through the trachea; and this condition they retain till, by the action of the external muscles on the bony framework, the cavity of the trunk is diminished, and the air is expelled from the lungs and air-sacs, which are again filled as soon as the pressure is taken of.' Now, during the fit of asthma, the state of the thorax and the mode of respiration closely resemble that natural to birds. The chest is distended to the fullest extent, being kept so by the spasmodic action of the muscles of inspiration. When the patient desires to breathe, he must first bring a strong voluntary effort to the aid of the muscles of expiration, so as to compress the thorax, and as soon as he relaxes this, the muscles of inspiration, being in a state of tonic persistent spasm, expand the cavity again, and cause fresh air to rush in through the trachea. In the healthy state of the human subject there is always a rest after each expiration. We have first an inspiration, then an expiration following immediately, and then a post-expiratory rest, occupying a time considerably longer than that of the inspiratory and expiratory movements combined. In asthma this post-expiratory rest is lost; the moment the expiration is completed, the inspiration begins—there is no pause. In fact, as has been already remarked, it seems plain that the muscles of inspiration are, during the fit of asthma, in a constant state of tonic contraction; and that the great difficulty is to empty the chest, so as to allow of its being refilled with fresh air. For this purpose the muscles of expiration are constantly contending with those of inspiration, and require the aid of a strong voluntary effort to overcome them. In consequence of the state of distension in which the chest is during the fit, and of the necessity of overcoming the tonic spasm of the inspiratory muscles, it occurs that the relative length of inspiration and expiration is reversed; instead of the inspiration being longer than the expiration, as is normally the case, the expiration is longer than the inspiration,—four or five times as long; and at the same time the inspiratory movements are infrequent, not more than nine or ten in the minute.

"While an examination of the phenomena of the asthmatic fit proves that the entire muscles of inspiration are in a state of spasmodic action, many circumstances show that the bronchial muscles are also concerned, and are, too, in a state of spasmodic action. This is most easily demonstrated in slighter attacks, where, on applying the ear to the chest, it is found that the respiratory sounds are heard in an undulatory sort of way, marking the dilatation and contraction of the bronchial tubes, and that the points of constriction are constantly changing their site."

In the above passages two things are endeavoured to be shown: First, that my theory of asthma is erroneous; and, secondly, that the disease essentially consists in tonic spasm of the muscles of inspiration. Let me, as briefly as possible, show that the pathology of asthma I advocate is consistent with all the clinical phenomena which the reviewer cites, and then point out certain facts which are incompatible with the views he advocates.

The clinical phenomena on which he principally relies in support of his theory are:—

The chest-distension;

The locked-up condition of the air; and

The special expiratory difficulty which gives the prolonged expiration.

Now, I maintain that these three conditions are just such as spasm of the bronchial tubes would produce.

It would create the first—the chest-distension—simply by creating the demand for air. In all diseases, or conditions, in which the aëration of the blood in the lungs is in arrears, chest-distension is present; in bronchitis, croup, laryngitis, violent exertion, I have noticed it, and always in proportion to the dyspnœa. It depends on an involuntary and instinctive effort to fill the chest, and is an inevitable result of the law that in ordinary breathing associates inspiratory effort with the *besoin de respirer;* when the one is exalted so is the other. Dr. Kidd says that if the tubes were contracted, this additional quantity of air, necessary to produce the state of distension, could not be got in. I cannot admit this; we know that air *is* got in and out at each respiration; and a slight excess of inspiration over expiration would, in a few respirations, give any amount of chest-distension.

How bronchial spasm would produce the locked-up stagnant state of air in the lungs, I need not explain.

The Third condition, the special difficulty and prolongation of expiration, is produced by the bronchial spasm thus:—

It is manifest that anything narrowing the bronchial tubes must offer a bar to the ingress and egress of air equally—must equally affect inspiration and expiration; that is, supposing these acts are performed by equal forces. But these forces are *not* equal; the *in*spiratory force is a strong force—muscular action, while the expiratory is a weak one—elastic resiliency; thus the obstruction is easily overcome by the one, and with great difficulty by the other; the muscular inspiration quickly draws the air in through the narrow tubes, the passive expiration slowly and tediously drives it out again. When the same kind of force as has performed the inspiration comes to the assistance of the expiration, namely, muscular action, the air is immediately pumped out and expiration completed, as is seen in a form of respiration characteristic of the intensest asthma, where a tedious and almost imperceptible expiration is suddenly terminated by an expiratory jerk; in the first half of such an expiration the air is driven through the narrowed tubes by the ordinary expiratory force—elasticity, in the last by the extraordinary expiratory force—muscular action. The simple reason, then, why in asthma expiration is so much more prolonged than inspiration is, that unequal forces unequally overcome the same obstruction.

Let me now, in the second place, cite the facts which I believe to be perfectly inconsistent with Dr. Kidd's theory.

1. In the first place, he says that the bronchial muscles are muscles of inspiration; that at each inspiratory act the bronchial tubes are contracted and relaxed at expiration. "There are," he observes, "few problems in physiology more difficult of solution than that of the function of the bronchial muscles. The theory as to their use that seems to me to accord with the greatest number of facts is, that they are muscles of inspiration, and, if this can be proved, it will afford a satisfactory explanation of their being associated with the other muscles of inspiration in the production of a fit of asthma. The principal fact on which this theory is founded is the difference between the duration of inspiration and expiration. Fournet says that in health the inspiratory sound is five times longer than the expiratory. Now, if a volume of air occupies a certain length of time in passing into the lungs,

and passes out again in one-fifth of that time, it is evident that it must either be pressed out with greater force than it passes in, or the tubes through which it passes out must be wider than those through which it passes in. One or other of these propositions must be true:—(1) Either the force of expiration must be many times greater than inspiration; or, (2) the bronchial tubes must be narrower during inspiration than they are during expiration—*i. e.*, the bronchial muscles contracted during inspiration and the tubes narrower, and relaxed during expiration and the tubes dilated." The former of these suppositions is eliminated by showing that in order to explain the relative lengths of inspiration and expiration by the difference of the forces executing them, it would be necessary to suppose the force of expiration at least ten times greater than that of inspiration; the other alternative, therefore, remains, viz., that the tubes must be contracted during inspiration.

For the arguments against the bronchial muscles being muscles of respiration at all, either of expiration or inspiration, I must refer the reader to my work on Asthma, p. 22. But supposing they were so, as Dr. Kidd imagines, and contracted at each inspiration, relaxing at each expiration, what effect would they have? Exactly the reverse of what Dr. Kidd supposes; they would antagonise instead of assisting it; instead of being muscles of inspiration they would oppose inspiration; for it is manifest that the air cannot be drawn in with the same facility through contracted as through patulous tubes. Indeed, Dr. Kidd's explanation of the prolongation of inspiration (as contrasted with expiration), by this supposed contraction, implies an inspiratory obstruction. One would almost think (what, indeed, I have often observed in writers on the physics of respiration) that Dr. Kidd imagined that the air was driven into the air-cells, not drawn into them, and that the contraction of the air-tubes helped to force it in. But it must never be forgotten that the motive power of inspiration is entirely at the superficies of the lungs, that the lungs themselves are passive, and the parietes of the chest the only active agents. Moreover, if the law of bronchial contraction were such as Dr. Kidd supposes, and it were in operation during asthma, it would have just the reverse effect of what we see; it would lengthen inspiration: whereas it is expiration that is so lengthened, the relative proportion of the two acts being entirely reversed. Thus Dr. Kidd's own theory (unless he supposes that during asthma the time of the contraction of the bronchial tubes is exactly reversed) is inconsistent with that very prolongation of expiration which it ought to explain.

2. In the breathing of asthma we have evidence that at inspiration there is a partial vacuum formed in the chest, that the lungs are unable to expand to the degree to which the parietes are endeavouring to expand them, that the air is rarefied thereby, and that the atmospheric pressure thus not being balanced, forces in those parts of the parietes that are yielding. Only in this way can be explained the deep sucking in of the surface beneath the ribs, between the origins of the sternomastoids, and behind them, between them and the scaleni, and in the intercostal spaces, which we see at each inspiration in asthmatic dyspnœa; the drawing-in at the attachment of the diaphragm produces quite a constriction, and at the root of the neck deep pits are formed at the situations mentioned every time the sufferer tries to tug in the air. This phenomenon is quite inconsistent with the difficulty depending on tetanic rigidity of the parietes, and throws the *onus* entirely on defective air-supply.

3. The kind of contraction of the respiratory muscles in asthma is not that of tonic spasm, it is intermittent, regular, rhythmical, and co-ordinated. At inspiration the muscles start into prominence, at expiration they disappear. We see just the same thing in the breathing of laryngitis, and in a less degree, in the violent breathing after strong exertion.

4. The frequency with which asthma is due to diseases affecting the air-passages, such as measles, hooping-cough, bronchitis, &c., points to the bronchial system itself as being primarily affected, and not to the nervous centres.

Lastly, although this is the least important of all the arguments, Dr. Kidd himself admits that in some instances "the disease arises very probably from spasmodic contraction of the bronchial muscles alone, caused either by the direct irritation of the bronchial muscular fibres, or through the medium of the nerves and microscopic ganglia." Now, if in some cases, why not in all? If the phenomena of asthma are in any case explicable by simple bronchial spasm, where is the necessity for calling in another system of explanations? There is always an *à priori* objection to invoking two theories to explain the same thing.

There are two or three other points of interest, suggested by Dr. Kidd's paper, both in relation to the pathology of asthma and the physics of respiration, the consideration of which I must reserve for a future communication.

In order to reconcile the undoubted existence of spasm of the bronchial tubes in asthma with Dr. Kidd's theory that asthma consists essentially in spasm of the muscles of inspiration, it is necessary to show that the bronchial muscles are muscles of inspiration. This Dr. Kidd undertakes to do principally from the fact that inspiration is so much longer than expiration. "If," he says, "it can be proved that the bronchial muscles are muscles of inspiration, it will afford a satisfactory explanation of their being associated with the other muscles of inspiration in the production of a fit of asthma. The principal fact on which this theory is founded is the difference between the duration of inspiration and expiration. On analysing our own sensation, or applying the hand to the chest of a healthy person, it is evident that the movement of expiration is much shorter than that of inspiration." The difference is such that Dr. Kidd concludes, from the data he has obtained, either that the expiratory force must be ten times stronger than the inspiratory, or the tubes must be contracted during inspiration; the former alternative not being true, the latter (he conceives) becomes inevitable.

Now, there is a third possible explanation of the relative length of inspiration in natural breathing, which in my opinion is the true one, and which Dr. Kidd has overlooked—it is, that inspiration is an antagonized force, and expiration is not. At the time the inspiratory muscles are distending the chest they are doing so in opposition to the elastic contractility of the lungs and resiliency of the parietes, whereas when these two last named forces empty the chest, they are unopposed and achieve the act at once. The two acts of respiration find, in this respect, an exact analogy in rolling anything up hill and then down again; and to say that because expiration is so much shorter than inspiration, the expiratory force must be so much stronger than the inspiratory, is like saying that the force that rolls a stone down hill is greater than that which rolls it up, because it is rolled up slowly, and rolled down quickly; whereas we know that the force that rolls a stone up hill must be greater than that which would roll it down, (gravitation,) because it antagonises it and overcomes it. In the same way the inspiratory force must be stronger than the expiratory, because it antagonises it and overcomes it; and yet it acts more slowly. The elastic contractility of the lung is, like gravitation, a constant force acting in one direction, and anything that effects a movement in an opposite direction must do so in direct antagonism to that force, and because it is stronger than it and overcomes it; and thus the force that enlarges the chest is not the inspiration force *per se*, but the excess of the inspiratory over the expiratory force. The force of expiration (elastic contractility of lung) is as much in operation during inspiration as at any other time, indeed more, becomes increasingly powerful as inspiration proceeds, because the more the lung is expanded the more does its elastic reaction come into operation, so that the expiratory force has acquired its greatest intensity at the end of an inspiration. Inspiration is a contest of two opposing forces in which one gets the better; but its mastery is not for a moment uncontested. Expiration is the unopposed action of a single force. The arm that pulls a bow is stronger than the bow, but the recoil of the bow takes much less time than the bending it. Only in this way, by bearing in mind the co-existence of two opposing forces in inspira-

tion, the constancy of the expiratory force, and the intermittentness of the inspiratory, can, in my opinion, the physics of the inspiratory movements be correctly comprehended. But once clearly understand and admit this, and all necessity for excess of expiratory force as an explanation of rapidity of expiratory movement ceases.

The mode in which Dr. Kidd conceives the contraction of the bronchial muscles would assist inspiration is as follows:—"The object of inspiration is to carry a current of air into the air-cells. This is effected by enlarging the cavity of the thorax relatively to its contents:—1st. By dilating the cavity. 2nd. By lessening the bulk of its contents. This latter is effected by the bronchial muscles, which narrow and shorten the bronchial tubes, leaving extra space for the enlargement of the air-cells. The narrowing of the tubes assists inspiration materially in another way. Though in ordinary respiration there be but twenty cubic inches of air introduced into the lungs and expelled again, we know that in health a quantity of air remains in the lungs after ordinary expiration, varying in different subjects between 117 and 430 cubic inches. The greater part of this is lodged in the air-cells under circumstances most unfavourable for its displacement. By the narrowing of the tubes the displacing force of the current will be greatly increased, and thus important aid given to the essential part of respiration."

This increasing of displacing power, or the existence of a displacing action at all on the part of the inspired air, I must confess I cannot comprehend. The air is drawn into the air-cells from the bronchial tubes because the cells expand, and the amount of expansion of the cells entirely regulates the bronchial current. No more air can be drawn in than would fill the cells. And if the air *were* displaced from the cells, where could it go? Into the bronchial tubes by which it is at the same time arriving? That would be to suppose two co-existing currents in the bronchial tubes, and that some of the air is setting outwards at the time of inspiration. But in reality the thing is impossible; and the idea can only be based on the supposition that the air is driven, and not drawn, into the air-cells. If we imagine an artificial imitation of the conditions, by placing in an air-pump an expansible bag communicating by a tube with the outer air, we shall immediately perceive that enlargement of this bag by exhausting the pump would be attended with a simple in-rush of air, that there could be no displacement, merely an increment and no counter-current.

The other supposed inspiratory purpose of contraction of the bronchial tubes, viz. that by diminishing the volume of air in the tubes it would increase that in the cells, I admit as theoretically possible; but I cannot admit that it would countervail the great increase of obstruction to inspiration that the contraction of the tubes would involve. In proportion to the contraction of the tubes would be the increased rapidity of the inspiratory current, and Dr. Kidd has himself shown to how great a degree increased rapidity of current increases resistance.

In support of the view that asthma depends on an irritation existing in the nervous centres, Dr. Kidd quotes my observation, that in asthma there is an itching sensation at the sternum and chin, and between the shoulders. Now I must ask, What is the cause of the sense of soreness in the same situation (the sternum and interscapular region) in bronchitis? Is it subjective? Is it central? Or is it reflex, and dependent on a certain condition of the bronchial tubes? And if this last in the one case, why not in both? If in bronchitis, why not in asthma? I have fully discussed in my book the question of this sensation, and believe it to be a very interesting example of what appears to be the law of visceral sensation, viz., that when a viscus is thrown into such a condition as to give rise to a sensation, that sensation is transferred to the superficies in the middle line, at a level corresponding with the irritated viscus. In bronchitis, the tubes are inflamed, and the sensation is soreness; in asthma, they are the seat of spasm, and the sensation is itching.

That asthma may be produced, and is in some rare cases produced, by central irritation, I have not the smallest doubt. I have instanced examples in my book in which such was clearly the case; but that goes no way to show

that it is the sole and essential pathology of the disease, nor that the inspiratory muscles are involved; nor that spasm of the bronchial tubes is inadequate to the complete solution of all the phenomena.

Whenever I meet a tenable objection fatal to the bronchial-spasm theory of asthma, I will willingly and at once renounce that theory; but I must confess that I do not think that Dr. Kidd has succeeded in touching it, nor have I yet, in any direction, become acquainted with any other theory that so well accords with all the clinical phenomena, or that is not open to insuperable objections.

I would at all times much rather state my opinions in a constructive than a destructive form, I would rather be simply didactic than argumentative, and anything like scientific polemics is very distasteful to me. If I could have expressed my opinions otherwise than as antagonistic to Dr. Kidd's I would have much rather done so, but sometimes one is hardly conscious of the arguments in support of one's views till those views are assailed, nor does the necessity of even stating them enter one's head till something opposed to them is broached. Moreover, in the present paper I have been really anxious not only to enforce the bronchial-spasm theory of asthma, but to refute views both pathological and physiological, which I believe to be erroneous. *Med. Times and Gazette, Nov.* 9 *and* 16, 1861, *pp.* 476, 500.

30.—ON ASTHMA WITH EMPHYSEMA.

By Dr. C. J. B. Williams, F.R.S., &c.

The grouping of asthma with emphysema is not very systematic, but it is a combination which we constantly meet in practice; and there are reasons for classing them together. "Spasmodic asthma" I consider to be rightly so named, notwithstanding what has been said against that title in the course of that long discussion in which Laennec and Bree took an active part. Its essential cause is indeed the contraction of the muscular fibres which enter into the structure of the bronchial walls. The existence of these muscles had not always been clearly demonstrated, but it certainly is so now, and their anatomical characters are demonstrated and defined. I have made a series of definite experiments on this subject, with the effect of establishing the existence of distinct muscular contraction; not mere tonic contractility, but true muscular contraction on stimulus, and followed by relaxation; such contraction as would considerably impede the passage of air through the tubes. There are many kinds of stimuli which will excite this spasmodic contraction in susceptible persons. Most of them are well known: the smell of a stable, of new-mown hay, ipecacuanha, dust. In persons who have that liability, a paroxysm is pretty sure to be brought on by an attack of bronchitis; and this is what we very commonly indeed see and are called upon to treat. The dyspnœa of spasmodic asthma is a wheezing difficulty of drawing breath; that which arises in cases of effusion into the cavities of the chest, and of diseases of the heart or circulation, is gasping. This wheezing resembles, perhaps, that of bronchitis; and probably in most cases of bronchitis there may be something of spasm in the tubes.

There are various conditions which are coincident with the habitual recurrence of attacks of asthma, and which are connected with it. Among these are—partial consolidation of the base of the lung, enlargement of the bronchial glands, and partial tuberculization of the lung. And in many cases where there have been symptoms such as hemoptysis, and the patient seems to have escaped phthisis, he has been subject to asthma. Sometimes calculous matter has been expectorated; and the post-mortem examination of the subjects of confirmed asthma has often shown the existence of tubercular matter, calcified or otherwise, deposited at the root and apex of the lungs. Sometimes the blood may be thought to be in fault. In children it frequently accompanies an outbreak of eczema of the skin, and either the one alternates with the other, or they are simultaneously relieved. So in adults I have seen

it in combination with gout, and the one seems to be relieved by the outbreak of the other. The exciting cause therefore may be supposed to exist sometimes in the blood, as well as in the root of the lung or great vessels.

The ulterior result of prolonged or repeated attacks of spasmodic asthma may be traced in emphysema of the lung. The air which enters the cells with difficulty is also with difficulty expelled; so that the spasm of the tubes easily gives rise to a condition in which we have great resonance of the lower part of the lung, while at the upper part air enters slowly. This condition may accompany more or less each attack, but when they are repeated and prolonged, a permanent pathological change is induced. Spasmodic asthma cannot continue long without giving rise to congestion of the bronchial tubes, and this ends in hypersecretion—itself an additional source of obstruction. This takes place the more readily if, as often happens, the attack is complicated with bronchitis, which perpetuates and keeps up the condition of irritation and obstruction; and these are the cases which baffle complete cure and become chronic. We have here spasmodic contraction of the tubes, accumulation of the secretions, and inflammation with narrowing of the tubes; so that the longer and more frequent the attacks, the more certain are they to end in emphysema. The process of expiration is impeded, as well as inspiration; and it may be observed, in cases of spasmodic asthma, that, whereas at first the act of inspiration is more difficult, towards the end the expiration becomes more prolonged, difficult, and imperfect. The consequence is permanent dilatation of the air-cells in the part of the lung affected. And in the worst form of emphysema we have not only distension, but the cells are wasted and broken down, and the lung approximated to the reptilian type. This commonly indicates something more than mere mechanical dilatation and rupture; there is an alteration of nutrition. I have noted a considerable number of cases of habitual asthma in persons who exhibit the arcus senilis; and I may say that I regard the arcus senilis as a valuable sign in conjunction with other signs of the degenerative process. It is, then, fatty degeneration of the lung-tissue which aids in bringing about the atrophy and rupture of the cells which I describe.

Asthma is of course not the only cause of emphysema. I have seen persons recovering from pleurisy where the lung has been bound down in parts by retractile bands of plastic effusion. This plastic lymph causing local contraction and stenosis of the air-passages, the lung beyond it becomes partially dilated and emphysematous; the air becomes, as it were, entangled in the cell; and the result of the difficulty of egress is to produce marked emphysema.—*Lancet, April* 12, 1862, *p.* 375.

31.—ON THE ACTION OF THE BRONCHIAL MUSCLES.

By Dr. Radclyffe Hall, Physician to the Hospital for Consumption, Torquay; &c.

The perusal in the Medical Times and Gazette of Dr. Hyde Salter's interesting criticism on Dr. Kidd's view of the pathology of asthma, tempts me to say a word in defence of that hypothesis concerning the action of the bronchial muscles, which they both alike reject.

Dr. Kidd considers that the bronchial muscles are inspiratory, and that they cause contraction of the tubes during ordinary inspiration.

Dr. Hyde Salter considers them not "muscles of respiration at all, either of expiration or inspiration."

Haller, Reisseissen, Elliotson, and others, suppose that the bronchial muscles act as auxiliary agents in ordinary expiration. Dr. C. J. B. Williams appears to be of the same opinion. In the *Transactions* of the British Medical Association for 1850 (p. 363), I published a paper, in which I endeavoured by experiments to substantiate this view. And, unless I remember inaccurately his remarks in conversation, Dr. Brown-Séquard has come to the same conclusion.

There are then three conjectures at present entertained respecting the function of the bronchial muscles.

1. That they contract in ordinary inspiration.
2. That they contract in ordinary expiration.
3. That they contract only occasionally to expel irritating matters, or in answer to morbid sensations, either in the quick manner of cough, or in the persistent manner of spasm.

This last proposition, apart from its exclusiveness, few will be disposed to deny. Dr. Hyde Salter has excellently well argued that spasm of the bronchial muscles is the *sine quâ non* of true asthma; is indeed the only explanation that will fully work the problem of the disease.

When on each contraction of the heart a wave of blood distends a small artery, the elastic and muscular coats of the bloodvessel yield, and the vessel expands. This answers to inspiration; excepting that, in the case of the circulation, the motor force is from the centre; in that of the respiration, from the circumference. As regards the artery and the bronchial tube, this difference may for the moment be left out of consideration. Immediately after its distension, the artery slightly contracts, and by so doing quickens the onward flow of its contents. This answers to expiration. Galvanise the bloodvessels in a suitable manner, and the small arteries contract. Or, without that, expose the skin to cold, or the mouth to ice, and the same will happen. This tonic contraction of the bloodvessels answers to asthma.

Hence, the view I am contending for assumes that the bronchial muscles contract slightly for every expiration; contract more vigorously and suddenly for every cough; and contract in the persistent spasmodic manner in every case of asthma.

The larynx and glottis expand for inspiration, and contract for expiration.

The glottis can contract vigorously and suddenly, as in cough; or in the shape of spasm of more or less duration, in laryngismus, in hooping-cough, and in croup. Laryngismus may be considered as an infantile laryngeal asthma. I assume that the remainder of the respiratory tubing in all these respects follows suit.

The smallest bronchial tubes are the most muscular. The larger tubes have the most of these elastic-spring openers, the bronchial cartilages. Hence, the bronchial muscles will mainly operate in the tubes which lie nearest to the finer and more delicate parts of the lung, the vestibular passages and air-cells.

We will assume that the ordinary excitor to action of the bronchial muscles is the distension of the tube by inspired air. This will take place just after the end of inspiration, when the newly arrived air has undergone expansion by warmth; that is, provided the in-rush be gentle and gradual, as in quiet healthy breathing. But in proportion as the in-rush is sudden, abrupt, and forcible, as when a person is out of breath from running, will the bronchial muscles act vigorously, not to *promote* inspiration, but to *oppose* it, and so prevent it from inducing more than a certain safe amount of distension of tubes.

In expiration, the compressed air-cells expel a portion of their air with a given force. The expulsion will be quickened by lessening the bore of the exit channel, provided the expelling force remain adequate. And this quickening of exit will take place most precisely where it is most necessary to get rid of foul air speedily; viz., in the smallest bronchial tubes which are nearest to the air-cells; less and less as the tubes become larger, and intermixture of foul with fresh air readier; for it must be remembered that, at the moment when expiration commences in the periphery of the lungs, the larger tubes are still full of unused air.

The objection usually urged against the supposition that the bronchial muscles assist in the act of expiration, is, that the contraction of a bronchial tube would oppose, and not facilitate the passage of air out, just as it does the passage of air in. This would be so, provided the bore of the tube were lessened out of proportion to the motor force, and provided it were kept persistently in the contracted condition. Within suitable limits, by lessening bore, rate is quickened and volume lessened. And so long as the proper balance is preserved between the quickening effect of lessened calibre of

bronchial tube and the amount of propelling power from behind in expiration, we may assume that as much may be gained by increase of speed as is lost by diminution of channel. If the objection were valid, it would be equally strong against the case of the glottis, or that of the artery. Contraction of the glottis must impede both the ingoing and the outgoing of the air, and does so in various morbid conditions. Yet, in the normal state of things, the glottis does expand for inspiration and contract for expiration.

The same person using the spirometer finds that he cannot do full justice to his vital capacity, if he make his trial expiration too quickly, or too slowly. He is stopped by positive inability to do more of the act of expiration, and not by the sensation of apnœa. When too quick, he finishes before he has got out all the expiratory air from the lungs, which he could by a quieter attempt expel. When too slow, he tires the expiratory machinery before he has emptied the lungs of all but the necessary residual of air. I suspect that the bronchial muscles are a good deal concerned in occasioning the inadequate performance of these spirometric expirations. The latter portion of the expiration consists in an evident squeezing out of the air from the bronchial tubes. If expiration have been commenced gently, and carried on smoothly and not too slowly, the air is first expelled evenly from the air-cells, then pushed on by the contracting tubes until their muscles are tired. If the tubes be contracted too quickly before the removable air is all out of the air-cells, the bronchial muscles have done their work too soon. If the expiratory effort be made too slowly, the bronchial muscles also become tired before they have completed the full expulsion of air. This hypothesis, at least, seems to fit the facts so familiar to every one who makes habitual use of the spirometer in his practice.

I assume the following propositions :—

1. The bronchial muscles are expiratory muscles. They contract rhythmically in quiet normal expiration, and by so doing quicken expulsion of foul air from the air-cells, and accommodate the size of the tubes to the lessening bulk of the lung. In forcible expiration, the thorax governs the lungs during the major portion of the act; but at the terminal point of squeezing out the last portions of expirable air, the contraction of the bronchial muscles is the chief agent, and the lungs govern the thorax in accommodating its size to theirs.

2. The bronchial muscles are never inspiratory muscles. They oppose undue force of inspiration when it comes to bear upon the lesser tubes, and so become regulators of the amount of air admitted.

3. The bronchial muscles under abnormal circumstances can act during either inspiration or expiration. They act with irregular vehemence in cough; with clonic spasm in hooping-cough; with tonic spasm in asthma.

There is another point of physiological interest, in which in all probability the bronchial muscles play the principal part. It is a very common fallacy, that the oxygenation of the blood is governed almost entirely by the amount of oxygen present in the surrounding atmosphere. In reality it is governed exclusively by the quantity of oxygen which gains admission into the blood through the walls of the air-cells; and this depends upon several other things besides the amount of oxygen in the atmosphere. That of course is one element; but the state of the lungs, and the need of oxygen in the system, are far more influential elements. There is less oxygen in a cubic foot of air on a mountain-top than in a low bottom; and yet, in consequence of the energising effect of mountain-air, a healthy person will generally use up more. There is less oxygen on a soft day, with the barometer low and the wind in the west, than on a sharp dry day, with the barometer high and the wind in the east. Yet a person with sensitive lungs will use up more on the warm day, because he will take in more. He will fill his chest better, and can tell by his own sensations that he does so. He feels that the air goes deeper; that, to use a familiar phrase, "it gets into his lungs." On the east-wind day, he feels a sense of constriction; the air does not get into every nook and crevice, because, metaphorically speaking, his lungs do not like the air, and refuse to have it. Hence, although there is more oxygen for healthy

lungs in a cold bracing place, and in winter, there is really more for diseased and sensitive lungs in a mild climate, and in summer; simply because in the one case the lungs cannot use it to the full extent, although it is there for them; and in the other, they can use it amply, and it is there to an adequate if not quite to the same extent.

Most irritating gases irritate the glottis, and so are shut out on the threshold; but when this is not the case, they induce a sense of pulmonic constriction, which in this case, as in that of the east wind, may be largely due to reflex action of the bronchial muscles—the natural protectors of the air-cells. —*British Med. Journal, Dec.* 14, 1861, *p.* 633.

32.—ON A SIMPLE AND EFFICIENT METHOD OF PERFORMING ARTIFICIAL RESPIRATION.

By Dr. William Marcet, F.R.S., Assistant-Physician to the Westminster Hospital.

[Several years ago Dr. Marcet published an account of an instrument for performing artificial inspiration, which was, however, subject to several very important objections. The one now described is liable to none of these.]

Dr. Marcet's new instrument acts on the same principle as that invented by Dr. Sibson. It has the form of a bellows. He has adopted for its shape that of a cylinder, although any other would be equally eligible—as for example, that of a common fire-bellows. It must be capable of containing thirty or forty cubic inches of air. On the inferior plate of the bellows, in addition to the leather valve of a common fire-bellows, is a round aperture, exactly closed by a little brass cone. This cone forms part of a brass rod, which projects to a given extent in and out of the bellows. The cone is forced into the aperture, and kept *in sitú* by means of a small coil spring, which is disposed in such a way that when the operator presses on the end of the rod which is inside the bellows, the aperture is opened. On releasing the pressure, the little cone returns to its former position, and closes the hole. The method of using this instrument requires but little explanation. After the air has been driven out of the bellows into the lungs, a slight pressure on the upper plate is required to depress the conical valve, and establish thereby through the instrument a communication between the lungs and the external air, when, of course, the thorax will contract by its own elasticity and the expiration takes place. On expanding the bellows, it is filled with fresh air to be used for the next inspiration. The present instrument differs therefore from that of Dr. Sibson, inasmuch as instead of a syringe and tap, or valve which is opened with the fingers after each successive inflation of the lungs, the author has adapted a bellows and the conical valve described above, which is opened by the very motion required to blow out the air contained in the bellows, and shuts of its own accord on discontinuing the pressure.

The instrument has the advantage of great simplicity in its construction. It is of a cheap description, which will place it within reach of every medical and surgical practitioner; it is very light and portable, and may be carried about in a coat pocket; its application requires no practice or experience, as anybody can use a fire-bellows; only a little attention will be wanted to allow the expiration by pressing on the upper surface or plate of the bellows after each inspiration.

Having found a good method for inflating the lungs, Dr. Marcet endeavoured to discover a means of connecting this instrument with the larynx or trachea by some process calculated to ensure the passage of air from the bellows into the lungs, without any being lost by its escaping upwards. The necessity of overcoming this difficulty appeared to him as accounting for the slight degree of favour which instruments for artificial respiration have hitherto received. Indeed, he observed, what is the use, except as a means of experiment, of an instrument constructed so as to imitate exactly normal respiration when tied to the trachea of a dead body, if there be no means of ensuring the passage of air from this instrument into the living human lungs?

Anybody who had performed artificial respiration by instrumental process would testify to the truth of this objection. On some occasions, when attempting to restore animation in animals, he had himself been unsuccessful from the canula not being properly fixed in the trachea, or from its becoming accidentally loosened just as the first signs of returning animation appeared.

In order to solve this difficulty, it was necessary to devise a canula so constructed that when introduced through the mouth into the larynx, it pressed on its sides with a sufficient degree of force to prevent air under pressure from finding its way between the instrument and the mucous membrane of the windpipe. The following contrivance answered perfectly this condition, and was executed under the author's directions by Messrs. Whicker and Blaise, of St. James's-street:—

A tube or canula of German silver, plated, about thirteen inches in length, and five-sixteenths of an inch in diameter, is bent into a shape not unlike that of a catheter. This tube is grooved transversely at its bent extremity, and a piece of sheet caoutchouc is very tightly fastened round the margins of this groove by means of a fine silver wire. A second tube, of a very small diameter, likewise of German silver, and nearly of the same length as the other, is soldered to it, on its concave side, and opens into the minute chamber existing between the sheet caoutchouc and the metal. At its free extremity the small tube turns off from the main pipe or canula, and its orifice is ground so as to admit the nozzle of a small india-rubber bottle, similar to those used as syringes; the small tube is, moreover, supplied near its free end with a stop-cock. The main pipe has also a stop-cock near its straight end, which is constructed in such a way as to establish at will a communication between the lungs and the bellows, or between the lungs and the open air, so that as soon as natural respiration appears to be returning, air may be admitted into the lungs without disconnecting the apparatus. It is advisable to have in readiness two or three canulæ of different sizes.

The extremity of this canula is to be introduced into the larynx, and the sheet caoutchouc at once expanded by applying the india-rubber bottle to the end of the narrow tube, and compressing it with the hand. By closing the stop-cock, the sheet caoutchouc remains permanently dilated, forming a round ball, which fills up perfectly the diameter of that part of the windpipe into which it has been introduced, acting as a soft cushion, which prevents the contact of the hard metal end of the canula with the delicate lining of the larynx; at the same time, the communication between the lungs and the external air remains open through the canula. This instrument is so effectually secured in the larynx as to admit of a certain amount of traction without being displaced, which is an important element for the successful performance of artificial respiration by the method in question.

In a case of suspended animation where artificial respiration alone was capable of saving life, the first thing to be done would be to adopt some ready method until the canula and bellows the author had described could be applied. It was advisable to begin with the Ready Method in order to lose no time, and keep up the spark of life until it might be kindled by appropriate means. The canula should be introduced into the larynx as rapidly as possible. The author said he could not presume to give advice as to the best method of inserting a tube into the larynx through the mouth and glottis during life, but when experimenting on the dead body he had acted as follows:—The shoulders and neck were slightly raised, the head being allowed to fall backwards; next, the larynx was pushed backwards against the spine, the tongue drawn forwards, and the canula introduced into the mouth with its concave side downwards; the end of the instrument was pushed forwards in the median line of the tongue, being kept in contact with this organ, when it found its way through the glottis into the larynx. On inflating the sheet caoutchouc now within the larynx, the canula would be found to resist a slight effort made to withdraw it; should it, on the contrary, give way, this would show that the end of the canula was in the œsophagus, and not in the larynx.

The author believed that this method, which he had practised also on living dogs, might be available for introducing the tube into the human larynx

during life; probably, however, the *modus operandi* recommended by Dr. Sibson to introduce a tube into larynx might be preferable. This gentleman, instead of carrying the point of the instrument along the upper surface of the tongue, pushed it backwards, keeping it in contact with the posterior wall of the pharynx; by this means the epiglottis was avoided, which must by the other method interfere with the free entrance of a tube into the windpipe.

The laryngeal tube is fixed in the windpipe by expanding the sheet caoutchouc with the elastic india-rubber bag, and closing the stop-cock on the narrow pipe. The bellows should be at once connected with the canula by means of a piece of caoutchouc tube, about eighteen inches long. Artificial respiration is now commenced by distending the bellows more or less, according to the amount of air required, and then compressing it, when the air it contains is introduced into the lungs. As soon as an obstacle is felt to the further compression of the bellows, the inspiration is at an end, and, by making a slight effort to overcome this obstacle, the lungs will be allowed to empty themselves, much in the same way as they would during natural inspiration. The expiration may be assisted by external pressure on the chest. This operation can be continued for any length of time.

In order to test the efficiency of the present method of performing artificial respiration, Dr. Marcet gave an overdose of chloroform to a dog, and when respiration had quite ceased, he succeeded perfectly in restoring animation with the instrument described above; indeed, animation was suspended twice in the course of a few minutes, and restored twice without the slightest difficulty. The subject of this experiment was none the worse for the operation.

Finally, the author remarked, that the laryngeal tube is equally applicable whatever be the instrument used for inflating the lungs; and, moreover, that it will prove of great utility for all experimental inquiries connected with suspended animation where artificial respiration is resorted to. It will save putting animals to a great deal of pain by doing away with the necessity of performing tracheotomy.*—*Lancet, Feb.* 22, 1862, *p.* 201.

33.—ON HÆMOPTYSIS.

By Dr. C. J. B. Williams, F.R.S., &c.

[It is a singular fact that where tubercular disease is situated near the root of the lung (the dulness existing in the interscapular space) the hemorrhage is generally considerable. Hemorrhage occurs much more frequently in the early stages of consumption than when the disease is further advanced.]

The influence of styptics avails to stanch the blood, and should be means calculated to moderate the circulation. After much observation, I favour the employment of the simple and known styptics, frequently exhibited, in large doses. I think well especially of the use of gallic acid in full doses. I know there is a doubt in the minds of some as to the efficacy of gallic acid as a styptic; but I must say that I have none. I give it in doses of ten grains, and its efficacy is increased by combining it with a derivative. I combine it frequently with cream of tartar, which my namesake, Dr. Robert Williams, has shown to be itself possessed of hæmostatic properties. Sometimes it is useful to combine it with opium or digitalis. If there be much excitement, then a useful adjuvant is found in tartarized antimony. Gallic acid failing, I believe that great service may be derived from the use of the acetate of lead, but not in small doses. I never trust to less than three grains to a dose each hour or half-hour, with a little laudanum and acetic acid. There is no harm or risk of lead-poisoning if once a day the alimentary canal be swept clear with the sulphate of magnesia. This prevents the colic. In this way the styptic effect of lead is produced better than by administering the smaller doses, and at the same time any ill effects may be thus prevented. If called to a case of hemoptysis, I would not put ice on the chest. I am

* The price of the bellows and canula is about £2.

obliged to speak thus curtly, because our time presses. No doubt placing ice on the chest will control the bleeding, but it does this at a great cost. It substitutes for the hemorrhagic condition a worse evil. The violence of the reaction which follows is very injurious. Some of the worst cases of galloping consumption—that is, of phthisis accompanied with rapid inflammatory change—which I have ever seen, have been cases in which the most rapid symptoms of destruction have followed the arrest of hemorrhage by the local application of ice to the chest.—*Lancet, April* 19, *p.* 399.

DISEASES OF THE ORGANS OF DIGESTION.

34.—ON APHTHOUS INFLAMMATION, AND ON ULCER OF THE TONGUE.

By Thos. Bryant, Esq., Assistant Surgeon to Guy's Hospital.

Aphthous inflammation of the tongue.—The ordinary aphthous inflammation of the tongue is by no means uncommon; it may be found in children or in adults, and appears to be caused by any irritation in the alimentary canal; it is easily subdued by means of the chlorate of potash, given internally in five-grain doses, and a lotion of the same salt, of about one to two drachms to the pint of water.

Simple ulcer.—Occasionally the tongue is the seat of a simple ulcer, and this is perhaps the form which is described by authors as the dyspeptic ulcer; it is certainly, as a rule, associated with some disturbance of the digestive apparatus and, as a consequence, may be correctly designated by such a term. I have the notes of an obstinate case which came before me last year, which is worth quoting.

Case.—It was in a delicate-looking, but otherwise healthy, banker's clerk, æt. 20, who had been the subject of a superficial ulceration of the tongue for *thirteen months*. He had never had syphilis, and his living had been good and steady. He had sought all kinds of advice, had taken mercury, and the ulcers had been irritated by the nitrate of silver and other applications, but all without benefit. When I saw him the tongue was extensively ulcerated over the anterior portion and its under side; it was superficial and indolent in its character. I gave him the chlorate of potash as a medicine and as a lotion, and ordered him to live on the simplest diet; under this simple treatment he recovered perfectly in three weeks.

About six months subsequently he had a slight return, but it was subdued as rapidly by the same treatment.

Remarks.—This case is given to show how a simple disease may be mistaken for a more serious one; and how, by such an error, the patient may be kept in misery and subjected to every form of treatment that can be applied. Having recognised the fact of its simple nature, simple treatment will suffice, and the time usually required for its cure is not great.—*Guy's Hospital Reports, Vol.* vii. *p.* 12.

35.—ON TUMOURS AND DEEP-SEATED ULCERATIONS ON THE TONGUE.

By Thos. Bryant, Esq., Assistant Surgeon to Guy's Hospital.

[It is necessary to class tumours and ulcerations of the tongue together, inasmuch as the ulcerations are the later stages of the tumours. The tumours and deep-seated ulcers of the tongue are all either syphilitic or cancerous.]

That *syphilitic tumours* are formed in the tongue, as they may be in other parts, is a point which can hardly now admit of a doubt. They appear to be composed of little else than ill-organized inflammatory products, this material having been effused into or between the muscles of the part. Sometimes the tumour thus formed will be of a diffused character, at others it will be more

localized and will give the surgeon the idea of a distinct growth; both forms, however, have the same pathology, and both may be completely removed by appropriate treatment.

Like the inflammatory syphilitic exudations in other parts, and more particularly like those which are so frequent in the cellular tissue, degeneration is a common termination; an abscess, as a result, takes place, leaving an excavated and ragged ulcer.

A *cancerous tumour* may pass through all the same stages; a local or infiltrating tumour may first appear, and grow to any size with variable degrees of rapidity; at some uncertain period its degeneration will certainly ensue, and softening, with the formation of an abscess, will be the result. Having discharged its contents, an ulcer will be left, which is generally described as being cancerous; its deeply excavated and irregular surface, indurated, ragged, and everted edges, are points which, to the practised eye, at once suggest its true nature.

It is hardly necessary to dwell on the importance of making a correct diagnosis in these two forms of disease, for the syphilitic is amenable to medical treatment, whilst the cancerous can only be relieved by some surgical means.

I have notes of several interesting examples of syphilitic tumours, the following abstract of which may be read with interest.

Case.—A married woman, æt. 37, applied to me in September, 1857, with a large, ill-defined tumour developed in the extensor muscles of the right thigh; it had been growing six months, but somewhat slowly. It caused little or no pain. She had had an ulcerated tongue some months previously, but from this she had recovered. Her husband had also been under my care with a specific eruption, which had been cured by iodine.

The iodide of potassium and iron were given to the patient, and in six weeks the tumour had entirely disappeared.

Case.—A married woman, æt. 36, applied to me for advice with a large tumour developed in the muscles of the left scapula, beneath the spine; it was diffused, and completely occupied the whole of the surface of the bone. It was firm to the touch, and at times painful; it had been growing about nine months. No history of syphilis could at first be obtained, but it appeared that one year previously she had suckled a neighbour's child, and had suffered from sore nipples of an obstinate character, an eruption of the skin, which was cured by treatment, having also subsequently appeared.

Under the bichloride of mercury and tonics the whole of this tumour disappeared in three months, and convalescence was established.

The cases of cancer of the tongue are more numerous. I have 18 examples, 13 in males and 5 in females. In the majority of instances the disease had existed but a few months:

1	patient was	24	years of age.
3	were between	30	and 40
4	,,	40	50
6	,,	50	60
3	,,	60	70
1	aged	77.	

Treatment.—In the largest proportion of cases nothing but palliative treatment could be applied; the disease was too extensive and diffused to allow of any surgical operation, and consequently relief was given by means of lotions to the part to keep the wound clean. The biborate of soda and the sodæ chlorinatæ are the best forms, combined or not with some preparation of opium. Tonics should be administered to improve the health, and good living advocated. In four cases the operation of excision was performed, and in two of these a subsequent return rapidly took place. In these examples the disease was quite local, existing only as a distinct tubercle, and excision was carried out with difficulty; in one case some copious hemorrhage followed, which was, however, speedily arrested by the actual cautery. In two or three other examples of the disease, relief was for a time obtained by the application of caustics or the hot iron; but such remedies are, as a rule, by no means satisfactory; they may do good, but it is for a very short period,

and they should be employed only when the local disease is very painful; in these cases the destruction of a portion of the growth by such means appears to be beneficial.—*Guy's Hospital Reports, Vol.* vii., *p.* 15.

36.—ON DIPHTHERIA.

By Dr. Edward Latham Ormerod, Physician to the Sussex County Hospital.

Diphtheria may display itself in the throat in three different forms, each of which requires a special consideration. First, there is the typical form, from which the disease takes its name, characterized by exudation on the surface; or secondly, the soft palate and adjacent parts may be inflamed and ulcerated, with or without exudation; or thirdly, and more rarely, we may have inflammation ending in suppuration of the deeper tissues of the soft palate.

1. The first or typical form of diphtheria is characterized by the appearance of membranous patches of a pearly whiteness on the soft palate. These occur sometimes as minute spots, which coalesce as they spread, but more commonly begin as broad, white, milky films, which look just as if a blister had been raised on the soft palate, or the surface had been touched with caustic. They chiefly affect the velum palati, running down to the tonsils, but are also found on the uvula. Altogether the local changes in this form are so slight on their first appearance, so different in this respect from those which characterize the form of the disease next to be described, that it is difficult to realize in the first instance the terrible importance of these white films.

After a little while, generally two or three days, this appearance changes; the exudation becomes opaque, and is stained yellow or black, and as it grows thicker and stiffer, it parts at the edges from the subjacent mucous membrane. If it is pulled off at this period, the surface is seen raw and bleeding beneath, but there is no loss of substance.

I have seen cases where a firm leathery coat of this description invested the soft palate so completely as to seem like a membrane stretched across the aperture, and almost entirely obstructed the passage. Under such circumstances the partial removal of this membrane is required, and life may be prolonged by this means. Dr. Hall tells me that in one case he snipped off the entire uvula with the most decided temporary relief to the sufferings of a child, who before could scarcely breathe or swallow, so great was the mechanical obstruction. But, as a rule, nothing is gained by detaching this deposit, for it will speedily be reproduced, and the operation causes much distress, if not actual pain, to the patient; and in estimating the probable benefit from such interference, we must remember that, besides the exudation which we see, and can reach, there is probably much that we can neither see nor reach, and must perforce leave alone: though, on the whole, I believe that this typical exudation is more marked on the parts of the soft palate which are in sight than anywhere else, the plastic exudations on the interior of the larynx and bronchi only excepted.

In fatal cases this exudation becomes thicker, larger, and darker, stained by blood and food, and perhaps by local applications, and dried by the current of the breath, which cannot pass now through the obstructed nostrils. The upper edge becomes ragged, and is confused with the similar deposits which appear in the roof of the mouth; while below, the swollen uvula and tonsils are often with difficulty recognisable. At this period the exact distinctions between this and the ulcerative form of disease are to a great extent effaced. In favourable cases of this nature, the patches become less and less as the swelling of the parts subsides, and seem gradually to be worn or crumbled off rather than to melt away.

2. The form of diphtheria just described is not to my experience so common as the ulcerative or sloughing form, where there is actual loss of substance of the parts involved; where there may or may not be exudation; where

almost the only circumstance distinguishing it from the scarlatinal sore-throat is the implication of the uvula in the general swelling; and where from the first, the local disease assumes a formidable appearance.

It is unnecessary to describe here at length the familiar characters of a sore-throat of this kind: the grey sloughy spots, where the folds of the soft palate lie in contact, which gradually extend into broad patches along the fissures between the uvula and tonsils; the enormous watery swelling which shuts out all but the front of the velum palati from our view; and the horrible mass of slough and exudation, which in three or four days almost fills up the back of the mouth. All this is unhappily a matter of familiar observation, and much of this may be seen in the form of diphtheria already described; much, too, in the most malignant forms of scarlatina.

Though I believe that little advantage is gained by the frequent repetition of any local application to this diseased mass, yet it is of consequence that the throat be examined at least once a day, if this can be done without distressing or irritating the patient. For, besides the information as to the progress of the throat disease which is to be gained from this inspection, we may learn much as to the state of the system generally from the appearance of the mucous membrane of the soft palate in the neighbourhood of the sloughs or ulcers. When the mucous membrane looks pale and ashy, and displays no signs of any active effort at repair, even though there be no extension of the local disease, we must augur ill of the result. Any indications of power, on the contrary, such as the separation of the sloughs or deposits, a bright-red colour of the mucous membrane, even sometimes a little bleeding from an ulcerated surface, justify a more favourable opinion.

Are sore-throats of this kind properly to be classed as diphtheria? I cannot myself doubt it. There is not a single circumstance in the pathology of these cases from beginning to end, except the different appearance of the throat at an early stage, which seems to me to justify a contrary opinion. I have not found them more closely connected with scarlatina than the typical form of diphtheria seems to be, and the whole history of the two forms runs parallel both as to the primary disease and the remote consequences; and if I could doubt, the occasional appearance of a regular diphtheritic exudation in this form of disease might seem to settle the question that the two forms are identical. It is in this form that the disease has chiefly shown itself here, and it is in this form that the severe disease seems to shade off into the slighter cases already alluded to.

3. The third form in which diphtheria displays itself in the throat, which I have met with but rarely, and only in adults, differs little from common quinsy. The most striking distinctions are the greater tendency to affect both sides of the throat simultaneously, and the greater amount of ulceration which precedes and follows the discharge of the matter from the tonsils; perhaps, also, there is less pain and more prostration than in common quinsy.

Dissimilar as this form of throat affection is from typical diphtheria, yet, as in the form just described, and on the same grounds, I believe that the disease is essentially the same in both. Singularly enough, the throat affection was of this form in some of the cases where the secondary effects of diphtheria were most characteristically displayed. In one case, indeed, which was under observation in the hospital throughout its whole course, the man was supposed for some days, judging merely from the appearance of the throat, to have common quinsy; yet he had subsequent paraplegia and paralysis of the soft palate, just as after typical diphtheria.

I have met with this form of the disease rarely, and only in adults. It may be owing to this circumstance that the results in this have been so much more favourable than in the other two forms.

Lest from the large space which has been allotted to the description of the distinctions between these forms I should seem to attach undue importance to them, I must recapitulate a little. The types of these classes are not permanent and invariable throughout the whole course of the disease; on the contrary, the case which belongs to one class at one time may assume the characters of another class at a later period. Thus, where a throat has been

affected with ulceration or sloughing, we may, at a later period, find a genuine diphtheritic patch, or destruction of tissue may proceed beneath the diphtheritic crust. So that we cannot assert that there is any specific difference between these forms; and objectionable, on some accounts, as the term diphtheria may seem, yet we must own that a connecting link between all the three forms is found in the tendency to this kind of exudation which exists in them all alike. We must own, too, that the term malignant or putrid sore-throat, is not quite as comprehensive of the whole disease as might be thought from a limited experience: instance the occasional occurrence of nasal diphtheria.—*Lancet, December* 7, 1861, *p.* 543.

37.—ON THE TREATMENT OF DIPHTHERIA.

By Dr. JOHN WEST WALKER, Spilsby.

[These are the three following salient characteristics of a case of diphtheria: 1. A more or less acute unhealthy form of inflammation affecting the faucial region. 2. The presence of the peculiar leathery exudation. 3. The marked tendency to death by exhaustion.]

For each of these several characteristics, our materia medica possesses a remedy of almost specific virtue.

I have long looked upon guaiacum as a specific in common sore-throat; for, no matter in what place, what the age, sex, or condition of the patient, or what the stage of the complaint, I have always administered it, and had reason to be satisfied with the result; indeed, I consider the effect of this remedy in such cases to furnish an example of cure as distinguished from treatment. (*Vide* Dr. Latham's Lectures.) Frequently have I seen cases of cynanche and diphtheria existing at the same time and in the same family; and I was first induced to give guaiacum in the more severe affection by observing its good effect in cases which, at the onset, were supposed to be examples of common sore-throat. I now unhesitatingly consider guaiacum as the remedy *par excellence* for diphtheria. The presence of the peculiar exudation almost instinctively suggests the employment of the chlorate of potash. For the debility, &c., our sheet-anchors are ammonia and bark.

My prescription for all cases of diphtheria is the following:—

℞. Potassæ chloratis ℈iv; tincturæ cinchonæ comp. ℥ss; tincturæ guaiaci comp. ℥ss ad ʒvj; mellis q. s.; aquæ ad ℥viij. M.

Of this mixture from one tea to two tablespoonfuls may be given, according to age; but the interval between each dose, from one to four hours or three times a day, must be regulated according to the severity of the case.

These, then, I consider the therapeutic essentials in the treatment of diphtheria; but, in a complaint so terrible, it behoves us to resort to every possible adjuvant, however slightly it may contribute to the general good; and under this heading I am disposed to place all topical applications. My plan is to apply (by no means frequently) a concentrated solution of nitrate of silver, believing it to hasten the removal of the exudation, and to induce a more healthy action over the denuded surface. Of gargles, I consider weak vinegar and water, or what is more elegant, dilute acetic acid with infusion of roses, to be as good as any, producing slightly stimulant and astringent effects on the parts, and washing away offensive detached particles. Externally, warmth applied after the manner of poultices, of which a bag of chamomile-flowers is as convenient as any, together with mild terebinthinate embrocations applied two or three times a day, are to be recommended. Like most of my fellow-practitioners, I avoid mercurials; and should the bowels require moving, castor-oil or the milder laxatives suffice.

The *diet* should be nutritious, with an increasing allowance of wine from the first. Lastly, we should ever be on the alert to remove as far as practicable the primary cause of the complaint by attention to every sanitary particular.

In advocating the guaiacum treatment of diphtheria, I must disclaim all credit for originality. Others have recommended it to the notice of the pro-

fession; and more, I doubt not, are daily in the habit of carrying it out in practice. What I desire is, that many of my fellow-practitioners may be induced to try a remedy which, from what I have lately read on the subject, is not so extensively employed as it deserves to be.—*British Med. Journal, Dec.* 21, 1861, *p.* 655.

38.—SULPHURIC ACID MIXTURE IN DIARRHŒA.

By Dr. Andrew Clark, London Hospital.

There is at least one advantage in dealing with large numbers of out-patients at a public hospital. You can classify cases of particular diseases into sets, study the comparative values of different remedies, and acquire possession of broad therapeutic facts. It is objected that therapeutical experiments upon out-patients are worthless on account of the unfavourable hygienic conditions under which they are placed. But this, it seems to me, is the very reason that they are eminently *worth-full*. When you experiment upon in-patients, it is often doubtful whether the cures you get are due to the improved hygienic conditions of the patient, to nature, or to your particular remedy. But the cures you get among out-patients, in spite of unfavourable hygienic conditions, and in the very circumstances under which the diseases are contracted, may more justly be considered criteria of the values of your plans of treatment.

I have lately had the opportunity of renewing our therapeutical experiments upon diarrhœa; and with the same results as before. Sulphuric acid has been tried in contrast with chalk, opium, nitrate of silver, copper, bismuth, lead, pernitrate of iron, and others, singly and in combination; and it has come out of the trial demonstrably as a more rapidly efficient agent than any of them in the cure of diarrhœa; but it is much less effective alone than in combination. Given in the manner prescribed below, it has succeeded in curing 74 per cent. of cases of diarrhœa within forty-eight hours.

When the stools are scanty and frequent the administration of the acid is preceded by a dose of castor-oil. When they are abundant and watery the acid is begun at once:—

℞. Acid. sulph. aromat. ℥xx.; tinct. camph. comp., ʒj.; ætheris chlorici, ℥x.; spirit. menthæ pip. ʒss.; syrupi rhœados, ʒj.; decoct. hæmatox. ad ℥j.*

To be given every four, five, or six hours, according to the urgency of the diarrhœa. The object of the aromatics in the formula is the correction of the fæcal fermentation which is one of the chief conditions in the production of diarrhœa. In some cases the second or third dose succeeds in checking the disease, and then the patient is troubled for a day or two with pinching pains in various parts of the bowels. These are best relieved by a small dose of castor-oil with an aromatic. The acid is rarely successful in advanced phthisical or dysenteric diarrhœa, but it is so in the early stages of these complaints.—*Med. Times and Gazette, Jan.* 4, 1862, *p.* 10.

39.—ON COLORATION OF THE FÆCES.

By Dr. Thomas Inman, Liverpool.

We do not advance truth by opposing flippancies to facts or sneers to scientific inquiry; we do advance it by a patient investigation and collation of observations. False theories have been, and still are, the bane of medicine; and no one hugs them more closely than those styled "practical" men. Your correspondent is no exception to this; for he attributes to two false theories all the force of absolute facts.

The subject to which I have again called attention cannot, however, be so easily disposed of as Mr. Pope would imply; and I shall be glad if you will

* Or, if the mixture is to be kept, some aromatic water.

allow me to point out some of the difficulties which surround it. I will enumerate them as propositions which I believe to be certain.

1. In health, and generally in disease, the brown colour of the fæces begins *after* they have entered the colon, and the nearer they are to the duodenum the lighter is the colour. If the colours were due to the bile alone, and this fluid being only partially excrementitious, the reverse would be the case; *i.e.*, they would be brown in the duodenum and light in the rectum. 2. The bile is yellow during life; the motions are brown, becoming darker by exposure. 3. In obstruction of the biliary ducts, the intestinal secretions remain of the same colour throughout the bowels. 4. In cirrhosis, on the contrary, when the bile is thin and watery, we frequently see the stools of the normal colour. 5. In cases of poisoning by calomel, there is no change noted in the contents of the gall-bladder, or of the small intestines; and autopsies show that mercury does not make the bile brown, but the colon is occasionally inflamed. (*Taylor on Poisons*, p. 417.) 6. In dysentery—a disease affecting principally the colon—we usually miss the ordinary feculent stools; nor are they temporarily restored by mercurials; they do not reappear until a very large portion of the colonic mucous membrane is healthy. This observation is true, even when the liver is demonstrated to be in good order. (*Morehead*, p. 251, 296.) 7. When infants have diarrhœa, the stools are frequently watery and green; after taking too much mercury they are sticky and green, there being *no* corresponding change noted in the *bile or liver*. (Pereira, *Materia Medica*, p. 852; West on *Diseases of Children*, p. 390, 392-3; Petrie as quoted by myself; *Foundation*, &c., p. 251.) 8. The fæces of the fœtus *in utero* are black; of the infant fed on milk, yellow; of the adult fed on milk or veal, white; of the infant fed as usual, brown. The young pig, puppy, foal, and calf, pass stools of different colour, as do their adult parents, and they are unaffected by calomel. (Pereira, vol. i. p. 851). *Yet in all the bile is essentially similar.* (Kirkes's *Physiology*, 2nd edit., p. 241.) 9. In man the colour of the motions depends very much upon the ingesta, and independent of these, upon climate. Port wine, *e.g.*, makes them darker than natural; and a tropical climate does the same. 10. Lawson and Frerichs have demonstrated that the brown colour is not due to bile alone, or any modification of it, but is due to a special secretion (in the colon?). 11. In a total absence of bile there is no brown colour at all.

Putting these considerations together, we deduce:—

a. That the peculiar colour of the stools in each animal does not depend upon a variety of colour in the bile. *b*. That the colour is due to some change which ensues in the colon. *c*. That the colour varies with the food, the bile remaining the same. *d*. That no change occurs in the colon in the total absence of bile.

The question, then, is narrowed to this: Why is calomel supposed to restore a rich brown colour to the stools, by purging, by acting on the liver, or otherwise?

The answers are curious:—

1. It does not do so when used alone, (see Pereira, pp. 851-3; Taylor, pp. 416-17.) 2. It does not do so when combined with colocynth, jalap, senna, decoction of aloes, or other black draught. 3. A patient of mine tells me that every physician he has consulted declared that he had congestion of the liver; for he is subject to whitish stools. Each had prescribed mercurials; but, he adds, *these invariably make the motions a lighter colour*. 4. Diarrhœa, with clay-coloured evacuations, without jaundice, is not uncommon; and experience tells us that the colour returns to the stools again in four days without any medicine whatever having been taken.

The only positive conclusion, then, that we can draw is, that *occasionally* calomel *seems* to bring about a change in the colour of the fæces in the colon; and the evidence that it does this by influencing *the liver*, is not a bit stronger than that it does so by acting on the colon.

But we must not rest here; we must point out again the difference between a theory and a fact. Mr. Pope assumes that white stools are an indication of congestion of the liver. Let us enumerate a few truths bearing on this point.

1. There is generally congestion of the liver in every case of cardiac dropsy; but in such cases the stools are perfectly healthy. The same remark applies

to old asthma and bronchitis. 2. Assuming that there is some similarity between congestion and inflammation, let us turn to Morehead's work on *Indian Diseases*, the latest and best authority we have. We read thus, "In acute hepatitis, the alvine discharges are not peculiar." (p. 373.) "In portal congestion the stools may be dark or pale." (p. 387.) "In hepatic abscess, the fæces are unchanged prior to the abscess bursting." (p. 378.) With this evidence, what can we say of the *theory* which assumes that white stools are proofs of hepatic derangement; and that a restoration of their colour is a proof of the action of mercury on the liver?

In conclusion, let me suggest to your correspondent that an invocation to the Deity is a dangerous weapon to use against an opponent, when Morehead writes: "The opinion at one time generally entertained that mercury exercises some special power in hepatic inflammation is unsupported by clinical experience." (p. 389); and again, "There is no organ so prone, in India, as the liver to become the seat of suppuration, and the constitutional states which favour this result of inflammation, and *frequently cause it*, are certainly *aggravated by mercury*." (p. 390.) Might I not fairly retort by saying "God help the patient with congested liver whose doctor has mercury for his panacea, and who sees in every white stool an earnest demand for a calomel drench." I forbear to remark further on your correspondent's note than to add, that if writers would think more deeply, and reason more closely than they do on the facts before them, we should have more tolerance and less difference of opinion than at present characterise the members of our profession.—*British Med. Journal, Nov.* 2, 1861, *p.* 478.

40.—ON THE CAUSE AND CURE OF TAPE-WORM.

By Dr. Fleming, Birmingham.

In his clinique at the Queen's Hospital, Dr. Fleming has frequently made the cause and cure of tape-worm the subject of clinical remark. This affection prevails to a wide extent in Birmingham, and is often seen in its more severe forms among the out-patients. As compared with Edinburgh, Dr. Fleming observes, that the greater frequency and severity of the disease are very marked. The patients may often be recognized on entering the consultation room by their worn, vacant aspect,—their trembling gait and hands, staring eyes, and hesitating manner. Many suffer for years, with alternate periods of aggravation and improvement. Of the relation of cause and effect between the measle of the pig, or *cysticercus cellulosæ*, and tape-worm, this town affords ample evidence. Dr. Fleming has ascertained that large quantities of measly pork are sold and used as food in Birmingham. It is not exposed for sale in the public markets, but is disposed of privately, and doubtless, much of it is employed in the manufacture of sausages. These are made use of to a large extent by the working classes, and are frequently eaten imperfectly cooked or nearly raw. Dr. Fleming and Mr. Lloyd have found portions of the "measle" in the sausages with the microscope. Although the worm must be for the most part destroyed in the mincing, it may sometimes escape sufficiently intact to renew its life in the bowels of an unhappy artisan.

Dr. Fleming has made comparative trials of several vermicides on the tape-worm; amongst others of male fern, kamala, koosso, and spigelia. All are useful, but Dr. Fleming finds the first to be most efficient and the safest. He gives it thus:—℞. Ol. filicis, ʒj; mist. acaciæ, ℥ ss; M. ft. haust. To be mixed with an ounce and a half of sweet milk and taken at bed-time. The patient must omit the dinner and evening meal of that day. Taken thus, on an empty stomach, the mixture is carried speedily into the intestines, to feed, and, at the same time, poison the hungry parasite which nestles there. Milk is the favourite food of the worm. A large experience leaves no doubt of the great efficiency of this medication. In 1857 Dr. Fleming obtained a supply of the kamala from Dr. Anderson, of the H.E.I.C.S., and was one

of the first to use that drug in this country. As a worm-killer, it is often very efficacious, but less so than the male fern; while its action on the bowels and secondary depression of the system are often unduly severe.—*Med. Times and Gazette, Nov.* 16, 1862, *p.* 512.

DISEASES OF THE URINARY ORGANS.

41.—ON HEMATURIA.

By Dr. Hyde Salter, F.R.S., Assistant Physician to Charing-Cross Hospital.

[The two conditions—hematuria and uræmia—are often coexistent, but each often exists alone. We constantly get blood in the urine, without there being the slightest evidence of urea in the blood, the functional power of the kidney not being in any way lost.]

Though the meaning of the word hematuria is peculiarly definite, and its existence most easily recognisable, its pathological significance is peculiarly uncertain, and the diagnosis based on it wide and doubtful. Any condition in any part of the urinary (or genital) passages giving rise to the escape of blood may cause hematuria; and the presence of blood in the urine, taken *per se*, tells no more than that such a condition somewhere exists. A patient may have a calculus lodged in the pelvis of his kidney, and hematuria may be a principal symptom; a man suffers acute congestion of his kidneys from exposure to cold, and straightway hematuria appears; a child has scarlatina, and a fortnight afterwards passes urine black with blood; a man falls on his back across a pole, and, the next time he passes urine, it is seen to contain blood; a woman is menstruating, and she has temporary hematuria; a man has been catheterised, and blood is passed in his urine. Stone in the bladder, simple ulceration of the mucous membrane of the bladder, fungus hematodes of the bladder, malignant disease of the kidneys, are all frequent causes of hematuria. There is a peculiar and common idiopathic hematuria in children, the blood evidently coming from the bladder; and there is a form of bleeding from the mucous membrane of the bladder, without any apparent structural change in it, not at all uncommon in old men. Hematuria is sometimes the principal form of bleeding in cases of diathetic hemorrhage. In all these ways, and I dare say in many more, hematuria may occur; and urine slightly smoky, or black with blood, may be shown you; and you simply know that it is bloody urine—you know just that, and nothing more. A naked-eye inspection of the *pôt de chambre* will not take you a step beyond this simple fact.

When I say *not a step*, I am saying perhaps too much; in one or two cases it will, I think, take you just a *step*. I think, for instance, that hemorrhage from the ultimate structure of the kidneys never has a redness such as we see in some other urinary hemorrhages, as from vesical calculus, for instance; and I think, too, that the temporary hematuria of menstruation has a distinctive appearance; it looks simply like urine, *plus* a little fresh blood, and may be simulated exactly by mixing a few drops of blood with urine out of the body; it never has the *smokiness* of many other hematurias. This slight negative information a mere naked-eye inspection *may* furnish; but, beyond this, I think my observation holds good.

How, then, are we to determine whence the blood comes? I think there are few cases indeed that will leave this at all doubtful, after a thorough sifting of the symptoms, local and general. But short of this, without a lengthened examination of the case, I think we may at once determine, within certain limits, what is the seat of the hemorrhage. We may say at once that the blood does or does not come from the ultimate structure of the kidneys; we may say that it does or does not come from the urethra; and we may say that it comes from some part of the urinary tract of mucous membrane below the one and above the other; thus—

If we find blood-casts of the uriniferous tubes, we know that the blood is

Malpighian, the hemorrhage ultimate renal. If a thorough examination of the urine fails to find any casts, the blood, *if at all abundant*, is *probably* not from the ultimate structure of the kidneys.

If the blood is passed without any urine, or without the act of micturition, it is urethral. If, too, it is confined to the first jet of the stream, it is urethral. If, again, it is confined to the last drop or two, the last jet by the ejaculator urinæ, it is probably urethral, and situated in the bulb.

If the blood is never passed except by an act of micturition, and intermixed with urine, and there is a clear absence of casts, then it comes from above the urethra, and below the ultimate kidney-structure; but "further this deponent sayeth not." It may be from the pelvis of the kidney; it may be from the ureter; it may be from the bladder.

You can, then, in this simple way, by a look with the microscope, and one or two plain questions, without any reference to the general symptoms, reduce yourself to this triple alternative, if I may use the expression: you can tell if it comes from the urethra, from the recesses of the kidney, or from some uncertain point between the two.—*British Medical Journal, March* 22, 1862, *p.* 300.

42.—"DUMB BELLS" OF OXALATE OF LIME, FROM A CLINICAL POINT OF VIEW.

By Dr. Lionel S. Beale, F.R.S., Physician to King's College Hospital.

Oxalate of lime, as is well known, occurs in human urine in two different forms:—1. In well-defined octohedral crystals. 2. In the form of spherical or oval masses more or less resembling a "dumb-bell" in their general appearance, and spoken of as "dumb-bell" crystals. In very rare instances, this salt crystallizes in other forms than those above-mentioned, but these need not now engage our attention.

The octohedral crystals of oxalate of lime are not generally present when the urine is voided, and they do not commonly make their appearance until some time after the secretion has left the bladder. In not a few instances this urinary deposit actually results from changes occurring in the urates after the urine has been allowed to stand for some time. Minute octohedra having once made their appearance, may be seen in some specimens of urine to increase in size during some days. In other specimens, however, no such alteration takes place. The minute octohedra are most frequently met with, and they form one of the most common urinary sediments.

The presence of octohedral crystals of oxalate of lime in the urine does not necessarily depend upon the existence of any peculiar habit of body or diathesis; indeed, in the great majority of cases in which the deposit occurs, no special symptoms are associated with it.

There is reason to believe that the crystals are not discovered in very many cases in which they are present, from the attention not being directed to an examination of the urine, owing to the absence of any symptoms indicating a morbid condition of this secretion. But little assistance in the diagnosis of many cases is afforded by the presence of this deposit in the urine: and one may even go so far as to say that, as a general rule, it is of no clinical importance whatever. It is found in the most opposite conditions of the system, being common enough in the urine of poor, broken-down subjects, and in that of well-nourished country gentlemen. It often appears if we live too well and take too little exercise. It is common enough in chronic pulmonary affections, as chronic bronchitis and emphysema. In dyspepsia, in jaundice, in general debility and over-fatigue, and in persons who have over-worked their minds it is generally found. Like the urates, it probably indicates that the processes connected with oxidation are not performed to their fullest extent. I have found it, however, and on several occasions, in very large quantity, in the urine of men who appear in all other respects in perfect health. Mineral acids, tonics, fresh air and exercise, and attention to the action of the skin and careful regulation of the

diet, especially as to quantity, will generally cause the formation to cease in those cases in which it is desirable to interfere at all. There are, however, cases where oxalate of lime is found in the urine which are characterised by the presence of most important symptoms requiring active interference on the part of the practitioner; but in such instances he is influenced in the view he takes by the general character of the symptoms rather than by the fact of the presence of the oxalate of lime in the urine.

"*Dumb-bell*" *Crystals.*—The presence of the dumb-bell crystals in the urine is by no means unimportant clinically. These crystals do not increase in size after they have been allowed to remain in the urine. They are always present when the urine is first passed. They never result from any change occurring in any constituents of the urine after the secretion has left the bladder. Dumb-bell crystals are occasionally present in the casts of the uriniferous tubes, and have been found impacted in the tubes of the kidney. There can be no doubt that these peculiar crystals are formed in the secreting tubes.

I have seen small collections of dumb-bells in the urine, and have specimens of small oval calculi not more than the 1-200th of an inch in length. The latter, like the former, consist of oxalate of lime, and seem to have been formed in the same manner. In some specimens, several dumb-bells which are connected together to form a small compound mass, are seen to vary much in size. A few of the largest crystals seem to be increasing at the expense of the smaller ones, and at last the entire mass consists of only one or two comparatively large crystals.

These dumb-bell, spherical, and oval masses of oxalate of lime formed in the secreting tubes of the kidneys, are, in truth, microscopic calculi, and they frequently form the nuclei of larger concretions. I have examined several specimens of small calculi, and in the great majority have actually found dumb-bell crystals in the centre. In the small uric acid calculi which are not unfrequently formed in great number, it is easy to demonstrate this point. If the uric acid be very carefully dissolved away by solution of potash, the dumb-bells will be found in the slight residue which has resisted the action of the alkali. I have now repeated these observations on more than ten specimens from different persons, and think that dumb-bells very generally form the true nuclei of uric and oxalate of lime calculi. Dumb-bells are sometimes passed in considerable number, in the urine of patients who are suffering from this formation of small uric acid calculi, and I have now under my care a patient who has for months been passing large quantities of blood in the urine with several masses of a brownish colour looking like shreds of fibrinous matter. Upon examination these were found to consist of pus and blood corpuscles, probably fibrinous matter, crystals of hæmatin, numerous fungi, and hundreds of dumb-bell and spherical crystals of oxalate of lime.

Treatment.—From the above facts it seems important that we should endeavour to get rid of these dumb-bell crystals or minute calculi when we know they are formed, and employ means to prevent their formation in cases in which the tendency exists. The first object is attained by making the patient take once a week large quantities of fluid during a few hours of the day. Vichy and other alkaline waters, or a solution of bitartrate of potash, may be given. Thus the quantity of urine secreted is very much increased, and these crystals are washed out of the tubes. To diminish the tendency to the formation of this urinary deposit, attention must be paid to the general health, and especially to the state of the digestive organs. Iron, quinine, and other tonics may be given according to the general state of the patient, but air, exercise short of fatigue, and a diet regulated as to quantity according to the proportion of nutrient material required by the system, are of the first importance.—*Medical Times and Gazette, Jan.* 4, 1862, *p.* 6.

43.—RECOVERY FROM BRIGHT'S DISEASE.

By Dr. Thomas Williams, F.R.S., Physician to the Swansea Infirmary.

Twenty years ago, Bright's disease, in all its forms, was believed to be incurable. Ten years later, it came to be admitted that recovery was possible from some of its varieties. This change of opinion was in great part due to the researches of Dr. George Johnson. The investigations of living pathologists and practical observers place it beyond doubt that *permanent* and *complete* recovery may take place, under certain circumstances, from this disease. But though this conclusion has been arrived at by a few special investigators, it is by no means accepted by the profession at large. No physician acting on behalf of an insurance company will, even at the present time, pass as "healthy" the life of one who has once been the subject of albuminous urine (that from scarlet fever excepted). It is therefore believed that the cases which are recorded in this paper will constitute no uninteresting contribution towards a better knowledge of the natural history of this disease. In the narrative of these cases all details will be studiously avoided: such particulars only will be stated as will enable the reader to perceive the form or type of disease under which they should be classified.

Case 1.—Twenty years ago, a medical student in whose fate I was deeply interested, and whose age was 22 years, became affected with an illness under the following circumstances: At the end of the winter session in one of the London hospitals, at which he had been severely working, and while enjoying his ordinary health, he was seized in one night with bloody urine, and frequent desire to pass it. The urine was first discovered to be dark in colour, and much reduced in quantity on rising in the morning. His suspicions were excited as to this symptom, and he went to Dr. Barlow, who tested the urine, and found, upon the addition of nitric acid, that it was literally converted into one clot of albumen. My friend on the following day visited Dr. Bright, who at once pronounced the urine to be highly albuminous, and recommended the patient to leave town for the country, holding out no hope of recovery. I then accompanied my friend to Dr. Prout, who, after careful examination, said that the case was one of hæmotrophy of the kidneys, the urine being bloody, highly albuminous, and of high specific gravity. Satisfied, now, that he had become the subject of Bright's disease, my friend resolved to leave for the country. Neither Dr. Bright nor Dr. Prout would hold out any hope of recovery. Dr. Prout said that he had known several people who had gone on for ten years in comparative health, the urine being constantly albuminous. Dr. Golding Bird, upon whom we called just before our departure from London, manifested the strongest and warmest interest in the case, and stated, after a patient examination, that the case was one of "acute congestion of the kidneys." Neither of these physicians recommended lowering treatment.

Our return to the country was made on the tenth day after the first appearance of the symptoms. The urine was still scanty—less than three quarters of a pint in the twenty-four hours; the dropsical symptoms every day increased; the urine was as dark as porter, and highly albuminous; the skin was dry and feverish; the breathing was growing in tightness and difficulty. In a fortnight further the body had greatly increased in size, while the urine had scarcely at all augmented in quantity. Convinced that he was going to die, greatly alarmed at the swelling and difficulty of breathing, the patient determined to resort to the compound jalap powder and warm baths. From this moment a change for the better became evident; the skin began to act, and the urine increased in quantity. Thus he continued for three months.

About this time he again visited Dr. Prout, who reported that the case was going on favourably. The urine had increased in volume; the skin was acting; the dropsy had diminished very considerably; the quantity of albumen in the urine had also become much smaller, although large enough still in amount to form a clot, and it had become smoky in colour, indicating a great reduction in the proportion of the red corpuscles. He prescribed the citrate of potash and iron.

Dr. Prout, on this occasion, introduced us to Lord S., who was (in 1843) labouring under precisely the same symptoms as my medical friend. We then again repaired to the country, a constant correspondence being kept up with Dr. Prout. For six months the patient continued gradually to improve. At this period the dropsy had entirely disappeared, he had become pale and thin, the urine had greatly increased in quantity, and micturition was frequent both during the day and the night. The urine, allowed to stand in a long conical glass, deposited masses of casts, epithelial scales, oil-globules, red corpuscles, and granules. Possessed of a good microscope, my friend made repeatedly beautiful drawings of these objects. It would be easy to prove, by reference to his elaborate notes, that he fully anticipated Dr. Johnson in his definition of the fibrinous casts. He invariably stated that they consisted of fibrin, which had undergone coagulation in the uriniferous tubes, —that the epithelia constituted the scaly lining of these tubes,—that the oil-globules had been secreted by these cells into their interior, as Bowman had explained as regards the oil and cells of the liver.

For eighteen months this case was most minutely watched. During this time the urine averaged three pints in twenty-four hours; it continued to present a slight muddy or smoky tint; in specific gravity it never fell below 1012; the albumen gradually lessened until at length it contained no clot, but only a slight granular opalescence was formed under heat and nitric acid. The patient was still pallid, dyspeptic, and nervous, but active in intellect and bodily habits.

It could not be said that the albumen had entirely and completely disappeared from the urine until two years had elapsed from the first attack. For several years (certainly three or four) after this date, there persisted a slowly-decreasing amount of renal irritability—that is, micturition frequent at night, and the urinary secretion was rapid and excessive in the day, if a *stimulus* at any time were taken. At the end of two years it was easy to detect, in specimens of the urine which had been allowed to stand for some hours, a few casts, cells, and oil-globules. No return of the bloody urine after the first onset ever took place, nor of the general œdema. No evidence of any serious inflammation at any stage of the case ever occurred. Palpitation of the heart, caused by anemic condition for some years, was complained of.

Nearly twenty years have elapsed since my friend was down-struck by his attack. He is now in perfect health. The urine has been perfectly healthy for at least fifteen years. He has never experienced the slightest relapse or return of the original symptoms.

It should be stated that the subject of the above case experienced a severe attack of scarlet fever at the age of 16 years. During the interval which elapsed between this date and the time when he became the subject of Bright's disease, he passed through a severe illness from typhoid fever. He believes, from the account given him by his mother, that slight dropsical symptoms followed his illness from the scarlet fever.

Case 2.—In 1843 and 1844, when my patient and myself were visiting Dr. Prout, the latter introduced us to Lord S.; he conducted the analysis of the urine in our absence; it was bloody, albuminous, and scanty, and the patient was dropsical. The course and stages of his case have been most remarkably similar to those of Case 1. A recent communication has assured me that his lordship has been in the enjoyment of perfect health for many years, and is so at the present time.

Case 3.—In 1844, in the practice of the late Mr. Terry, of this town, there occurred a case which came under my especial notice. A collier, aged twenty-one years, became dropsical. Mr. Terry and myself examined the urine: it was scanty, and had been so for eight or nine days; it was red in colour and highly albuminous. The body was swelled considerably, the breath tight, and the skin hot and feverish. Mr. Terry, who was then an old practitioner, remarked that such cases had been familiar to him for many years, and that he had always been in the habit of treating them by *bleeding*. He withdrew about eighteen ounces of blood; the patient fainted. I watched him narrowly for a month. On the day following the bleeding, the breathing had improved,

the urine increased in amount, the skin became moist, and the albumen diminished in bulk. In six weeks he was at his work again. For fifteen years I have from time to time seen this man: he has never had a return of the blood or of the albumen in the urine; he has never suffered from pleurisy, and the urine is now quite healthy.

Case 4.—The late Mr. Terry, who was concerned with a fatherly interest in the case of the medical student which I have described, being summoned to visit a farmer's son, who was eighteen years of age, requested me to accompany him. This was in the autumn of 1844. We found the patient swelled with dropsy everywhere; the urine very small in bulk, like porter, highly albuminous. Mr. Terry took away blood to syncope, according to the fashion of that time amongst country practitioners. Deeply interested myself, I continued to watch this young man for some time after. He certainly was quite well in two months from our first visit. I know him to be at the present time a fine, strong, hearty man. He had never had scarlet fever, and has never suffered from illness since that time.

Case 5.—In 1845, I was requested to visit a patient in company with the late Dr. Rowland, of this town. Dr. Rowland knew my medical friend, and took much interest in his case. As soon as a case occurred to him which appeared to be similar, he drew my attention to it. We went. We found a young man, about twenty years of age, in a highly dropsical state; the urine sanguineous and scanty, clottily albuminous. The patient had been in this state for some days. I related to Dr. Rowland the treatment adopted by Dr. Terry in such cases. I contrasted it with that of the great London physicians. He agreed that we should adopt an intermediate course. Our patient was purged, cupped on the back, and put on low diet. He was six months in recovering, but he did recover, and has ever since been quite well. I have only recently seen him; he is perfectly healthy.

Case 6.—A young clergyman, in 1845, came to me in great distress. He was about 25 years of age, and had lately entered upon a curacy. He had for some time suffered from indigestion. Suddenly he found that he was swelling, and that his urine was becoming scanty and high-coloured. I found that it was charged with blood corpuscles, that it was highly albuminous, and of high specific gravity. I treated him as Dr. Bright, Dr. Prout, and Dr. Golding Bird had treated my friend. He was purged every morning with the compound jalap powder, and very soon he was put upon mild preparations of iron. He was at least two years in recovering, but he is now quite sound and hearty. I am not aware that he has ever experienced any illness from that time to this.

The preceding six cases I venture to relate as cases of perfect and lasting recovery from Bright's disease.

During the last twenty years I have seen many others. I am sure that many of them are now alive—some of 10, 8, 7, 6, and five years old. During the last few years many of my "Bright patients" also have died.

Twenty years ago, not for a moment supposing that the life of my medical friend would have been long vouchsafed, I grasped with eagerness at points of relevant practice, and noted the incidents of every case occurring either in my own practice or that of my friends. I feel quite confident that the mode of treatment adopted by the practitioners of that time was the most happy and the most rapidly successful. In every case, *at the right stage*, in which venesection was resorted to recovery was rapid. In recent years, I have never hesitated in my own practice, in cases which Dr. G. Johnson has so excellently described as those of "acute desquamative nephritis," which corresponds with the hæmotrophic kidney of Prout, and with the first stage of the large white mottled kidney of more recent writers, to conduct the treatment upon the antiphlogistic plan. Of late years, from my own experience, the conviction has strongly grown upon me that under appropriate, judiciously-directed management, every one of these cases of hæmotrophy *ought* to issue in permanent and complete recovery. But, alas! in this country thousands of persons die under this unquestionably *curable* form of renal

disease. As soon as the urine becomes bloody and albuminous, and the system œdematous, resolute treatment should be adopted.

Ever since it was my fate to track the march of the symptoms in the case of my medical friend, I have been convinced that his recovery would have been much more speedy if the treatment had been more decisive. But that is past. Let us deduce the lesson. In every instance in which recovery has taken place, the urine has been bloody and its specific gravity high, the system œdematous, the general power good. In the rapid recoveries, venesection has been adopted; in the slow recoveries indifferent treatment has been resorted to. The latter, modifiedly, may be the right course in large cities; in the country unquestionably the former mode of practice is the best. In no single instance in which the urine has slowly and insidiously become albuminous, in which the system seldom becomes œdematous, in which the urine never presents a bloody tinge, have I known recovery to have occurred. In such cases the kidneys are small, contracted and granular; and, with Dr. Dickinson, I believe in the specific distinctness of such cases from those commencing with œdema and bloody urine. From all that I have observed, I yield my support to those who maintain that the fatty and amyloid degenerations are stages of one declensionary course, and that they draw their commencement from the hæmotrophic kidney.

No observer in England has studied the conditions of the general system which always prelude the development of kidney disease. These preliminary stages do exist, and are well marked. On another occasion they shall be discussed.—*Lancet, Nov.* 23, *p.* 492.

44.—*Albuminous Urine.* By METCALFE JOHNSON, Esq., Lancaster.—In examining albuminous urine I have observed a source of fallacy in the nitric acid and heat test which is new to me, and very interesting, but which I have never seen noticed in works upon the subject. It is the prevention of coagulation by heat through the agency of nitric acid.

It is laid down as an axiom that urine clouded by heat indicates either albumen or phosphates. If the former, nitric acid increases the turbidity; if the latter it clears it. By reversing the operation in urine highly charged with albumen, adding to a drachm of urine one drop of nitric acid, a precipitate falls, which after shaking is redissolved by heat. The solution on cooling forms a solid jelly, which I presume is gelatine. This, however, is not redissolved by heat. If access of nitric acid is applied so as to precipitate all the albumen, the heat does not dissolve it.

The error of supposing urine containing albumen to be non-albuminous might be brought about thus:—Supposing a test-tube, in which nitric acid had been largely applied, to be simply put aside, after emptying the contents, without washing, and taken for the next experiment by mistake for a clean tube, the urine on boiling will become clear, and thus deceive the operator into the impression that he was dealing with urine free from albumen.—*Lancet, Nov.* 16, 1861, *p.* 487.

45.—*The Influence of Arsenic upon Albuminuria, in a Case complicated with Psoriasis and Lichen.* Under the care of Dr. FRED. FARRE, at St. Bartholomew's Hospital.—The tonic and anti-periodic effects of the liquor arsenicalis are well known in the practice of medicine. According to Dr. Anthony Todd Thompson, it determines to the surface, equalizes the circulation, excites the cutaneous capillaries, and improves the general action of the skin. Some months back we watched with considerable interest a case of acute renal anasarca, which became complicated with psoriasis and lichen, for which this solution of arsenic was employed, and with extremely beneficial effects upon the quantity of albumen in the urine.

The patient was a female, aged nineteen years, who was admitted with acute renal anasarca of two weeks' duration, possessing all the usual characters of that affection, with the urine not only highly albuminous, but very

smoky. For this condition she was ordered suitable treatment. About a month after her admission, an eruption of psoriasis appeared, associated with lichen, on the arms and hands, for which she was ordered five minims (afterwards increased to seven) of Fowler's solution, in peppermint water, three times a day. This was found to be of service, not only in dispelling the cutaneous eruption, but also in diminishing the quantity of albumen to such an extent that it had almost wholly disappeared from the urine. In two months the eruption had much diminished, and was quite cured by the tenth week; and although the albumen had all but gone, it subsequently reappeared in very small quantity whilst taking the arsenic. Besides other treatment, she had alkaline baths and creasote ointment. She left the hospital comparatively well at the end of the twenty-fifth week, being much stouter and altogether looking a great deal better. The arsenic had acted evidently as a powerful tonic, and exercised a very decided influence on the albuminuria.—*Lancet, Jan.* 18, 1862, *p.* 68.

46.—ON THE INFLUENCE OF ALKALIES IN CHECKING THE PRODUCTION OF ARTIFICIAL DIABETES.

By Dr. F. W. Pavy.

[Dr. Pavy has already shown that diabetes may be artificially produced in an animal by injuring certain portions of the sympathetic. He now shows that this effect may be prevented by the influence of an alkali; the amyloid substance previously called by him hepatine disappears without any production of sugar.]

The following is a summary of the facts brought forward at present:—

That the introduction of carbonate of soda into the circulation prevents the production of saccharine urine after lesions of the sympathetic otherwise occasioning it.

That the carbonate of soda injected into the general venous system does not prevent the urine from becoming saccharine after the destruction of life, when the circulation is kept up artificially; but, injected into the portal system, so that all may pass through the liver, it has the effect of keeping the urine entirely free from sugar.

That in the disappearance of the amyloid substance, under the influence of the carbonate of soda, it is not concealed nor transformed into sugar, nor destroyed by any direct chemical power possessed by the carbonated alkali. The facts before me would suggest that it is transformed by a process of the nature of catalysis, the product having escaped as yet being discovered.

That the carbonate of soda, injected into the liver after death does not effect a disappearance of the amyloid substance, but even in moderate quantity holds the saccharine metamorphosis completely in check.

That there is probably a close connection between the disappearance of the amyloid substance, the production of fat, and the condition of the bile.—*Guy's Hospital Reports, Vol.* vii. *p.* 208.

47.—*The Influence of an Acid in producing a Diabetic State of the Urine.* By Dr. F. W. Pavy.—[Dr. Pavy then proceeds to show that an acid produces diabetes. He injects phosphoric acid into the general circulation. He says:]

It is evident that, when a sufficient quantity of acid is introduced into the circulatory system, the operations of life are so altered that sugar appears to such an extent in the blood as to occasion a more or less strongly marked saccharine state of the urine. From all that I have seen, it appears to me reasonable to conclude that this result is due to a perverted condition of the processes belonging to the liver. The unnatural state of the blood occasioned by the presence of the acid seems to induce the change of the amyloid sub-

stance into sugar. It seems to promote that change, the result of chemical action, which occurs with such activity after death, and which must be held in abeyance under natural circumstances during life, sugar being found in the system to so scarcely an appreciable extent.

When the quantity of acid used has been large, the blood nearly loses its power of coagulating. In some of my experiments, also, I have noticed that such a morbid state has been induced as to lead to an escape of blood from the vessels during life. I have met with ecchymosis of the liver and stomach, an accumulation of blood in the stomach and intestine, and the presence of blood in the urine.

Looking upon the effects of the acid injection as due to an action upon the liver, it occurred to me that this organ might be much more directly influenced by injecting the acid into a branch of the portal system, instead of the jugular vein. It happens, however, that a circumstance occurs to render this mode of experimenting unsuccessful. Although I have never found the acid lead to a solidification of the blood in the vessels when introduced into the general circulation—indeed, as I have mentioned, the effect is the reverse of this—yet when injected into the portal system, it causes the blood belonging to this system to solidify, and the vessels to become so plugged up, that the circulation of portal blood is completely stopped. I have made five experiments in this way, and in all but one, the effect referred to has occurred. In three of the cases, the acid was introduced undiluted, but injected very slowly into one of the mesenteric veins; and in each, under an ounce was used; in the fourth, six drachms of the acid, diluted with twelve drachms of water, formed the injection employed. All the dogs died in less than an hour and a half; and the trunk of the portal vein, with its larger divisions in the liver, was found plugged up with solidified blood, so as entirely to check the circulation. The tissue of the liver was, in places, white and hard, as if it had been chemically acted upon by the acid. There was no sugar to be detected in the urine; and the liver, submitted to an ordinary examination after death, behaved in the usual way. From one of the experiments, I learned that the effect produced by the acid in solidifying the portal blood instantly occurred on the first portion of the injection being made, so that the portal circulation was at once stopped; which would account for the absence of any sugar in the urine.

In the experiment where the circulation remained free, half an ounce of the acid was used, diluted with two ounces of water. The injection was slowly made. The urine, in an hour and a half's time, was found to contain a slight amount of sugar. Two hours after the injection, when life was destroyed, the liver was perfectly natural in appearance, and all the vessels unobstructed. There was, therefore, no engorgement of the spleen, nor blackening of the intestine from congestion, as was the case in the other experiments.

Finding this mode of experimenting proved objectionable, I next tried the effect of introducing the acid into the alimentary canal, instead of directly into the portal system. Injecting it into the stomach was of no use, for it very soon induced vomiting, and was rejected. Half an ounce of phosphoric acid, injected into into an empty stomach through a tube passed down the œsophagus, I found rapidly to occasion repeated acts of vomiting. Resorting to the use of chloroform, I, therefore, introduced the acid into the duodenum or some other portion of the small intestine.

I have a record before me of five experiments that I have performed in this way. They in the most striking manner show that an excess of acid in the system occasions the production of saccharine urine.—*Ibid. p.* 210.

48.—*On the Dietetic Treatment of Diabetes.* By Dr. F. W. Pavy.—We are in a position, I conceive, safely to say thus much concerning the nature of diabetes. In the healthy person, the starch and sugar consumed as food are assimilated, and rendered subservient to the requirements of the economy. In the diabetic, there is an absence of this power of assimilation, so that what-

ever of such food is taken, on account of its not being properly elaborated, it filters through the system in obedience to the simple laws of physics. I believe, in thus passing through the system, the sugar in such quantities so alters the natural character of the blood as to unfit it for properly performing the healthy operations of life, and in this manner occasions the functional disturbances and structural changes that occur in diabetes. The obvious rational plan of treatment (until a means can be discovered for restoring to the body what is deficient, or correcting what is faulty) is restriction from food charged with the unassimilable materials.

The sugar that may still continue in the urine under abstinence from vegetable food results, I consider, from a non-assimilation of the amyloid substance manufactured by the liver out of the materials resulting from the disintegrating of tissue and the retrograde metamorphosis of aliment absorbed into the circulation in excess. As long, however, as the sugar in the urine can be kept down to ten, twelve, or even fifteen hundred grains a day, my experience so far as it has extended is, that the patient does not in any way know of the existence of his complaint otherwise than by the necessity of restricting himself to the regimen essential to attain this object. With only such an amount of sugar escaping, I believe that the blood remains adapted for the healthy performance of all the functional operations of life. —*Ibid. p.* 382.

49.—GLUTEN BISCUITS FOR THE DIABETIC.

By Dr. John Camplin, Islington.

I wish to call the attention of the profession to a new preparation for the diabetic, which appears to me important. I allude to the gluten powder and buscuits now prepared by Mr. Blatchley, and sold by him under the title of English Gluten.

It is not necessary that I should describe the unpleasantness of the old preparations of gluten, nor discuss the merits or demerits of the French preparations at present in use; it is sufficient to observe that they have proceeded on a principle of obtaining something analogous to fermentation. In this new preparation nothing of the kind is attempted; the gluten is made as free as possible from starch, and then brought into a state in which it may be made into cakes or biscuits.

This preparation, will, I expect, be generally agreeable to patients; and I regret that it cannot be furnished so cheaply as the bran, and is therefore less fitted for hospital use. It is, however, less expensive than the French gluten, and a small allowance will suffice. Whether the bran or gluten is used, I prefer that the powder should be purchased and made into cakes at home; but those who wish to avoid all trouble will find the gluten biscuits very pleasant.—*Med. Times, Nov.* 30, 1861, *p.* 568.

50.—ON THE DIET OF DIABETIC PATIENTS.

By Dr. F. W. Pavy.

[The following is part of a letter by Dr. Pavy in answer to Dr. Camplin. The two physicians are rather at issue on the subject of the diet proper for diabetic patients.]

Dr. Camplin says, "I am induced to make these remarks, inasmuch as a cursory view of Dr. Pavy's case would lead the reader to suppose that the bran and gluten had better be dispensed with, since the urine and sugar (although far below what they were during the mixed diet) increased during their use." Such, I must say, is the opinion I have formed from the results observed in this and in other cases. I do not deny that instances may occur where it may prove desirable to allow the bran biscuit or gluten bread; but, looking at diabetes in the light there is reason to consider it ought to be re-

garded, the exclusion of food containing starch and sugar rationally constitutes the most essential point in its treatment.

Without entering into any theory, it may be stated that the disease is attended with a want of proper assimilating power over the saccharine element of our food. When bread is consumed by a healthy person, its starch and sugar are so influenced after reception into the system, that they become susceptible of appropriation to the requirements of the economy. They no doubt administer to the production of fat, and are concerned with the maintenance of animal heat. In the case of the diabetic, starch and sugar do not follow a similar course. The saccharine principle remains unassimilated, and simply filters through the system unconsumed, creating in its passage through the blood such an alteration of its natural constitution, as to give rise to those disturbances and, but too often, fatal consequences concomitant upon this disease. From what I have seen, the intensity of the morbid symptoms varies with the degree of saccharine saturation of the blood; keep down the amount of sugar in the blood, as by avoiding its introduction through external sources, and although the patient's disease is not cured, yet his symptoms are relieved. By restoring the blood as near as possible to its natural constitution, we conduce to the healthy performance of the nutritive and other operations of life. But patients sometimes get rid of their complaint; and if there is anything that can favour the production of such an issue, I should say it is keeping down their sugar to the utmost extent possible. In North's case I never got lower than 569 grains during the day; but this was a reduction from upwards of 9000 grains, which he passed in a full mixed diet at the commencement. In another case, where the disease was of about the same intensity as in North, I once observed the sugar fall to 116 grains for the twenty-four hours, and the quantity of urine to 33 ounces. No one at this time could have judged that the patient was affected with diabetes, so robust and healthy an appearance did he present.

I must confess I cannot see the force of Dr. Camplin's argument about an animal diet being fit for the South American, and yet necessarily proving injurious to the Englishman, by producing "serious congestions," unless, as further suggested, "it either passed off very partially digested, or the diabetic drain continued to a considerable extent." I fully concede that man may be looked upon as naturally an omnivorous feeder; but there is sufficient evidence to show that either an animal or a vegetable diet may be taken alone, and will prove adequate for all the requirements of life. Vegetarians contend, from their personal experience, that we do wrong by taking any animal food; and it is a fact beyond dispute that the inhabitants of certain parts of the globe do not know the taste of vegetable food. Arctic discoverers relate to us that the inhabitants of the extreme North live purely upon animal food: indeed, there is none other procurable from the region they inhabit, for them to consume. Seeing that animal food contains all that is requisite for us, and that our organs are thus shown to be adapted to the digestion and assimilation of it, under the exclusion of any other, I cannot admit, with the known power generally possessed by the human body of accommodating itself to the most varied external circumstances, that the Englishman's capacity is limited within such narrow limits as to render him, "in his climate and with his occupations," incapable of subsisting on a purely animal regimen. But, beyond this, experience has not only now, but in former times, proved that the diabetic may be converted, and with advantage to his complaint, into an animal feeder. Use is truly, as well as proverbially, second nature. After a little time I have seen, both in private and hospital life, the animal regimen taken with relish, and without leaving a longing after vegetable food. With the different kinds of meat and fish, combined with eggs, cream, and jelly, a sufficient variety may be easily obtained to satisfy the palate. But there is no reason that such vegetable materials as greens, spinach, and water-cress should be prohibited, as these do not prove injurious to the disease.—*Medical Times and Gazette, Dec.* 7, 1861, *p.* 591.

SURGERY.

AFFECTIONS OF THE BONES AND JOINTS, ETC.

51.—ON AMPUTATION BY RECTANGULAR FLAPS.

By Oliver Pemberton, Esq., Surgeon to the General Hospital, Birmingham.

[The advantage of amputation by a long and short rectangular flap was first shown by Mr. Teale, of Leeds, whose paper on the subject will be found fully given in Retrospect, vol. xxxviii., p. 159. The advantages claimed by Mr. Teale for this mode of proceeding are—1st, The avoidance of tension; 2nd, The formation of a soft covering for the end of the bone, consisting of parts free from large nerves; 3rd, The lessened tendency to pyæmia in consequence of the non-disturbance of the plastic process; 4th, The favourable position of the incisions for allowing a free outlet for purulent and other discharges. Mr. Pemberton now claims to be able to speak from abundant experience as to whether these advantages have been realised. He first proceeds to consider the absence of tension in the early coaptation of the flaps.]

In all amputations, no matter where performed, or by what method, more mischief is developed by undue stretching to close the face of the stump, aided by bandages and plasters, than is produced by any other circumstance belonging to the first stages in the treatment of such operations.

The desire for that, impossible "union by the first intention" may have much to do with this, especially when it is believed in with deficient materials and an intruding bone; but let this be as it may, we shall rightly give a preference to that form of amputation which enables the most inexperienced to measure his flaps with the closest accuracy.

The rules for measurement, laid down by Mr. Teale, render the fitting of the flaps, under all circumstances, a matter of certainty. The angles of the flaps and the edges of the wound are kept in apposition by wire or thread sutures. These may wear their own way out. The stump is simply laid on a pillow covered with a sheet of gutta percha tissue, and permitted to ooze and discharge unrestrained by dressing or bandage of any description.

Under these circumstances there can be no tension. Whatever may be the circumference of the limb, the length of the flaps will be equal to the emergency, provided their measurements are accurately assured. A very satisfactory illustration of the truth of this remark occurred in a case under the care of my colleague, Mr. Bolton, where, from long-standing necrosis of the lower part of the femur, the soft parts of the thigh having become enormously thickened, it was feared that the long flap would not prove of sufficient length to overcome the unusual denseness of its tissues; it was otherwise, however, for it fitted without difficulty, and an excellent stump resulted.

The formation of a soft covering for the end of the bone, and the ability of the stump, thus constituted, to sustain the weight of the body wholly or partially, is the next matter for consideration.

Wherever amputation is performed by Mr. Teale's method, I believe that a durable cushiony stump may be obtained, capable of bearing a considerable amount of direct pressure on its face—containing neither large nerves nor bloodvessels, and absolutely devoid of cicatricial tissue, and in many respects, indeed, comparable with the best of all stumps—that which we owe to Mr. Syme at the ankle-joint.

This fact, from the mode in which the soft parts are folded over the end of the bone, and the resulting cicatrix is rendered distant from the bearing point, can hardly admit of question, and it is no inconsiderable merit in the proceeding that such a description can be given; but whilst we admit thus much, are we prepared, in all situations and under all circumstances, to abandon the transfixion and circular methods in its favour?

All who amputate much, and care to examine the stumps in after years, when wear and tear have told their story on their shapes, know that in by far the more numerous instances they fail in the promises they seemed to give of being able to sustain a direct pressure on their ends, and in many cases degenerate so as to become frequent sources of annoyance, and even of disease.

This inability to bear pressure is especially observable in amputations of the lower third of the thigh and of the leg, when performed, as has hitherto appeared to be most desirable, in these situations, by the circular, in preference to the transfixion method.

The length of limb left in these instances is a strong temptation to both patient and mechanician to try what the face of the stump will bear. But the experiment does not succeed, and has shortly to be abandoned in favour of the tuber ischii on the one hand, or the lower part of the thigh on the other.

The method of amputation proposed by Mr. Teale appears to me, in these situations, to have supplied that want which the plans in previous use failed to afford. It has, indeed, only one drawback, that arising from the length of the long flap necessitating that the bone must be sawn through higher up than in either of the ordinary modes; for though in some instances it may be possible to get a long flap from the anterior surface of the ankle-joint, it will not be prudent to take one too close to the patella, for the chances of sloughing are by no means in this spot to be forgotten.

In other parts of the thigh and leg the short anterior and the long posterior flaps, obtained by the ordinary transfixion method, appear to me to answer everything desired when properly shaped. There ought to be, and I believe there is, in such instances, an entire absence of tension. The healing process may proceed undisturbed by strappings and dressings, and as to the large and small veins being possibly more exposed to take up purulent matter—considering that the discharges can readily escape, and that the vessels divided have been so by the cleanest sweeps of the knife, in contrast to a somewhat niggling and tedious dissection, I am at a loss, on these grounds, and in the absence of any special statistics to the contrary, to understand why a preference should be claimed for the proceeding of Mr. Teale.

Flaps thus formed unite with their line of cicatrix well above the point of pressure, and can, in consequence, take their share of bearing the weight of the body. Moreover, as all the soft parts from behind go to form the long flap, these appear to me to be as truly folded over the bone to form the future cushion, as the same materials from the front are most correctly described to be in the plan of Mr. Teale.

I am by no means therefore prepared to abandon transfixion in these situations in favour of the rectangular method; but, as regards a circular amputation in the same localities, on the ground that the end of a bone must be more capable of bearing pressure when covered by a mass of soft parts, than when only protected by an adherent cicatrix, I should always, where practicable, rather advise recourse being had to transfixion or the rectangular flaps.

In the upper arm the modes of amputation as at present carried out, whether flat or circular, appear to me to satisfy all the requirements that the practical surgeon or mechanician can desire, whilst in the forearm, from the peculiarly tendinous character of the part, and the necessity, always imperative, to keep the stump as long as possible, I deem a change to the rectangular method to be decidedly objectionable.

In this great machinery district, accidents requiring amputation in various parts of the forearm are of most frequent occurrence, and from time to time every possible contrivance is required in the substitute to be provided for

the loss of the skilled hand of the artisan; yet, during nearly fifteen years that I have watched the many stumps that have been made at our hospital, I can hardly call to mind an instance in which the circular method—the soft part being sloped off from bone to skin—did not best meet the present and future exigencies of the case. This statement is, I am sensible, at variance with the opinion of some surgical authorities; but, apart from injuries, in which no choice remains than to make the best of what is left, I must confess my inability to form any other conclusion.—*Med. Times and Gazette, Dec.* 21, 1861, *p.* 632.

52.—ON AMPUTATION OF THE FOREARM.

By Prescott Hewett, Esq.

There have recently been several cases of amputation of the forearm at St. George's Hospital which are worthy of special notice, in consequence of the well-covered and excellent stumps which resulted from the manner in which the operation was performed. It consists simply in making a circular incision through the skin of the forearm, drawing it well back, then making flaps of the muscles, and finally sawing through the bones. The healing process goes on with great rapidity, and the ends of the bones have a firm substantial cushion of muscle, which is perfectly covered by the skin. There is no redundancy of material, but a smooth, even, comfortable-feeling stump. The advantage of this is apparent in such a part of the body as the forearm, where it is desirable to fasten on some artificial means of manual support.

The first operation of this kind was performed by Mr. Hewett in October last for a crushed hand, the patient being up and about the ward in a week. A similar proceeding was practised by Mr. Johnson upon a man, John James B., aged 28 years, who was admitted into the hospital on the 28th December last. A third case that came under our notice was that of George C., a horsebreaker, aged 29, who was admitted on the 6th January with a crushed hand. When we examined him the amputation had been performed ten days: he was sitting up, and apparently quite well; the stump had nearly healed, and was in every respect an admirable one. In one or two subsequent cases the same mode of amputation has been practised with equally good results, and we think it one well worthy of trial in other hospitals.—*Lancet, March,* 15, 1862, *p.* 277.

53.—ON THE THERAPEUTIC INFLUENCE OF MECHANICAL AND PHYSIOLOGICAL REST IN ACCIDENTS AND SURGICAL DISEASES.

By John Hilton, Esq., F.R.S., Surgeon to Guy's Hospital.

[It is commonly supposed that prolonged rest of a joint causes deterioration of it. This is erroneous. If the foot of a Chinese lady be examined, in which the joints have been compressed and motionless for twenty or thirty years, the articular surfaces will be found in a perfect condition, and the structure not in the slightest degree deteriorated. To take another instance.]

If you strap up a foot firmly in order to cure disease of one of the tarsal joints, do the other tarsal joints become anchylosed? Certainly not, necessarily. Here is a beautiful illustration of this fact: It is the foot of a young man whose limb I removed for disease of the knee-joint. Upon the inner side of the foot there may be seen a well-marked specimen of anchylosis between the scaphoid bone and the astragalus, yet the joints nearest to it are not in any way involved in that mischief; there is no anchylosis; they are perfectly healthy. The anchylosis had taken place about two years previous to the amputation of the limb. It is a very typical instance of anchylosis, and I employ it for the purpose to which I am now referring—namely, to show that, although pressure by strapping the foot, quiet, and rest was resorted

to during a long period, in order to cure the disease between the astragalus and scaphoid, yet the only two bones which became anchylosed were those between which the joint was in an unhealthy condition.

Again: Is the larynx, in a case of severe cut throat, voiceless after a rest of six, seven, eight, or ten weeks? or is an unfed stomach, after six or eight weeks, incompetent to resume its duties of digestion, if provided carefully with food, and a small amount of necessary exercise? Certainly not. In cases of cataract, either congenital or of recent date, is the retina incapacitated to receive or appreciate the rays of light carefully introduced into the organ? Certainly not. Then, I say, if all this be true, what right have we to expect that a joint should present different pathological and physiological phenomena?

I think I have advanced facts enough to induce those gentlemen who entertain these opinions, as to scrofula being very constantly the cause of mischief in diseased joints, to admit that other causes, such as a chronic inflammatory condition, and slight, local injury, are by far the most frequent; and, further, that rest to healthy joints does not induce disease leading to anchylosis. Here I might say that, in teaching on this subject at Guy's Hospital, during many years, I have taken occasion to point out to those whom I have had the honour of instructing that the generally received impression as to scrofula being, as a rule, at the foundation of joint diseases, is really not true. This was taught publicly at Guy's before the professional existence of those who now publish such views as new.

It is not stating too much to say that the diseases of joints are modified by age, in one or two respects especially. In the adult period of life we see disease of the individual articular structures, whether of synovial membrane, articular cartilage, or bone; and we observe that not only is the progress of the disease usually slow, but the progress of repair at that period is slow also. In children, however, we note a very quick implication, if we may so term it, of all the articular structures in disease, and quick destruction of the parts, and subsequently very speedy repair. In young children this progress is very rapid in acute disease. Here I have the outline of a cast of an anchylosed knee-joint. It does not, perhaps, represent a specimen of perfect surgery, but it indicates the rapidity of repair in a young person. The boy was three and a half years old, when he fell from a window and damaged his knee. The injury led to suppuration within the joint, and enormous swelling around it, absorption of all the true articular structures, and ultimately to complete bony anchylosis. The whole of this was accomplished in a very few months.

But I advance this case in reference to another and very important point. It is a very common thing for surgeons to conclude that a joint is irreparably damaged when they hear or feel the articular ends of the bones grating upon each other. Now I had a good opportunity of testing the value of that point in this child when he was very ill, and the joint was very much swollen, and suppuration going on in it. My dresser said to me, "When dressing this leg, I can hear and feel the bones grating upon each other;" and he rather looked upon this symptom as fatal to any probability of the repair of the joint. I remarked to him, "Before we amputate this limb we will look into the interior of the joint." I made a free incision into the joint, on its inner side, and washed out all the purulent fluid; and then I saw the dense articular laminæ of bone still upon the femur and upon the tibia, and on rubbing them together the harsh, grating sound was produced. The internal soft parts of the joints were all destroyed. I saw that the articular laminæ on the bones presented a worm-eaten, or minutely cribriform, appearance, indicating that interstitial absorption of the laminæ was going on; and I came to the conclusion that it would be completely absorbed. I therefore secured the bones in as easy and as accurate apposition as I could. I wished to divide the tendons of the flexors; but it was determined by the father that, as the child was so ill, he would not permit him to be touched any more in the way of operation; therefore we did the best we could with mechanical appliances to prevent flexion of the knee-joint, and ultimately the boy got well, and can now walk a mile or a mile and a half with facility. In this case I touched the interior of the joint

with my finger, and I saw distinctly what was the cause of the grating sensation when the surfaces were moved upon each other. It arose clearly from the persistence, for a time, of the articular laminæ between the cancellated structure of the bones and the articular cartilage. You may observe here the articular laminæ interposed between the articular cartilage and the interior of the bone. It is, as we all know, a very dense, compact structure; and it is intelligible how it occurs that when these surfaces were brought together they would create the hard rubbing, grating sensation which we so frequently hear and feel on examining diseased joints.

As another practical application of this point, I may say that the fact of this grating sensation sometimes determines in the minds of surgeons the propriety of excision or amputation. Now it should not be overlooked that there must be a period in every joint disease which is to be cured by anchylosis when this grating sensation is to be experienced, and that is before the articular lamina is actually removed by absorption. When the articular lamina is removed, consolidation can take place, but just anterior to that period it is plain that the friction or rubbing of two layers of compact bone upon each other may produce a rough grating, and might lead unjustly to the conclusion, that those portions of the bone which ought to be in a healthy condition in order to secure subsequent anchylosis, are irreparably diseased. I have repeatedly heard and felt this grating noise in the fingers, ankle, hip, and other articulations, and yet the patient's joints have done well by anchylosis. This practical point appeared to me to be well worthy of a passing remark, especially as I had met with an opportunity of having the explanation of the facts and the symptoms actually demonstrated.

I purpose placing before you this fact, that diseases of the joints in children follow a peculiar course, and not perfectly in correspondence with that which obtains with respect to adults. In adults the individual structures of a joint may be diseased, and each may present its own local indications, or special local symptoms. Thus we may meet with isolated inflammation of the synovial membranes or ligaments, inflammation and deterioration of the articular cartilage, or a disease of the articular ends of the bones in the adult. Now, although these numerous structures are at all periods of life necessarily continuous with each other and closely allied in function, yet it is at the adult period—after the completion of their cognate and harmonious development—that each separate structure seems to have acquired, and thenceforward to manifest both in health and disease, a structural independence, which gives a character of individuality and isolation to the diseases of the different structures of the joint.

In children all the structures of the joint must be formed, built up, and nourished in concert and in relation due to each other, under the excitation of extreme vital energy or epigenesis. There must be a great and intimate sympathy existing between the different parts of a joint during childhood, or during the period of growth, through the medium of an organic or vital power impressed upon those parts by nature. It is this formative or constructive power affixed by nature mutually to the different articular structures which engenders in their pathological conditions a tendency to diffuse disease contemporaneously in all the articular structures. Hence we see in our practice the quick propagation of inflammation from one articular structure to another, and a rapidity of implication of the various structures of the joint in childhood and youth which we do not observe at a later period of life.

It is true, I believe, that in childhood the separate and distinct disease of the articular structures seldom can be recognised with any useful precision during life. In cases of inflammatory condition the results of severe local injury, or of slight injury with strongly-developed constitutional tendencies, all the various structures (including the bone, ligaments, cartilage, and synovial membrane) tributary to the formation of the joint, become at this period of childhood soon more or less involved. It is, therefore, to my mind—and this is no new idea, for I have taught it publicly these last fifteen years or more—an unsubstantiated refinement in most cases of joint disease in childhood to attempt to depict the symptoms indicating distinct or separate

pathological states of the individual structures composing a joint. It is certainly not in accordance with clinical experience, and surely it is not a sound basis upon which to fix and determine the plan of treatment.

This active principle of epigenesis and pathogenesis is not peculiar to the joints alone in children; it is the great feature, it is the pathological type, which this young period of life constantly displays in other parts of the body. Some of those gentlemen whom I now address know better than myself, and I take it upon the statement of those in whom I have confidence, that in adults you meet with pneumonia as a separate disease; you may see pleurisy as a separate disease, or bronchitis as a separate disease, each recognised by distinct symptoms, and treated in reference to the structural implication. But not so in childhood; at that period of life you scarcely ever meet with pleurisy, pneumonia, and bronchitis as distinct inflammations. They all appear together, or there is a general and rapid implication of all these structures nearly at the same time.

So it is in diseases of the brain. It is a common thing to see disease of the membranes or disease of individual parts of the brain in the adult; yet it is rare to see these in the same distinct and isolated manner during childhood. So, as I remarked before, with regard to the larynx. In disease of the larynx in children all the soft parts become equally involved; whilst in adult life, when the laryngeal structures seem to have acquired a normal independence, which they had not previously, they possess also the pathological tendency towards the same kind and degree of independence.

Thus, I think, we mark by analogy a feature of high physiological and pathological importance, and very suggestive as a guide to treatment in practice—viz., the difference between the relative progress and implication of the various structures of diseased joints in childhood and in manhood, showing that in children vigour and rapidity as to the diffusion and progress of inflammatory conditions, as well as rapidity of repair, stand in very strong and distinct antagonism to the like diseased conditions (in all other respects alike) which may attack the same structures, but at a more advanced period of life.—*Lancet, Nov.* 16, 1861, *p.* 467.

54.—ON DIAGNOSIS OF DISEASE OF THE HIP-JOINT.

By John Hilton, Esq., F.R.S., Surgeon to Guy's Hospital.

An error often committed is one which we have inherited from a previous generation of surgeons, and the entail of which I should like to see cut off. It was, and is now in some places, the disposition of surgeons to require that the patient suffering from hip-joint disease should manifest those marked symptoms which are deemed to be characteristic of hip disease, such as the shortening or lengthening of the limb, a fulness or flatness over the gluteal region, want of symmetry in the subgluteal folds, pain at the inner side of the knee, greatly disturbed health, and considerable lameness. Now the very fact of its being the opinion of some surgeons that shortening of the limb is characteristic of the disease, and of others, that lengthening of the limb is characteristic of it; of some, that there should be a want of symmetry in the gluteal region, and of others, that the symmetry is not necessarily altered—all this, I think, is a satisfactory proof that those symptoms may be wide of direct and positive indications. As far as I have been able to judge, however, early lengthening or shortening of the limb is almost always the result of inclination of the pelvis. I have taken great care and trouble to determine this point in my own mind, by carefully measuring and comparing the two limbs, and I do not know that it has ever fallen to my lot to see a case of well-marked uncomplicated hip-joint disease, where there was a difference in the measurement, in which the lengthening or shortening did not depend upon the inclination of the pelvis. I am not speaking of those cases of advanced disease about the hip-joint, nor of cases of dislocation from disease. These highly-characteristic symptoms which are put prominently

forward as indicating disease of the hip-joint, or morbus coxarius, are not the early symptoms of hip-joint disease. When we see such symptoms, we may be sure that the disease in the joint has made a considerable progress. Now it seems to me that we should search for the local symptoms which precede the period referred to, for when these symptoms stand out so prominently and distinctly as to be recognizable by anybody, the local mischief must be great, and there can be then but little professional credit in "making out" the cause of the symptoms. It is highly important that the surgeon should recognise the diseased condition of the joint previous to that period, for that is the time when the most beneficial effects will be consequent upon a steady and long-pursued plan of treatment by rest.

If we succeed in diagnosing disease of the hip-joint early, I am quite confident that it will not fall to the lot of surgeons to see those sad and sometimes hideous cases which we so frequently observe, especially in hospital, and sometimes in private practice. It is, I think, a most condemnatory fault for surgeons to assume that there is nothing wrong in the hip-joint, unless some of those very conspicuous symptoms be present. The all-important point is the early recognition of the first deviation from a healthy state; and I should anxiously urge upon you, that even a suspected case of disease justifies a plan of treatment by rest, which, in my belief, would, in the majority of such doubtful cases, be the means of preventing the occurrence of the more formidable symptoms; and I would venture to affirm, that even if the more advanced and more formidable symptoms be displayed, still the case may be amenable to the influence of rest, and I hope to prove this by illustrative cases.

Referring to this hip-joint disease in children, let me say, it will be especially important for the surgeon to bear in mind that the acetabulum in a child is very shallow compared with that of an adult, and that it thus offers greater facilities for displacement; and I apprehend this may be the reason why dislocation of the thigh-bone occurs so frequently in cases of hip-joint disease at an early period of life. I do not know that that is the only element which determines this point, but I think it may be considered an influential one. I shall not dwell upon the anatomy of the hip-joint, except to remind you that its muscles perform their functions in groups, that each group has a trunk nerve of its own, and that each nerve contributes a branch to the hip-joint itself. You see a branch of the anterior crural nerve passing to the hip-joint; a branch of the obturator going to the capsular ligament, and to the ligamentum teres: and here is a branch from the sciatic and pudic nerves, the same nerve that supplies the gemelli, the quadratus femoris, and the obturator internus, proceeding to the posterior aspect of the hip-joint. This anatomy should be borne in mind, because it explains how it happens that the remote and sympathetic pains associated with an inflammatory condition or chronic disease of the hip-joint are not always found at the same part of the limb. We all know very well that one of the earliest symptoms, in some cases of hip-joint disease, is remote from the actual seat of mischief—namely, within the knee, or on the inner side of the knee-joint; and we are familiar with the explanation of it—namely, that the obturator nerve, which contributes a branch to the ligamentum teres, extends a branch to the interior of the knee-joint, to the inner side of it, and sometimes lower down than that. The inflammation or a diseased condition of this ligament necessarily involves the little branch of the obturator nerve, and sympathetic pain is produced at the other end of the same nerve, on the inner side of the knee or within the joint. As it is frequently with the obturator, it ought to be sometimes with respect to the other nerves of the hip-joint; but the frequency of this knee-pain, whether within the knee-joint or on its inner side, indicates the ligamentum teres to be the most common seat of early disease. We observe this same sympathetic pain in old persons, who may or may not have had a slight injury to the hip, but in whom the ligamentum teres softens down and disappears. Such persons often complain of severe pain within or on the inner side of the knee-joint. Some years ago I saw a case in which, after injury to the hip, this symptom of pain in the knee-

joint was well and early marked. Afterwards the limb was slightly shortened, and foot everted, imitating the reputed symptoms of fractured neck of the thigh-bone. I subsequently examined this patient's hip-joint, and found that the injury was confined to the ligamentum teres.

Now, suppose that the anterior part of the capsular ligament (which receives a branch from the anterior crural) is inflamed, you will then see, applying the same law, how it may happen that the patient with a diseased hip-joint may have pain on the outer side of the knee, or in front of the knee, or on the inner side of the ankle, because the anterior crural nerve sends branches to all those particular spots. Or if the inflammation or injury begins at the posterior part of the capsular ligament, which receives a branch or branches from the great sciatic nerve, then the patient may have a sympathetic pain actually at the heel or in the foot. I repeat these remarks in reference to the nervous supply, because an impression I think is abroad that the sympathetic pain of hip-joint disease is always on the inner side of the knee-joint, and that this local symptom is essential to a correct diagnosis. This, it seems to me, is not true. I admit its greater frequency, because, as I have already intimated, the ligamentum teres is perhaps the part where hip-joint disease actually begins most commonly, and that corresponds with the increased frequency of the sympathetic pain on the inner side or within the knee. To put this point more strongly: it sometimes happens in hip-joint disease that there is no pain in the knee-joint at all (I have seen several such cases); this local pain therefore must be considered as a fortuitous, not a constant, symptom, and to be relied upon as diagnostic or indicative of diseased hip-joint.

We ought further to bear in mind that the hip-joint lies very deep, and that therefore one of the earliest symptoms of an inflammatory condition—namely, a sense of heat in the part, is not likely to be recognised early in the disease except by a careful manipulation. There is no local symptom which characterizes the inflammatory condition of a hip-joint so certainly as the increase of temperature in and over the part inflamed. There is one of the symptoms on which every surgeon may positively rely when examining a suspected disease of any joint, provided he is able to make accurate manual examination of it, in reference to the existence or non-existence of an inflammatory condition; and when this local symptom of an increased heat exists, it is absolutely satisfactory. But you will observe it is one of the symptoms which you may not be able to reach in a very early stage of hip-joint disease, because the joint which is the seat of the inflammatory heat lies so deeply, and is covered at some parts by such a large mass of soft structure, that it is difficult for the hand to appreciate an increased temperature on the surface, but if, with the other symptoms of lameness and tenderness to which I will presently allude, you find an increased heat in the neighbourhood of the hip-joint, you may be sure that there exists a subjacent inflammatory condition.

Here, then, we see two sources of fallacy in diagnosis of hip-disease which have existed: one as to the position of the sympathetic pains, and the other as to the non-existence of heat. If we say that the sympathetic pain is always on the inner side of, or within the knee, and we are called to a case where it does not exist, of course it excludes from our consideration hip-joint disease. Or if we say that every inflamed joint is marked by an increase of heat, and we are called to a case where this does not exist (but I think it very rarely absent) of course that again would exclude hip-joint disease from our consideration.

There is another source of difficulty in diagnosis, which time will not allow me to dwell upon—namely, cerebral or spinal marrow disease, which may induce or cause some of the physical signs of hip-joint disease.

[Mr. Hilton observes that it frequently happens, that the hip-joint, when diseased, is not examined in the same exact, direct, and methodical manner employed in the examination of other joints. Many habitually depend upon or place too much reliance on what may be called the outlying symptoms.]

If we see a child with a slight degree of limping or lameness in walking, that must depend upon something. If a child is suffering from a slight degree of lameness in walking, and we recognise no indication of an inflammatory state by the increase of heat in the ankle or the knee-joint or the foot, and no pain at all on isolated or definite pressure by the hand at either of these parts, we may be certain that the mischief is not there, and we may be pretty confident that it will be most probably found at one of the pelvic joints, or in the immediate neighbourhood of the hip-joint, either within the articulation itself, or at the epiphyses of the bones; and possibly, on placing the hand attentively upon these different parts, the precise seat of the mischief may be detected by a more or less increase of temperature at that spot, and this symptom is usually, and more especially, manifested towards evening, and after walking exercise. This latter point may be deemed by some unworthy of notice; but it is one which we ought not to forget—that a slight inflammatory condition may manifest itself by local heat in the evening, after the exercise of the day, but by the rest of a few hours during the night it may have almost entirely disappeared in the morning. In hip-joint disease the femur is slightly bent upon the abdomen and somewhat adducted, and this occurs from the combined action of the flexors and adductors, which compel the thigh-bone to follow the mean direction of the two muscular forces. This is a constant law to which I have already alluded—that when a joint is inflamed, the moveable part of it is obedient to the more powerful muscular action. If, then, you see a child that limps a little in walking, the femur somewhat bent upon the abdomen, and the toe a little pointed, with inability to bear the whole weight of the body upon the limb, and when you make a little pressure over or upon the trochanter major, excessive or moderate pain is felt, as compared with the result of a like pressure upon the corresponding parts on the opposite side; if in association with these signs there be pain on pressure upon the front of the hip-joint, and pain in or near the hip-joint when the foot is struck upon its sole, and in addition to these local symptoms there be a slight degree of febrile excitement towards night, a little restlessness during the night, with occasional starting of the limbs, the suspected limb being more flexed and more adducted during sleep, (an almost constant occurrence,) then, I say, you may be sure that these symptoms are indicative of something wrong at or near the hip-joint itself. It will be remembered that at the early period of life to which I am now alluding, the bones forming the joint are composed of several parts. The trochanter major is an epiphysis; so is the trochanter minor; so likewise is the head of the femur an epiphysis, having a separate circulation, and joined to the neck of the femur by a temporary cartilage.

Now, to revert to hip-joint disease symptoms. If a patient presents such local and general symptoms as those to which I have alluded, it is hardly worth while to go into the question as to what tissue is involved in the mischief. That to my mind is a pathological refinement which is of no advantage whatever in the treatment of the case; and I think it is beyond the professional acumen of any man to be able to tell decidedly, at that period of childhood, whether the disease be between the neck of the bone and its epiphysis, the head, or whether it be in the ligamentum teres, or upon the surface of the head of the bone, or upon the floor of the acetabulum, which at that period is divided into three segments, indicating the separate contributions of those three individual parts—pubes, ischium, and ilium.

A little child, however, suffering from such so-called obscure or uncertain symptoms of hip disease, is likely to suffer from a misinterpretation of the symptoms, on the supposition that they may be caused by teething, or perhaps rheumatism; and these, I may say, are very common professional phantoms with the surgeon when considering such a case, so that all attention is given to the innocent teeth and gums, which are scarified and punished unfairly for sins which do not belong to them, whilst the hip-joint symptoms are allowed to continue, or are left to themselves, and thus to constitute what may be a fatal mistake. The other day I thought I would call and speak to Mr. Thos. Bell, as to what he had observed in his large

experience on this matter, and his opinion coincided with my own views as to this misplaced association of teething and hip disease. He added, "I wish you would also intimate that a very great deal of mischief is often done to the evolution of teeth by scarifying the gums too deeply." And it is obvious that it must be so. If the gums are scarified deeply, the rudimentary apparatus, which determines the development of the teeth either in their direction or integrity, must be very much interfered with by a reckless or badly-directed incision upon the gums.

I should have no hesitation, in a case presenting the hip-joint symptoms to which I have referred, in expressing my conviction that the femur or acetabulum has suffered local injury in some of its epiphyses, or that the soft parts of the hip-joint itself are inflamed; and that the essential, and probably the only required remedy would be sufficient mechanical rest secured to the patient by the recumbent position during several weeks. By persistent rest in such cases, you will surely anticipate the next series of more formidable symptoms which are said to be the characteristics of hip-joint disease. In cases presenting all these milder or less striking symptoms of hip-joint disease, I admit that it requires a strong determination and a strong conviction on the part of the surgeon to say to the parents, "This child has disease about the hip-joint, or symptoms which lead to the suspicion that the hip-joint is diseased, and that it is necessary that he should lie down for two or three months, with a splint upon the limb, to keep the joint quiet." But I know from experience, that we may act upon it with great and not unmerited confidence. It is a sound discretion, and it is the safest and best plan we can, under such circumstances, pursue. I feel strongly on this matter, because I am confident that if these early symptoms are properly made use of, and the surgeon will not wait for the next series, (said to be the characteristic symptoms,) which will be too conspicuous to everybody, he will not be troubled with long-continued cases of hip disease, nor will he meet with those old cases of deformed and displaced hip-joints as a result of unmitigated disease, to which I have before alluded.

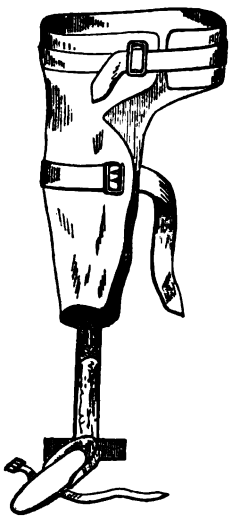
Suppose that time should show that you are wrong in your opinion regarding the disease in an obscure case, rest during one, two, or even three months, is not a very serious matter to the child. Assuredly, if you have given the proper advice, after the child has been lying down about a fortnight, many of those constitutional, and nearly all the local, disturbances which formed the early symptoms will have become much mitigated, and it will be then apparent that you have hit upon the right treatment of the case, if not the precise pathological anatomy or the exact locality of the disease.

I am anxious to inculcate the importance of recognising, or of diagnosing, a case of disease of the hip-joint or its neighbourhood at a period before the full manifestation of those symptoms which are spoken of and traced in books as the characteristic symptoms of morbus coxarius. I must admit it is still the opinion of some surgeons that inflammation or irritative disease of the hip-joint is sometimes associated with, or induced by unhealthy teething. I cannot say that it is not so, but I think it must be a comparatively rare occurrence, for it certainly has not fallen to my lot to see more than one or two cases which could give any legitimate support to such an interpretation. On the other hand, I certainly have seen a great number of cases in which teething has been said to be the cause of hip-joint symptoms, but has turned out to have nothing to do with them, and a great deal of time has been lost, and the patient has been placed in danger by the delay.

I wish now to detail shortly the case of a patient who had an incipient disease of the hip-joint, and who did remarkably well, I believe, in consequence of the recognition of the early symptoms to which I have directed your attention. It was the case of a boy whom I saw in my private practice. While dealing with this subject of hip-joint disease, I find myself obliged to refer to cases in private practice, because we rarely see these early cases in the hospitals. In truth, such cases are scarcely ever seen in public institutions until the symptoms of hip-joint disease have become exceedingly conspicuous and advancing towards suppuration; and I am trying to rivet atten-

tion upon the state of the patient antecedent to such severe conditions, for no doubt can be entertained that the earlier the cases are rightly interpreted, the fairer is the chance of recovery for the patient.

Case of disease in the shoulder-joint; the joint not kept at rest and the joint destroyed; hip-joint in the same patient cured by "rest."—This young gentleman was born in April, 1846. His mother was phthisical; she had cavities in one o both lungs when the child was begotten. In 1847, the child had inflammation or a diseased condition of the left shoulder-joint. The father took him to hospital surgeons who have now passed away, and who did not recognise this as simply an inflammatory condition, the probable effects of local injury, nor as a case to be cured by "rest." No, it was a scrofulous joint, and the shoulder was poulticed and the general health well attended to, as he had every possible advantage of good domestic care and change of air; but the arm, slightly supported by a handkerchief around his neck, was allowed to be used as a child would use it, without any positive restraint, except that suggested by pain. It ended in extensive suppuration, and in complete disorganisation of the shoulder-joint. The child became in very bad health, and there was a fair prospect of his dying from suppuration; and moreover, there appeared little or no chance of a good result for him, as he was thought to have inherited from his mother this scrofulous or fatal tubercular taint. I saw this patient professionally in 1848, and he then had all the symptoms of incipient hip disease to which I have alluded, and Mr. Key, in consultation, confirmed this opinion as to there being distinct hip-joint disease. The prospect for the patient was not good. The mother was dying, and did die, of consumption, and she had also hip-joint disease, with dislocation of the head of the femur, as the result of a very slight accident whilst travelling with her husband in North Wales. The child was very delicate and unhealthy-looking, with a thin, fine, silky skin. Suppuration was then proceeding abundantly from the shoulder-joint. In fact his health was going on from bad to worse, and every local system in the shoulder-joint indicated a decidedly bad reparative power. But be it remembered, no credit at all had been awarded to nature as regarded the shoulder; not a single chance had been given to her to repair the injury; no useful kind of mechanical rest had been prescribed; and the disease in the shoulder had been allowed to take its own course.



When I saw the patient, in 1848, he had disease of the left hip-joint. I had him placed upon a well-stuffed hair mattress, with a leathern splint upon his leg, thigh, and pelvis, like that here depicted; and I kept him lying there uninterruptedly for the space of six months—from September 1848 to March 29th, 1849,—with the left arm (the side diseased) fixed in a sling, but giving him permission to use the right arm as freely as he might wish, in order to amuse himself with his various playthings. The parents were constantly saying to me, "Let him get up; lying in bed will make him so weak, and his general health will suffer," and I as constantly replying, "No, no, his health is improving," and insisting upon the child remaining absolutely quiet for at least six months. The child was then allowed to get up, and to be carried about out of doors, but the splint was not taken off until the end of August, eleven months from the time it was first applied. At that time the whole of the hip-joint symptoms had entirely disappeared, the general health was good, and the shoulder was also greatly improved and approaching cure by anchylosis, yet discharging now and then little fragments of cancellated bone. The hip-joint was movable, painless, and would bear the weight of the body in standing without inconvenience; so that in truth, in the same person, with

the same constitution, the shoulder-joint had been destroyed and the hip-joint was saved.

What was the cause of the difference in the actual condition of the two joints in this case? Just this, I apprehend: that with the shoulder, Nature never had a chance of curing it by rest; and with the hip disease she had every opportunity offered to her, and she did not fail to make good use of it. With one joint the treatment adopted was ineffectual, and in the other perfectly successful. In 1850 the shoulder was still suppurating, still discharging a little thin pus and very minute portions of bone, and there was a large open ulcer at the posterior part of the joint. In 1854 I had a leathern splint placed upon his shoulder and upper arm, which he wore for two years, and at the expiration of that time the joint had got perfectly well. I saw the patient early in June, 1858, and the hip-joint was then in a perfect condition; the left shoulder joint was completely anchylosed, and the anchylosis, with its remote effects, showed themselves in this way: the humerus and scapula moved rigidly together, and, in addition to the rigidity of that joint, the clavicle was short as compared with the other side, and the chest on the left or shoulder-disease side was not so much developed as on the other side. Time will not allow me to dwell on the other details; but I must express my conviction that if the surgeon who first saw this child's shoulder had acted upon the idea that it was a diseased joint from simple inflammation, the result probably of injury, and had given Nature credit for being very active in the reparative as well as in the formative process at that period of life, and had kept the shoulder perfectly quiet for some weeks, the shoulder-joint would have been saved as satisfactorily as was the hip-joint.

To disencumber this case of its details, it is just this: Here was a child who had manifested by his shoulder-joint a scrofulous taint, if you like to call it so (although I am not a believer in scrofula to the extent that many people are), whose mother died scrofulous and from pulmonary tubercular phthisis and hip disease; yet by giving this child's hip-joint rest for a long time, in spite of these constitutional and inherited difficulties, *he perfectly recovers* from this affection of the hip-joint, because it was recognised early enough to enable Nature to repair the injury which the child had sustained.

Notwithstanding disease of the hip-joint may have advanced to that period when the symptoms of hip disease are conspicuously distinct, and notwithstanding a very bad constitution, and the destruction of the soft parts of the joint, patients may get perfectly well by rest, with an anchylosed hip-joint. This would appear at once to negative the idea of there being necessarily a scrofulous condition of the bones in such cases.

The notes of the next case to which I shall allude are also taken from my private practice.

Case of hip-joint disease of one year's duration cured in seven months by rest.—In January, 1855, the patient, (a little girl between four and five years old,) lame, and in great pain, was taken to a surgeon, who pronounced that she had hip-joint disease, and that he regarded it as scrofulous. She was under his professional care about eight months. He directed her to be taken to the sea-side, to have medicine, and to move about or take exercise, that her general health might be improved, thus hoping to cure the case by invigorating the constitution; but no direct rest was given to the hip-joint. The local and general disturbance and distress increased so as to become excessively severe. In December, 1855, eleven or twelve months after the appearance of the first symptom, she was brought to my house. She could not bear the pain of being very gently and carefully carried in the arms of her mother. The child was much wasted, and a picture of really extreme bodily distress, screaming with pain nearly all the time she was with me; and her mother informed me that she had had scarcely any sleep during many weeks from the pain in the hip and the outer side of the knee, these pains being especially severe at night. The thigh was flexed upon the abdomen, adducted, and the knee turned inwards, evincing a disposition of the head of the femur to be dislocated backwards; there were also fulness and rotundity at the back of the joint. The slightest pressure upon the trochanter major or

upon the sole of the foot, for the purpose of forcing the head of the femur into the acetabulum, caused fearful agony. There was marked disposition of the head of the bone to be displaced backwards, (always a serious complication,) and the distinct fluctuation of an abscess below Poupart's ligament to the outer side of the hip-joint. The patient was taken home with great care, and, by my direction, Mr. H. Bigg, the same evening, went to the house, and moulded this leathern splint, or case, upon the pelvis and upon the hip, with an extension to the foot for the purpose of keeping the hip-joint and the whole leg at rest. Great pain and distress were experienced by the patient whilst the limb was being straightened, but there was no alternative, and it was done. In adapting a splint to a case of hip-joint disease, it is important not only to prevent any movement of the joint, but also to ensure easy, persistent contact between the articular surfaces without pressure, so as to facilitate bony union, supposing the soft parts to be destroyed; or if the soft parts be simply inflamed and swollen, then to prevent undue pressure of the soft parts, mutually inflamed, upon each other. Another object in this case was to prevent rotation of the limb either inwards or outwards, because it is obvious that if no such rotation be permitted, no dislocation can occur. All the requirements were carried out in this patient by the splint you see before you. It was accurately adapted, and answered remarkably well. You will observe that a sliding footpiece is attached, so as not to interfere with the growth or elongation of the limb. Children grow very rapidly, especially so if well fed, when in the recumbent and supine position; and if the footpiece of the splint be fixed, there will be a constant contention, if we may so term it, between the restraint on the part of the splint and the changes incident to growth; so that the footpiece should be allowed to elongate itself as the child grows. The splint is also provided with a transverse portion under the footpiece, which prevents the possibility of any rotation either inwards or outwards. The patient was placed on her back upon a well-stuffed hair-mattress bed, and this splint was kept on uninterruptedly, or nearly so, during seven months. It was taken off three or four times by myself with care, merely for the purpose of examining the progress of the cure, and of facilitating personal cleanliness. During the latter portion of the time she was taken out of doors, with the leathern case, or splint, on, and lying horizontally upon a little wheeled carriage, with easy springs. The medical treatment consisted simply of opium, in large doses at first, to secure sleep, and one sixteenth of a grain of bichloride of mercury twice a day, with sarsaparilla, during about a couple of months; no seton, no issue, no tartar emetic ointment, no croton oil liniments, nothing of the kind. The general health and appetite improved rapidly after the first month of confinement to bed. At the expiration of the seven months, all pain, tenderness, and constitutional disturbance having disappeared, she was soon allowed to take gentle and well-watched walking exercise, without harm. The abscess in the thigh gradually subsided, and was finally absorbed. I saw the patient again in about sixteen or eighteen months from the time that she began to take independent exercise. She walked into my room quite comfortably, well in health, but rather flat-footed. She could then walk two miles without any difficulty or pain. The muscles of the thigh and leg were increasing in size almost weekly. There was a slight limp in the walk, the foot very little everted, and there was, perhaps, from about a quarter to half an inch shortening of that limb as compared with the other; there was no abscess to be felt, no fluctuation detectable anywhere, and the bony anchylosis was perfect. For the purpose of testing this latter point of bony union, I placed the child recumbent upon a couch, and, pressing downwards upon the pelvis, endeavoured to flex the thigh-bone upon the pelvis, but I could not carry the thigh-bone towards the abdomen without lifting the pelvis at the same time; the other lower limb was perfectly natural in all its movements. I might suggest that the slight shortness of the diseased limb was due to the arrest of growth on that side while the other side was growing. This case, then, proves that although hip-joint disease may have existed uncontrolled during nearly a twelvemonth, yet that by rest alone the case is perfectly curable, with a stiff joint, which may be styled the best possible re-

sult in such a condition of joint; and the case shows also, I think, that there was no scrofulous condition, no tuberculous state of bones; for I cannot believe that if those bones had been scrofulous, we should have obtained such a rapid and perfect anchylosis, or bony union. I had intended making some remarks on this case in reference to the absorption of abscesses, but the time will not permit.—*Lancet, Nov.* 23, *Dec.* 7 *and* 14, 1861, *pp.* 491, 539, 563.

55.—ON THE STRAIGHTENING OF CONTRACTED JOINTS.

By James Paget, Esq., Surgeon to St. Bartholomew's Hospital.

[The following are notes taken of a clinical lecture delivered at the Hospital. Mr. Paget adverted to the practice of straightening stiffened joints, the patient being under the influence of choloroform.]

There were, he said, two classes of cases. In one, this method was adopted to rectify deformity; in the other, to prevent it—in the latter, when the disease was still in progress; and in the former, when the effects only remained. We should, however, be careful not to attempt this in cases in which there are signs of acute inflammation, nor in cases in which the patients had been found liable to recurring attacks of inflammation. In cases, also, in which the joint contains fluid, or in which the skin is firmly bound down over it, this plan should not be tried. In those in which active disease has terminated, there might be distinguished two kinds of cases: First, those in which stiffness depends on effusion and adhesion of the ligaments and tendons external to the synovial membrane. Of this a good example was then in Darker Ward,—one in which the success of the treatment was well shown. The other and more common class of cases was those in which changes had taken place in the true joint itself. In these cases it was true that gradual extension would obtain the same end as the forcible extension under chloroform; but the great length of time taken up, (months, or even years,) was a great drawback to it.

The cases in which forcible extension should not be tried, Mr. Paget recapitulated, were: 1. Those in which the inflammation was apt to recur. 2. When the change had been of very long standing. 3. When there was a strongly-marked diathesis, (strumous, syphilitic, &c.) 4. When the skin was involved and adherent over the joint,—soundness of skin being an essential condition.

There were other cases in which, from rigid muscles, a joint would be contracted, there being no affection of the joint itself. This occurred after a blow on a joint, which, however, did not produce inflammation, or in cases where the bone near a joint was fractured, without, however, producing any disease of it. In these cases, under chloroform, the contractions were reducible almost by simple extension, no force being required.—*Med. Times and Gazette, Feb.* 15, 1862, *p.* 157.

56.—TREATMENT OF "HOUSEMAID'S KNEE."

By Richard W. Martyn, Esq., Martock.

[Mr. Skey, in a lecture lately given at St. Bartholomew's, recommended the treating of these cases by passing a seton through the tumour, and states that he has cured many hundred patients in that manner. Mr. Martyn says:]

For the last forty years I have never met with a case that I could not cure in the simplest manner, without the least pain, confinement, or danger, the patient doing her usual work, except kneeling. I merely apply a simple plaster and bandage in the manner Scott recommends. I have always found a fortnight sufficient for the cure; and, perhaps, two applications will be necessary.

The following is the plaster: Cut a piece of leather of sufficient size to cover the knee above and below the joint, and to nearly meet behind; spread

this with emplastrum plumbi. Cut some white curd soap into thin shavings, and about the same weight of emplastrum plumbi; add about a teaspoonful of olive oil. Melt together over a slow fire. Take half an ounce of muriate of ammonia, (commonly called sal ammoniac;) pound this in an earthenware mortar as fine as you can. Pour the melted soap plaster into the middle of the plaster spread on leather so that it may cover the upper surface of the knee; let it be nearly an inch thick. Now while it is liquid and hot, mix into it the powdered muriate of ammonia, and apply it as warm as the patient can bear. Apply a roller *secundem artem.—British Med. Journal, Nov.* 23, 1861, *p.* 565.

57.—ON A NOVEL AND EFFICIENT METHOD OF REDUCTION IN DISLOCATION OF THE SHOULDER-JOINT.

By Professor N. R. Smith.

After stating that the real difficulty in reducing dislocation of the shoulder is the production of immobility of the scapula, and that this is insufficiently provided for by several of the means in use, Dr. Smith describes the procedure which he has himself had recourse to for many years past.

"In effecting counter-extension, it is undoubtedly in most instances expedient to apply our resisting bands as directly as possible to the bone from which the other is dislocated. But the difficulty in this case is to effect it without defeating the object in the manner indicated above. On noticing the mechanical relations of the scapulæ with their apparatus of muscles to each other, it occurred to me to make counter-extension from the opposite wrist. Anteriorly the two acromion processes are bound together by an unyielding chain of bone and ligament. The two clavicles, the sternum, and the intraclavicular ligament, chiefly constitute this band of union. The fibrous, resistant ligaments in this chain are not capable of being stretched; and if traction be made from opposite wrists, the two acromion processes, thus tied together, are not capable of being drawn asunder to the extent of half an inch. Posteriorly, the continuity of resisting parts is almost as perfect. . . . . The object of counter-extension is to prevent the yielding of the scapula to the tractive force. Nothing does this so effectually as the *fixing of the opposite scapula* by counter-extension at the wrist. Not only is the scapula thus sustained but the spine is erected and prevented from yielding to the tractive force, and becoming carried to the injured side, as invariably happens when counter-extension is chiefly made from the axilla, as usual. . . . . In some of the cases in which I first employed this method, I directed simply traction from the two wrists, and I am not now confident that this is not the best method. I placed the patient in a chair, and directed two strong persons to make steady horizontal traction from the two wrists. As soon as the spasmodic resistance of the muscles was overcome, the head of the bone was disengaged, and the muscles which help us in such cases suddenly lifted the head into its place. It will be observed that this means causes no appreciable pain, but rather relieves the suffering of the patient caused by the pressure of the head of the humerus. In cases in which, from unusual muscular development, or the age of the dislocation, much resistance is expected, I have modified the application of counter-extension thus: I place the patient on a chair, sitting a little on one side of it, so as to allow room on the side of the injury for the operator's foot. I then pass a piece of stout muslin, folded, around the chest and under the axilla of the injured side. The tails of this I carry horizontally to the opposite side—one in front, the other behind—and extending the arm horizontally, bandage them firmly to the wrist on the sound side, leaving the ends projecting, to be well secured to the wall, or other unyielding substance. I then pass an ordinary roller over the top of the injured shoulder, and back and forth twice under the muslin band, to prevent its slipping down. Then I continue the same roller under the bottom of the chair and over the shoulder three or four times. This helps to give steadiness to the scapula, and especially prevents the involutary rising of the

patient, or the tilting of the scapula upwards, when it is necessary to make the manipulation of which I am about to speak. I first apply a wet roller to the wrist of the injured side, and then attach a muslin extending band by the clove hitch. Next, I direct extension to be made by two persons, at first outwards and a little downwards, gradually raising the arms to the horizontal direction, and finally a little above it. The extension must be made gently and steadily, gradually increasing the force so as to not provoke the muscles to spasmodic resistance. As no pain is produced by the force thus employed, it may be continued for a considerable time. The muscles, which at first resist, become fatigued, and finally relaxed; and in a large majority of instances of recent luxation, the head will slip into place without resort to any manipulation. I would even continue this traction, where much resistance is encountered, for a quarter of an hour before modifying the force; but in case the object is not then effected, let the surgeon place his foot on the margin of the chair and his knee in the axilla. Then let the assistants raise their line of traction above the horizontal as much as possible, and continue it for a moment. The surgeon should then direct that the arm be, by a sudden movement, carried downwards, while, by extending his foot, he elevates the knee in the axilla. He aids the assistants in this by grasping the arm near the elbow and using it as a lever. If the first effort be not successful, he should repeat it. Sometimes I place on the knee a ball made by rolling up a bandage, but it is not important. The surgeon can thrust his knee into the axilla, so as to avoid much pressure on its marginal muscles. I generally direct those who make the traction to sway the limb horizontally backward and forward, and, grasping it with my hands, at times I rotate it a little on its axis, thereby contributing to the disengagement of the head of the bone. . . . . The supine position of the patient on a table is a very convenient arrangement in all dislocations of the shoulder, and is well calculated to promote the efficiency of the method which I recommend. I am not sure that it is not the very best, especially when chloroform is employed. . . . . I will here state that I have by this mode repeatedly reduced dislocation of the shoulder which was two months old, and once when three months had elapsed. In cases in which I expect great resistance I commonly employ chloroform. The effect of this agent must, however, be rendered very complete, for its incomplete use causes spastic rigidity of the muscles and defeats the object.—*British and Foreign Med. Chir. Review, Jan.* 1862, *p.* 257.

58.—ON THE TREATMENT OF LATERAL CURVATURE OF THE SPINE.

By William Adams, Esq., Surgeon to the Royal Orthopœdic and the Great Northern Hospitals.

A combination of lying down to the extent of four or six hours a day, together with light gymnastic exercise, is the treatment best adapted to such cases at the commencement of the spinal affection, when curvature is threatened rather than positively formed; but if curvature has actually taken place, then mechanical support must be combined with the partial recumbency and light gymnastic exercises.

The Constitutional Treatment in these cases must be regarded as at least of equal importance with the local treatment, and every effort made to improve the general powers of the patient. For this purpose, I have found the free exhibition of the hypophosphite of lime, and preparations of iron the most useful. Generally I direct five grains of the lime-to be dissolved in a wineglassful of water, to which ten drops of the tinct. ferri sesquich. may be added, and taken twice a day, at, or directly after meal times. In addition, also, I give half-an-ounce of cod-liver oil, to be taken twice a day, an hour after meal times, and recommend the use of wine or beer, as the patient may prefer. The influence of soil and place of residence is also of the greatest importance, and, where practical, I advise the removal from large towns,

damp situations, and clay soils, to high and dry situations, on a chalk soil. The great improvement in the general health and strength of delicate children and growing girls with spinal curvature, which I have seen follow a change of residence from such counties as Lincolnshire, Cornwall, Devonshire, &c., to high and dry situations on chalk hills or downs, has led me to recommend such a change of residence in these cases where practicable. To these measures we may also add the use of the cold bath where it can be borne, followed by frictions to the back, and free out-door exercise.

The Local Treatment of lateral curvature of the spine is that to which the attention of the profession has been generally—I might almost say exclusively—directed; and at different periods various methods, or so-called *systems of treatment*, have sprung into existence, through the advocacy of those who have specially devoted their attention to the treatment of this class of affections; and each *system* in its turn has more or less completely enjoyed the support of public opinion, and the sanction of the profession.

I have already enumerated the most important of these systems, or those which have received the greatest weight of authority, and will now again advert to them in reference to the anatomical and pathological principles upon which they are based, and examine how far these are compatible with the views which I have brought forward in reference to the anatomical construction and mechanism of motion of the spinal column, and the pathological conditions which I believe will be found to exist in every case of lateral curvature, however slight. We have, then:—

1st. The system of *complete recumbency.*

2nd. The system of *muscular exercises, or gymnastics.*

3rd. The system of *cutting the spinal muscles.*

4th. The system of *mechanical extension.*

5th. The sytem of *mechanical treatment by support and pressure*, applied by means of spinal instruments.

All these different systems, acting for the most part on principles diametrically opposed to each other, are reported to have been in every way successful and satisfactory in effecting the cure of lateral curvature of the spine; and the perusal of the various works or treatises in which these systems have been advocated, serves only to impress one with the conviction that lateral curvature of the spine has even at the present time no fixed pathology; and therefore no fixed principles of treatment have as yet been determined.

1st. The system of *complete recumbency*, by which patients used to be condemned to constant lying down for at least one or two years, and frequently for a much longer period, has had many advocates in this country; but its injurious effects upon the general health, as well as its complete failure to cure the spinal curvature when severe, has led to its discontinuance as a system; and its admitted disadvantages have been so loudly proclaimed by the advocates of other systems, and more especially by those who profess to cure even severe cases of spinal curvature by means of spinal instruments, that the advantages which undoubtedly belong to recumbency when judiciously employed, and combined with other means, have been to a great extent lost to the public of late years, and the treatment of spinal curvature proportionably less successful than it might have been if the views taken of its pathology and treatment had been upon a more enlarged and liberal scale.

The great object of recumbency is to relieve the spine from the superincumbent weight of the head and upper extremities, which it is obviously unable to support when curved or even disposed to curvature; and as the immediate cause of curvature of the spine is undoubtedly mechanical in its nature and action, viz., the irregular distribution of the weight of the head, and upper extremities, though this again may depend, as it generally does, upon a combination of local and constitutional causes, yet the immediate object is most completely effected by the patient assuming the recumbent position. All the mechanical conditions upon which lateral curvature so much depends are thus removed, and the patient, during recumbency, relies upon her constitutional strength and powers of development and growth, which are, in many instances, sufficient, during the period of youth, to lead

to the complete reparation of such structural damage as the intervertebral cartilages and oblique articulating processes may have sustained when the curvature is slight and of recent formation. I regard lying down, therefore, during the period of active growth, as essentially a curative means in cases of spinal curvature, and whilst I reject it as a system to be relied upon exclusively in any class of cases, I recommend its adoption in every case during the period of growth, in conjunction with such other means as may be thought advisable; in some instances with gymnastic exercises, and in others with mechanical support, occasionally also in conjunction with both these methods of treatment; and here I would observe that there is no greater error than to suppose that these different methods, or so-called systems of treatment are essentially incompatible with each other. Their objects, it is true, are very different, yet both theory and practice teach us that in many instances they may be most advantageously combined for the benefit of the patient. As a general rule, I advise recumbency in proportion as the spinal curvature predominates in the lumbar region, in which situation it must be admitted that mechanical support can be of little use; here there are no ribs against which pressure can be exerted, and no one will contend that the transverse processes are adapted to receive mechanical pressure.

With regard to the extent to which I recommend lying down, I would observe that in ordinary cases I advise recumbency at least during four or six hours a-day; and this can be borne with comfort and without any disadvantage to the general health, if the patient use the reclining chair.

The inclination of this chair can be varied to any angle from that of an upright chair to a horizontal couch, but the angle of 45°, or rather more, is that best adapted to long-continued recumbency for spinal cases. In this position the weight of the head and upper extremities is effectually removed from the spinal column; and the patient does not suffer any of the inconveniences, such as headache, indigestion, &c., so frequently attendant upon lying down on the *inclined plane*, or lying flat upon the floor, or upon boards, which formerly used to be recommended. The addition of arms and a reading desk to the chair adds materially to the comfort of the patient, and allows education in its more important branches to proceed without interruption. This chair has been made for me for several years by Mr. Ward of Leicester-square. Writing, drawing, and music must be diminished in the education of young ladies under treatment for spinal curvature, as the long continuance either of the sitting or standing positions is that which we have more especially to avoid.

2nd. *The system of muscular exercises, or gymnastics*, as a curative means in the treatment of lateral curvature of the spine, has been relied upon with more confidence by Continental than English Surgeons; and according to the statements made by the leading Professors of this system, lateral curvature is certainly cured when the deviation is slight, and as certainly arrested when more severe; hence it is assumed to be at least equal, if not superior, to other methods of treatment. The pathological idea upon which this system is based is that lateral curvature essentially depends upon muscular debility, and the argument therefore is—if muscular debility be the cause, will it not be desirable to strengthen the muscles by means of a proper system of gymnastic exercises, rather than apply mechanical support, which must, by superseding the necessity for muscular effort, tend to weaken the muscles to a corresponding extent? or than to adopt a system of lying down, which would still further debilitate both the system and the spinal muscles. The argument is logical and attractive enough both to the public and the Profession, who generally adopt the muscular-debility theory of the production of lateral curvature; but there can be no doubt that this theory can only apply to a limited number of cases. I have already stated that certainly less than half of all the cases of lateral curvature which have passed under my observation, have been neither preceded nor accompanied by any muscular debility, and that even where muscular debility has existed, other causes, both constitutional and local, have also existed, and according to the views of the mode of production and general pathology of lateral curvature which

I have adopted, appear to me to be of not less importance than the condition of muscular debility.

Let it be admitted, however, that in many cases an increase of muscular strength is desirable, and the question which naturally follows is—can this be so obtained as directly to cure, or to contribute to the cure of lateral curvature of the spine?—If in any case of lateral curvature, all the muscles of the spine could be at once raised to their maximum of power, the only effect could possibly be to arrest the curvature at that point, and we frequently see cases of moderate severity undergo spontaneous arrest where the general health and muscular power are improved, though I believe this only occurs when the mechanical conditions of the curvature—more especially the equality in length of the dorsal and lumbar curves—are favourable to such a result; but in such cases no advance is made towards straightening the crooked spine, which must be considered the main object of treatment. A general increase of muscular strength being thus considered useless as a curative means, it may be asked, Cannot we by special muscular exercises develope only the muscles which would tend to correct the spinal curvature? By the gymnastic professors it will doubtless be contended that we can, but I have no confidence in the curative effects of any system of special muscular exercises, whereby the muscles on one side of the spine are sought to be particularly developed. I believe such a result to be practically impossible for two reasons, 1st. The anatomical difficulty, arising from the number and complex arrangement of the spinal muscles, of stating precisely in any given case the exact muscles and portion of muscles, the increased power of which would act beneficially on the curvature. 2nd. The impossibility of strengthening these particular muscles and portions of muscles, if they could be selected, without at the same time strengthening other muscles the increased action of which would at least neutralize their effect.

Another, and I believe a fatal objection to the pretensions of the muscular exercise or gymnastic system is, that it rests upon the idea of free motion, or surface-play being allowed at the articulations of the vertebræ, and an absence of structural changes in the articular surfaces; in fact upon the idea of the anatomical analogy between the articulations of the vertebræ, and the moveable articulations of the extremities, and consequently upon the idea of an analogy in the pathological conditions attending the distortions of the spine and those of the limbs, whereas I have endeavoured to prove that there is a complete absence of any analogy between the vertebral articulations and the moveable articulations of the extremities; and that no curvature, however slight, can exist without structural changes affecting the intervertebral cartilages and the oblique articulating processes in a degree proportionate to the curvature.

The muscular exercise, or gymnastic system, must therefore be rejected as a system capable of curing lateral curvature of the spine; but nevertheless when used judiciously, and in conjunction with other means, in some cases of slight lateral curvature, associated with muscular debility, there can be no doubt of the great value of a well-regulated system of gymnastic exercises. In cases of *weak spine* which threaten to pass into actual curvature, cases which I sometimes speak of as *threatened curvature*, occurring in girls from twelve to sixteen years of age, gymnastic exercises are invaluable, and in conjunction with the use of the reclining chair for about four hours a-day, may be confidently relied upon, as the best *preventive* means at our command, the general health at the same time being improved by such constitutional means as may appear to be indicated. In cases of slight curvature also, I employ gymnastics in combination with partial recumbency and mechanical support by the lightest form of spinal instrument, such as the Eagland belt, and now from long experience I am enabled to state that this combination of means contributes to a favourable termination much more certainly than the employment of either system alone. The gymnastic exercises which appear to me to be the best adapted for strengthening the spinal muscles equally on both sides are those used by means of the *hand-swing, the elastic chest expander, and the act of drawing up a weight from the ground*, which may be

imitated by a short and strong cord of vulcanized india-rubber attached to the ground. This is the movement of the *top sawyer* at the saw-pit. More varied exercises may also be employed, but essentially these appear to me to be the most useful.

3rd. The system of *cutting the spinal muscles*. In direct opposition to the idea of muscular debility and the treatment by gymnastic exercises, we have the theory of M. Guérin, that lateral curvature of the spine is produced by the active or tonic contraction of some of the spinal muscles, in the same way that club-foot, wry-neck, and various contractions of the limbs are known to be frequently caused; and upon this pathological idea M. Guérin has recommended, and for many years extensively adopted, the practice of dividing the spinal muscles. The operation, however, I am happy to say, has found but few advocates in France, and still fewer in England. I have already discussed at some length, and I hope have succeeded in showing, the fallacies upon which this theory is based. It is unnecessary, therefore, for me to say more in reference to the practice arising out of it, viz., the section of the spinal muscles, than that I entirely disapprove of the operation, and regard it essentially as an unsound and unscientific procedure; and that I do not believe there are any cases of lateral curvature in which such an operation can ever be called for.

4th. The system of *mechanical extension*. The idea of straightening a crooked spine by the process of stretching it, is of ancient date; and the system of mechanical extension, employed both in the erect and the recumbent positions, has been very generally adopted throughout the continent, and is still in vogue to some extent. In England, also, the practice was in favour twenty years ago, but never so generally adopted as in France and Germany. I have never seen the *extension plan* in operation, but not long since I saw in a private establishment near Portman-square, several *extension couches*, and also a sort of hanging contrivance, by which people were drawn up by the head, off the ground, and allowed to remain suspended in the air for a certain time. I was informed, however, that these things were not used in the treatment at present adopted, which was essentially gymnastic. The couches were divided transversely in the centre, and the two halves could be separated by machinery when the patient was fixed on to it by straps, &c., the head and upper extremities being fixed to one half and the legs to the other. Our anatomical knowledge at the present time, and a momentary consideration of the series of adapted structural changes which we know takes place in all cases of confirmed lateral curvature, render it unnecessary for us to discuss any pretended merits assumed for the *mechanical extension* system.

5th. The system of *mechanical treatment* by means of spinal instruments. The idea of straightening a curved spine by means of mechanical contrivances fixed on the body, and constantly worn by the patient, without the necessity of lying down, and by what are described as *portable instruments*, acting on the same principle as we now adopt in straightening a bent knee-joint, for instance, is of modern invention, and may be described as the English orthopædic treatment of the present day, since it is the plan which has been advocated and adopted by the majority of the English orthopædic authorties during the last twenty years to the exclusion—and it may be feared injuriously—of all other methods of treatment.

The English public, as well as the profession, had seen the very satisfactory condition of the treatment of lateral curvature of the spine by the *system of recumbency*; and the general advance in the construction of orthopædic instruments for the cure of club foot, &c., led gradually to the improvement of such portable contrivances for supporting the spine as had been previously employed by Tavernier and others.

Time will not allow of my discussing the merits of the various instruments now in use; they vary considerably in their principles of construction as well as in the mechanical details, and the kind of apparatus to be used in any particular case must depend upon the form and situation of the curve; and also upon the age of the patient. In delicate girls with slight curvature I find

the lightest form of instrument such as that well known as the *Eagland belt* quite sufficient, and this instrument has been strengthened and otherwise improved in its construction for me by Mr. Blaise, of St. James'-street, but in many cases a much more powerful instrument is necessary, and a second plate must be added.

There can be no doubt that the greatest advance which has taken place in modern times in the treatment of lateral curvature of the spine has been made through the *system of mechanical treatment* by means of spinal instruments; but as might have been anticipated from the novelty and importance of the system, too much has been expected from it, and also too much claimed for it by its principal advocates. Compared with other systems, there can be no question as to the superiority of the mechanical treatment, and if one system alone was to be adopted, this would be most entitled to our confidence; but the evidence which I have laid before you of the constitutional as well as the local causes which contribute to the production of spinal curvature, and the nature of the structural changes which occur even in slight cases of lateral curvature, must prevent our placing sole and implicit confidence in the power of any spinal instrument to cure the curvature. I have no hesitation in stating that it is impossible to straighten a curved spine by any mechanical means in the same manner and upon the same principle as we can straighten a bent knee, ankle, or any other moveable articulation; but in combination with other means, such as partial recumbency, and in some slight cases with muscular exercises, it is quite possible to cure a curvature of the spine when slight and of recent formation in young persons; and it is certainly within our power most effectually to arrest the progress of curvature when more severe.—*Med. Times and Gazette, Dec.* 28, 1861, *p.* 657.

59.—ON CERTAIN GRAVE EVILS ATTENDING TENOTOMY, AND ON A NEW METHOD OF CURING DEFORMITIES IN THE FEET.

By Richard Barwell, Esq.

The author, in a paper read before the Royal Medical and Chirurgical Society, remarked that the purely mechanical treatment of club-foot, which had, since 1832, been superseded by tenotomy, could as a rule deal very successfully with those lateral twists of the extremity which are called varus and valgus, but that the equinal deformity could hardly ever be cured by these means. This peculiarity is attributable to the great power of the muscles attached to the tendo-Achillis; for contracted muscles may be lengthened with an ease which is, *cæteris paribus*, in direct ratio with their power. At the present time the treatment of pes equinus is, above all others, simple and satisfactory, because division of the tendo-Achillis gives the means of lengthening this part in a few days. In a similar manner, though in different degrees, the treatment of all other deformities in which extension mingles, is assisted by this section. The inward twist of the foot—varus—is now treated by division of four tendons at least, (tibialis posticus and anticus, flexor longus digitorum, and the tendo-Achillis,) and frequently also of the plantar fascia. A very similar treatment is inflicted on valgus. "The peronei tendons are to be divided, or, together with them, those of the extensor longus digitorum, and the tendo-Achillis, and also those of the tibialis anticus, and extensor proprius pollicis, when they are retracted." Can we be surprised that after this there should be, as admitted by the above-quoted orthopædist, "difficulty in continuing sufficient support to the arch of the foot? and even after the arch has been perfectly restored, support is required during many months." But if the deformity have arisen, not from spasm in the predominant muscles, but from debility of their antagonists, division of the still active tendons produces disastrous consequences. Mr. Barwell was led, four years ago, when examining some old cases of tenotomized feet, whose actions were clumsy and ill performed, to doubt the desirability of dividing several other tendons as unhesitatingly as above described; and he thinks himself now in a posi-

tion to explain the dangers and evils resulting from such practice. In 1842, M. Bouvier reported to the Académie Royale several experiments performed on the tendons about the feet of dogs, and one on a horse. In no one case did these tendons unite so as to be of any subsequent value. Moreover, there are examples on the human subject in a recent work on the "Reparative Process in Human Tendons." Mr. Adams has collected together all the cases he could get at of post-mortem examination after tenotomy. Amongst these there are seven in which other tendons besides the Achilles were divided, and in every one of them one or more of the several tendons is either not united at all, or has become attached to the bone or surrounding parts, so as to be of no possible use hereafter. The evident and sole conclusion from them is, that such muscles as the tibialis posticus, and flexor longus digitorum, might as well be struck by sudden and irremediable paralysis as be subjected to the knife of the tenotomist, and that the tibialis anticus is only a little better situated. Probably the peronei are quite as exposed to non or false union as the two first-named muscles, since their situation behind bones is analogous. Thus, to divide these tendons is to produce lameness, perhaps less apparent, but more incurable, than that for which the operation was originally performed. Mr. Barwell then proceeded to question the necessity of dividing those tendons in order to restore the shape of the foot, quoting Dr. Little's opinion, that indiscriminate section was the rule, even with regard to the tendo-Achillis, and giving his own experience, that division of the other tendons was hardly ever necessary. In support of this view, he mentioned another curious result of the above-named autopsies—namely, that in one case an attempt had been made to divide the posterior tibial tendon, and the limb came so easily into shape that it was thought to have been divided, but after death was found to have remained whole. With all these facts before us, the author said it was our duty to avoid dividing tendons about the foot, and to return to mechanical treatment. The mechanical shoes of the best form receive the foot, and then act simply by twisting it in a direction contrary to the abnormal bend. In such contrivances no heed is taken of the condition of the muscles, nor of their direction, and they are all such as greatly prevent the power of walking. The author makes the following propositions: The balance of the foot is kept up by a set of muscles which antagonize one another. By abnormal predominance of any one set, deformity is produced. The predominance may be from excessive contraction of the prevalent muscles, or deficient action of their antagonists. In either case, our efforts should be directed to counteract the predominant force by applying a power in the direction of and as much like the deficient muscle or muscles as possible, and, at the same time, the foot must be allowed its use. These indications are thus carried out: A broad and long piece of strapping-plaster, spread on a thick material, is made to adhere over the origin of the muscle and along its course. Upon this is laid a piece of sheet tin, long enough to reach from the upper part of the leg to just above the ankle, and carrying at its upper part a wire loop; the piece of strapping is turned back over it, and lies with its adhesive side outwards. Another piece of strapping is placed on the foot upon the insertion and along the course of the tendon; the end of this strapping reaches to the bend of the ankle, and is folded over (the two sticky sides opposed), and an eyelet let into it. An assistant is now to hold the limb as near as possible in the right position, while it is being smoothly strapped from the toes to the knee, leaving out the end (with its eyelet) of the piece on the foot and the wire loop at the upper part of the leg. Between these two—the one representing the origin, the other the insertion, of the muscle—there is to be stretched an india-rubber spring, at a proper degree of tension. Thus, while there can be considerable force used in the direction of any muscles in the leg, the *point d'appui* is so supported by a loop of strapping, which takes its bearings from both the skin of the leg and the surrounding plaster, that no injurious pressure occurs anywhere. There are also some little contrivances for changing the direction of the force,—as for the peronei tendons. Models showing this method and treatment were on the table, and cases of cure by these means were read.

Mr. HOLMES COOTE said that the Society felt indebted to the author for his paper, although it touched upon a class of cases that had of late years received, it must be owned, a very large share of general attention. Mr. Coote had listened in vain for the grave objections to tenotomy; and as for the treatment of deformities by extension, that was a principle dating from the earliest epochs of medicine. The three cases quoted by the author in support of his principle were, as far as Mr. Coote understood them,—1, a case of paralytic talipes equinus; 2, a case of weak ankle; 3, a case of flat foot. Now it was well known that the treatment of such cases by mere mechanical appliances was very generally all that was required. Tenotomy was the exception. But the division of tendons he affirmed to be almost invariably necessary in cases of congenital deformity, to which the author had not alluded. Experiments as regards tenotomy performed on animals were not satisfactory, owing to the impossibility of keeping the limb quiet, or the bandages properly applied. It did not occur to him that the evidence offered by Mr. Adams, in his late work, referred to by the author, was opposed to tenotomy, but he (Mr. Coote) affirmed that even if some of the smaller tendons did occasionally adhere to the sheath or neighbouring parts—an accident which should not happen—that inconvenience was not to be allowed to weigh against the great advantage of enabling an infant with a club-foot to put the sole to the ground before the parts had become moulded to their abnormal position.

Mr. WILLIAM ADAMS observed, that when he first heard the title of the paper, he expected that a general onslaught would be made on the practice of tenotomy, and that many serious evils would be traced to it, but was soon gratified to learn that the subcutaneous division of tendons was not objected to by the author. He had listened for the grave evils suggested by the title, and thought that after hearing the paper the Society might still ask what those evils were? He had heard only of certain imaginary evils, possibly resulting from the non-union of the posterior tibial tendon, or from the adhesion of the divided extremities of this tendon to the bone, and he was surprised that all the facts stated in the paper had been taken from his own (Mr. Adams's) book, on the "Reparative Process in Human Tendons," in which one instance of non-union of the posterior tibial had been recorded, and another in which non-union appeared probable, but not certain, as the reparative process was proceeding. Other cases had been quoted in which this tendon had become adherent to the bone, but no evils or ill-effects to the patients had been traced to these results, and it might be a good surgical compromise to cure a case of severe club-foot, even at the more or less complete sacrifice of the posterior tibial muscle. The adhesions described might limit, but would not completely destroy the action of the posterior tibial muscle. In reference to this point, Mr. Adams did not consider that the author's statements were accurate, or that the analysis of his cases had been fairly made. Aware of some evils arising from imperfect union of the posterior tibial tendons, he (Mr. Adams) had now adopted the plan of extending the muscles by bandaging the foot to a splint, for a week or more before the operation, and making the after extension very slowly. The author appeared to consider that tenotomy was unnecessary in paralytic deformities; but according to his (Mr. Adams's) experience, these were the cases from which the most brilliant results were frequently obtained, when the cases were judiciously selected and treated. Children and adults who had not walked for several years, or who had never walked, were frequently enabled to do so with mechanical supports after the contractions had been removed by tenotomy. Again, in reference to the method of treating deformities of the foot proposed by the author of the paper, he had listened in vain for novelty, and the plan of making extension by india-rubber bands was certainly not new. He (Mr. Adams) had seen a great variety of instruments constructed upon this principle, four or five years ago, by Mr. Bigg, who had applied them extensively, and had adapted them to some cases in St. George's Hospital. The continuous and increasing pressure produced by the elastic force could not be borne in many cases, and in some instances had to be discontinued from the sores produced, so that it was best adapted to severe cases. It was much

better to regulate the force by means of the cog-wheel. The views of the author were not new; this plan of treatment was only adapted to slight cases.

Mr. POLLOCK desired to remind the Society that the last speaker was in error in saying that Mr. Bigg made trials of instruments in St. George's Hospital. Mr. Bigg was instrument-maker to the hospital, and as such he merely acted under the directions of the surgeons.

The Author, in reply, said that he was aware that the cases he brought forward were not of the most obstinate description; but from the ease with which his treatment reduced all pedal deformities except equinus, he had no fear whatever that it would fail in the most severe form of varus. The necessity for rapidly changing the position of the foot in congenital varus was illusory, since section of the tendons was undertaken long before the child could use the foot; and his plan of treatment would bring the sole into the walking position before the infant was old enough to stand, and have the additional advantage of preserving some use in the muscles. As to tenotomy in paralytic cases, the author could not agree with Mr. Adams that such practice was successful; on the contrary, he had seen many cases in which deformity had only been changed for a worse evil. Surely it is not reasonable nor physiological when certain muscles of a limb are paralyzed, to divide the others? His (the author's) plan is simply to supply the amount of force necessary to the weakened parts. The success of this plan might be seen in the sample of flat-foot which he (the author) asserts arises from defect of power in the tibialis anticus muscle. He could easily understand that Mr. Bigg should find india-rubber bands produce slough; such results depend upon the mode of adaptation. When used according to his method, no such accident would follow.—*Med. Times and Gazette*, *Dec.* 7, 1861, *p.* 593.

60.—ON THE PLASTER OF PARIS SPLINT.

By Dr. LITTLE, New-York.

The limb is first shaven, or slightly oiled; a piece of old coarse washed muslin is next selected of a size so that when folded about four thicknesses it is wide enough to envelop more than half of the circumference of the limb, and long, to extend from a little below the under surface of the knee to about five inches below the heel. The solution of plaster is then to be prepared. Fine, well-dried, white plaster had better be selected, and before using, a small portion should be mixed with water in a spoon and allowed to "set," with a view of ascertaining the length of time requisite for that process. If it is over five minutes, a small quantity of common salt had better be dissolved in the water before adding the plaster. The more salt added, the sooner will the plaster "set." If delay be necessary, the addition of a few drops of carpenter's glue or mucilage will subserve that end. Equal parts of water and plaster are the best proportions. The plaster is sprinkled in the water and gradually mixed with it. The cloth, unfolded, is then immersed in the solution, and well saturated; it is then to be quickly folded as before arranged and laid on a flat surface, such as a board or table, and smoothed once or twice with the hand in order to remove any irregularities of its surface, and then, with the help of an assailant, applied to the posterior surface of the limb. The portion extending below the heel is turned upon the sole of the foot, and the sides folded over the dorsum, and a fold made at the ankle on either side, and a roller bandage applied pretty firmly over all. The limb is then to be held in a proper position (extension being made if necessary by the surgeon), until the plaster becomes hard. The time required in preparing the cloth, mixing the plaster, and applying the casing to the limb, need not take more than fifteen minutes. After the plaster is firm, and the bandage removed, we shall have a solid plaster of Paris case partially enveloping the limb, leaving a portion of its anterior surface exposed to view. If any swelling occurs, evaporating lotions can be applied to the exposed surface, and we can always easily determine the relation of the fractured

symptom, owing to an excellent constitution, aided by as indomitable pluck as ever graced a British soldier.

A recent clot was found in the popliteal artery, but its coats were not diseased, neither were those of the arteries of the leg, although it can scarcely be questioned that a portion of the contents of the aneurism suddenly blocking up the vessel at a higher point was the cause of all the evil.

The aneurismal tumour continued decreasing in size from the date of the accident, and two months afterwards completely disappeared.

There can be no doubt that the practice of operating at an early period in spreading gangrene, although sanctioned by names such as Larrey, Guthrie, Brodie, &c., is not always a safe, and possibly not a judicious procedure. Mr. Fergusson's experience of early amputations under like circumstances is particularly disastrous. He says, "I might possibly, in future, resort to a similar practice, but should feel greatly inclined to wait for a line of demarcation."

If amputation below the knee had been performed in the sergeant's case on the sixth or eighth day, when the foot showed evident symptoms of spreading gangrene, would the subsequent appearance of the disease in the calf have been averted? As far as one can reason from a solitary example, it appears to me that nothing is gained, and a great deal is at stake, by the surgeon's early interference in such cases. On the other hand, by waiting Nature's indications, and amputating when a line of separation has formed, it would be difficult to conceive a case of *sphacelus from sudden and spontaneous obstruction of the arteries of a limb* where such a plan of procedure would be unjustifiable. —*Med. Times and Gazette, Feb.* 15, 1862, *p.* 156.

63.—ON THE TREATMENT OF ANEURISM OF THE EXTREMITIES BY FLEXION OF THE LIMB.

By Ernest Hart, Esq., Surgeon to the West London Hospital.

[The first case treated by the author by means of flexion of the limb was published in 1859. (Retrospect, vol. xl. p. 100.) The present paper (read before the Royal Medical and Chirurgical Society), an abstract of which we give, details Mr. Hart's subsequent experience.]

The author remarked that the effect of forcible flexion of the healthy arm or leg might be observed in the considerable retardation of the blood flowing through the main artery, and the almost entire extinction of its pulse.

The application of this principle to the extreme flexion of the arterial trunks was obvious. Their structure favoured the effect described. Probably, also, the projecting bellies of the contracted muscles might cause some pressure on an artery which lay in contact with them; and in the case of an aneurismal swelling some direct pressure might be made upon the tumour; but of the occurrence of this circumstance, or of its desirability, the author was doubtful.

It was now pretty clearly established and generally accepted that the object in treating aneurism was not to cut off the supply of blood, or altogether at once to arrest circulation in it, but to cause such a retardation of the current as would lead to the gradual deposit of fibrinous laminæ in the interior, and so effect its gradual consolidation. The former method was uncertain and dangerous; the latter safe and permanent in its results. In the case in which the author first applied the method of flexion to the treatment of popliteal aneurism, and in that in which Mr. Shaw subsequently tested it, they were completely successful in obtaining the latter result. Mr. Hart had met with similar success in the following case:—

Case 3.—In September, 1860, a healthy and robust-looking man, aged 35, was sent to him by Mr. Bridge for treatment of a popliteal aneurism of the left leg. It was of the size and shape of a large lemon, projecting on the inner side of the ham. It had a loud bruit and very perceptible thrill, but was not entirely reducible. The skin was somewhat discoloured over the

tumour as though from a recent bruise. The patient had noticed the existence of this pulsating tumour for nearly two years. It had increased somewhat in size during the first nine months that he had observed it, and he had several times thought of seeking advice, but as he had felt little pain from the disease, he had neglected its cure. About three weeks before the author saw him he had received a blow in the region of the tumour, and since then he had suffered pain in it, and the swelling had increased. The arteries of the leg pulsated distinctly, and there was no very marked dilatation of the veins below the knee. There was no history of gout or syphilis. A week was allowed to elapse before commencing any treatment. The discoloration of the skin was then no longer visible, and the aneurism was pulsating distinctly as before. Mr. Hart desired in this case to separate the effect of treatment by flexion as clearly as he could from any collateral or adjuvant agencies. He did not, therefore, employ any form of medical treatment whatever; nor did he invalid the patient by enjoining the horizontal position and confinement to bed. He rolled the leg in a flannel bandage from the feet upwards, stopping below the tumour so as not to compress this in any way. He then bent the leg on the thigh, and retained it in the flexed position by means of three pieces of bandage attached to the ankle and along the leg. The patient was allowed to rise and go about the room by the help of a crutch. The forced flexion of the leg entirely arrested the pulsation of the tumour. Complete flexion caused, however, an inconvenient degree of pain, and after eight hours he relaxed the bandages so as to leave the leg in a semiflexed position. At night he ordered one grain of solid opium. The patient slept well. Next morning the tumour still pulsated distinctly. He again completely flexed the limb, and during the next two days it was retained in that position, being unmoved during the night. The patient moved about freely the whole time, by the help of a crutch and stick, and occasionally of a wooden pin leg roughly adjusted below the bent knee. He occupied himself in reading, writing, and smoking. He complained of pains in the knee-cap, but after the first night he slept moderately well without opium. On the fourth day Mr. Hart somewhat relaxed the bandages, and examined the tumour. He had the satisfaction to find that it was as nearly as possible solidified; a faint thrill, however, could be detected by placing the hand on the tumour. He again replaced the leg in the position of forcible flexion, and retained it so until the end of the sixth day. He could not then find on examination any trace of pulsation or thrill. The articular arteries beat perceptibly, and he concluded that the collateral circulation was satisfactorily established, and the aneurism consolidated by the deposition of laminated clots. As a desirable measure of precaution, he retained the leg in the semiflexed position for another week—a proceeding to which the patient, who fully understood the nature and object of the treatment, offered no objection. The subsequent progress of the case was in every way satisfactory; it offered no peculiarity worthy of note. It was three weeks from the conclusion of the treatment before the patient ceased to limp from the stiffness of the knee caused by its confinement.

It seemed to the author that this case sufficiently established the simplicity and efficiency of the treatment of popliteal aneurism by flexion in certain cases. Here he was enabled to effect the cure practically in six days, without the use of any apparatus—without the intervention of any watchers or assistants—without risk or danger. The patient had been cured of popliteal aneurism—a disease formerly so formidable—without being confined for a day to bed. In order to afford the grounds of a judgment on the relative merits of the flexion treatment, it might be proper to add a brief *resumé* of the cases which had been successfully treated by other surgeons in this country who had employed the method since the author had the honour to introduce it to the notice of the society. Some of these cases had been treated by flexion solely; in others, that method had been conjoined to the treatment by compression.

Case 4.—*Popliteal aneurism; recurrence after ligature of superficial aneurism; treatment by compression unsuccessful. Cure by flexion of the leg.* By

JAMES SPENCE, Esq.—T. H., a coalcarter, was admitted into the Royal Infirmary, Edinburgh, under the care of Mr. Spence, in August, 1858, with a pulsating aneurismal tumour, of the size of an orange. The superficial femoral had already previously been tied in May, 1857, with the effect of producing solidification of the tumour. But the pulsation recurred a few days before this second admission, and the tumour was then as large and pulsating as violently as before the artery was tied. Compression was carefully employed over a period of upwards of five months, but the tumour was at the end of that time increasing instead of diminishing. The patient left the house for some weeks, but returned, desiring to submit to any operation that might be thought necessary. The following is the sequel of this very interesting case in the words of the surgeon:—

"On examining the aneurism at this time, its condition was as follows:—It was the size of a pretty large orange, and pulsating strongly; the femoral artery could be felt to pulsate for about four inches below Poupart's ligament, ceasing to beat about an inch or less above the point where the superficial femoral had been tied. Pressure on the common femoral completely and readily arrested pulsation in the sac. Compression of the superficial femoral above the point tied also did so, but required very firm pressure to be made directly backwards. Compression along the course of the superficial femoral below the point tied produced no effect until it was made over the lowest part of Hunter's canal, when the pulsation of the aneurism became thoroughly commanded by it. Under these circumstances, after weighing the comparative chances of success between ligature of the external iliac, or the lower part of the superficial femoral, the latter plan was decided on, and the patient willingly gave his consent. But as the case did not seem a very hopeful one, it was thought as well to try the plan of flexion of the leg upon the thigh recently proposed. On bending the limb fully, all pulsation was at once and completely arrested, and the limb was bandaged in that position.

But the patient could not bear such flexion to be kept up. Accordingly, a slipper, with a slip of bandage sewed to the heel, was fastened to the foot, and the slip of bandage was then fastened to a loop connected with a broad bandage round his pelvis, and this gradually tightened, so as to increase daily the flexion of the leg on the thigh. This treatment was commenced on the 20th of May, 1859. On the 23rd, the pulsations were weaker, but returned when flexion of the limb was discontinued. On the 27th, the pulsation diminished; tumour smaller and firmer. There was still a tendency to increase of pulsation when the limb was allowed to remain straight for any length of time. On the 6th of June, the pulsation was scarcely to be felt in the aneurism, even when flexion was discontinued. The tumour felt solid and smaller, and enlarged anastomosing vessels could be felt over it and around the knee. The patient was now allowed to walk about with crutches, the affected limb being suspended in a flexed position. When in bed or sitting in the ward, he was desired to extend the leg occasionally, and not to keep it constantly bent. On the 23rd of June, there was not the slightest pulsation to be felt in the aneurism, which was firm and considerably diminished in bulk. Several very large anastomosing vessels could be felt over the tumour, the limb was of good heat, and there was no stiffness of the knee-joint. I kept the patient for about three weeks longer in the hospital under observation, and he was dismissed cured. Since then he has returned to his usual occupation of coalcarter, which requires him to walk considerable distances, but when I last saw him there was no tendency to return of the disease."

The author was not unaware that the flexion treatment had not always been successful. In cases under the care of Mr. Paget, Mr. C. H. Moore, and Mr. Birkett respectively, it had been employed, and disused as ineffective. The first two were, however, cases in which the contents of the aneurismal sac escaped by rupture or ulceration. These are severe forms of the disease in which extreme treatment is always likely to be required. In all three compression also failed, and ligature was resorted to. In the hands of other surgeons the method had been found painful, and apparently abandoned on that account. On this head he would only remark, that other methods of

treating aneurism are commonly both painful and tedious, as well as attended with greater risk.

The first results of any method of treatment are likely to be, as they have always been, less perfect and less uniformly successful than those which follow upon a larger experience and a more assured application. It is probable that attention to certain points of detail may tend to prevent pain and inconvenience. By carefully bandaging the limb, support is given to the superficial veins and rest to the contracted muscles. Friction of the limb upwards may serve the same purpose. Inunction of the knee-cap with oil and chloroform diminishes the sensation of stiffness and relieves pain. By allowing the patient to rise from his bed and dress himself, much of the tedium is obviated and coagulation of the fibrin aided. Sleep follows much earlier at night. Any favourite habit, such as smoking, may be usefully permitted, with the same object. It is desirable to bespeak the assistance of the patient himself, by explaining the nature of the malady and treatment. Flexion should be employed with care, and graduated. The author's reason for referring to these minutiæ might be found in the past history of this method and of the cognate treatment by compression, in which attention to minute details had been shown to produce a remarkable increase in the success obtained. For success, a watchful and intelligent interest was necessary.

In reviewing the cases already successfully treated by flexion, it seemed to the author that there were many elements for a favourable judgment. Compared with compression, it had the advantage of not requiring the constant and watchful attention essential to the success of that method. No instruments were employed, and no assistants were needed. There was no risk of ulceration of the skin, of erysipelas, or of that other rare effect described by Mr. Oliver Pemberton—the production of aneurismal varix at the site of the pressure. If not ultimately successful, it would still have had the effect of beneficially developing the collateral circulation. Of course, where practicable, it would always be preferred to the use of the knife. The interesting case of recurring popliteal aneurism under the care of Mr. Spence, affords an instance in which flexion effected a cure where the superficial femoral artery had always been tied, and compression during five months had failed; the undesirable alternatives being the ligature of the femoral close to the sac or the external iliac, under unfavourable circumstances. Mr. Spence states:—"From what I saw of the plan of the flexion of the limb in this case, I would have great hopes of its success as a curative measure, far simpler and more efficacious than any form of compression I have employed, devoid of all its risks, and not interfering with, but rather beneficial as a preparation for, ligature of the artery, should it fail in itself accomplishing the cure."

The cases related show that flexion may be satisfactorily employed as aiding compression where this is desired. Finally, Mr. Hart said that he did not now seek to define the position of the flexion treatment in relation to aneurism, or to establish arbitrarily its superiority to other methods. He desired to leave this matter to the judgment and further experience of the surgical profession. His object in this communication had been to submit to this Society the further facts which had come to his knowledge since he had the honour first to bring the treatment to their notice, and which furnish the grounds for a more complete judgment of its merits and deficiencies.

Mr. SOLLY said that the profession was greatly indebted to Mr. Ernest Hart for again bringing under their notice this simple plan of treating aneurism by flexion. This second paper presented a series of facts; and no one who heard those facts could hesitate to say that this was an ingenious and successful procedure, constituting an advance in the practice of surgery. He (Mr. Solly) wished that he could say he had any practical experience of the plan, but he should employ it on future occasions. Mr. Hart, however, must be aware that some aneurisms would not yield to this plan, and that neither this nor indeed any other method was uniformly applicable. He might mention to the Society a singular case of aneurism now under his care, which presented some features of interest. It was a case in which the patient

had a pulsating tumour on the arm; the arm had been paralysed. The skin over the tumour was stained by a network of vessels, but this was superficial. His colleague, Mr. Simon, was disposed to connect the aneurismal condition with the paralysis of the coats of the artery; but the nature and origin of the disease were doubtful. Mr. Solly referred to the medical treatment of aortic aneurisms by restricted diet, rest, and occasional doses of aconite, in terms of praise, mentioning cases now under his observation in which it was doing good.

Dr. HARE also referred to a case in which it had been of benefit.

Mr. WILLIAM ADAMS said that Mr. Hart's paper described what was evidently a great step in advance in the treatment of that very interesting surgical affection, aneurism of the extremities. It was to be regretted, in his opinion, that more attention had not already been given to this valuable improvement by competent surgeons, for he did not doubt that in the end a long list of successes would adorn this treatment, with which Mr. Ernest Hart's name was and must always be so honourably associated. In the three years which had elapsed since that gentleman's paper on the flexion treatment appeared in the "Transactions," there seemed to have been, however, nine cases illustrating the successful applications of this method. The treatment was founded and explained in the paper upon an accurate view of the pathology of aneurism. It was an ingenious and clever application of pathology to therapeutics, for which the author deserved great credit. The case in which flexion was shown to have effected a rapid cure of a popliteal aneurism, after the ligature of the femoral artery in the front of the thigh had been performed, and after a patient trial of five months' compression had failed, proved how especially this method might avail as a new and efficient surgical resource in the treatment of aneurism. Here it had afforded a safe and effective substitute for ligature of the iliac under unfavourable circumstances, or of the femoral artery in its diseased part close to the sac. In Mr. Hart's own case, for the first time a patient had been cured of popliteal aneurism without a day's confinement to bed. In any cases which came under his (Mr. Adams's) care he should certainly employ this method, and it might be expected that in a large number of cases it would supersede any other hitherto employed.—*Lancet, Feb.* 8, 1862, *p.* 147.

64.—NEW MODE OF MAKING COMPRESSION IN POPLITEAL ANEURISM.

By Dr. BLAND.

Dr. Bland, of Sydney, describes a mode of making pressure in a case of popliteal aneurism which he was induced to devise in consequence of the patient having refused to continue to submit to other procedures. The patient was placed in bed, on a firm mattress, in a half-sitting position, while the thigh was made to form an inclined plane in the opposite direction to that of the body, the leg and foot being placed horizontally on a soft pillow, in a frame or cradle, raised some ten or twelve inches above the surface of the mattress, and the bed-clothes being kept from the foot by a foot-board. A tourniquet was applied to the groin, its principal object being to divert the stream of blood as early as possible from the inguinal artery into the internal iliac and its numerous large branches. Another tourniquet was applied to the upper border of the popliteal space, care being taken not to make any painful amount of pressure on the nerve.

"The direct advantages derived from these arrangements were numerous and important. First, the position of the thigh tended to reduce the force of the blood-stream flowing into the aneurism, while its return by the veins was greatly facilitated. Secondly, the regulated, but gradually-increasing pressure on the inguinal artery, while reducing the stream of blood in that vessel, forced at the same time a larger stream into the internal iliac and its numerous large, and ultimately anastomosing branches. The same remark applies to the pressure made on the upper part of the popliteal artery. Thus,

every part of the arrangement was made to harmonise. The gradual obliteration of the main arteries concerned, was not only necessarily accompanied with the commensurate expansion of the extreme capillary ramifications, but that very expansion of the capillaries tended greatly to facilitate the obliteration of the main arteries in question; while, from the gradual manner in which this great change was brought about, the change itself was effected without pain or danger. The tourniquets each consisted of a light but strong iron ring instead of a strap. Each ring fitted a hinge on one side and immediately on the opposite side the ring readily opened or closed, so as to facilitate its application without in any way disturbing the patient. The pads, too, were not broad, but elongated, so that while the rings afforded a steadiness of position to the instrument in a transverse and vertical direction, an equal degree of steadiness was imparted to them in the longitudinal direction by the two pads attached to each tourniquet. The management of the tourniquets was simple, and attended with no incertitude or trouble. If the pad of either tourniquet began to feel a little tight, that of the other was at once slightly tightened, and then the pad which caused uneasiness was proportionately and cautiously unscrewed, so that the full amount of the requisite pressure was never, even for a moment, dispensed with. All this was carried on without pain or inconvenience to the patient, and in less time than it has taken to describe it; and so readily were the pads managed, that this alternate slight tightening and loosening was not unfrequently, towards the close of the treatment, left to the discretion of the patient." At the end of about ten days all pulsation in the tumour had ceased, and at the expiration of a similar period the limb was cautiously extended and flexed, the tumour having become much reduced in size. In a few more days the patient was discharged cured.—*Australian Medical Journal, and Medical Times and Gazette, Jan.* 25, 1862, *p.* 88.

65.—ON PLUGGING THE POSTERIOR NARES.

By Rawdon Macnamara, Esq., Surgeon to the Meath Hospital.

[Mr. Macnamara gives the plan employed in this operation by Mr. Smyly, senior surgeon to the Meath Hospital, from a communication by that gentleman.]

"The plugging of the posterior nares presents difficulties which deter some practitioners from attempting the operation. It is attended with great annoyance to the patient, not only at the time of its performance, but during the few days the plug has to remain; its removal also is sometimes found to be a troublesome task. No doubt patients will submit even to this to be relieved of the impending danger.

"I shall not easily forget a case I was called to see seven miles from town. I arrived in the middle of the night, and found an elderly gentleman lying like a statue, being not allowed to move hand or foot; the windows were wide open, the patient was shivering with cold, his head, neck, and shoulders drenched, and covered with wet cloths; he was pallid, yet the blood continued to trickle. On the nares being plugged the scene was changed from wretchedness to comfort. To his surprise and delight the patient was changed to a comfortable bed, and was told he might, with perfect confidence, compose himself to sleep. I mention this to show the great value of plugging the nostrils, and its superiority to other treatment.

"The plan I advocate now is to fill the cavity of the nose with slips of lint introduced from before. I prepare a few slips of lint, about one foot long and half an inch wide; doubling two inches of the first slip over the end of a strong director, I pass it along the floor of the nasal cavity as far as the posterior nares; the rest of the slip is then packed in, and the extremity, which should be made to taper, is left projecting out of the nostril for the purpose of its more easy removal. After this another and another slip is thus introduced, until the cavity is full. The first gentleman I prac-

tised this plan upon was a very witty person, who was very much amused at the process of taking out the slip, which he compared to the unpacking of a portmanteau; he named each slip as it came out after an article of dress."

Desault and Larrey were in the habit of employing a piece of linen sewed into the shape of a glove, which they pushed through the anterior nares backwards towards the pharynx; this was then packed with lint or some such material, and then drawn forwards, so as to press on the surrounding parts, and thus by direct pressure seal up the bleeding point. Though Larrey speaks highly of this procedure, still it presents many and obvious difficulties;—first, accurately to pack, and secondly, when it has discharged its duty, to *unpack* it. Mr. Bell improved on this plan by substituting for the linen bag, a piece of the small intestine of a pig; this fastened at one end is introduced as far back as the pharynx, and then filled with fluid or air, and pulled forcibly forwards, so as to act as a plug. In the 14th volume of the new series of the Medical Times and Gazette, p. 493, we find extracted from the Boston Journal an improvement on this suggestion of Bell's by Mr. Coate. I reproduce the passage: "Plugging in Epistaxis.—Dr. Coate employs a very simple contrivance; it consists in a piece of pig's gut, eight inches long, tied at one end, and then turned wrong side out, so that the knot may be on the inside, on a child's silver canula. By this canula it is introduced through the nose to the pharynx, and then blown up, and tied an inch or so outside the nose. He often introduces a spoonful of saturated solution of alum into it. It plugs up both the posterior and anterior nares thoroughly, and to remove it, it must be pricked, and gently twisted or drawn out. Dr. Coate always keeps a yard or two of gut on hand in a bottle of diluted alcohol." Franck also recommends a plan of treatment based on similar principles, and these have been improved upon by M. Martin St. Ange, in his *Rhinobyon*. This has, superadded to a small bladder, a silver canula fitted with a stop-cock; the bladder is to be pressed back into the pharynx through the nostril. Through the canula either air or fluid may be passed to dilate the bladder, when the stop-cock is turned, and the dilated bladder is then to be drawn forward until it is firmly pressed up into the posterior nares; the anterior are then plugged, and the operation is concluded. To withdraw it, the cock is turned, the fluid or air allowed to escape, and the instrument can be withdrawn, carrying before it all coagula, &c. Those who have employed this plan, speak highly in its favour. Its great merit consists in no interference through the mouth with the pharynx being required, as in the ordinary manner of plugging the posterior nares, which we shall presently describe, a point of no mean importance, as all know who have ever performed this operation, and in the great facility with which it can be effected in the younger periods of life, when our patients are not usually so manageable as in more advanced age.

The method generally pursued in these countries of plugging the posterior nares, is so well understood, that it requires but a very few words of explanation at my hands; it can be done either with the ordinary No. 10 gum elastic catheter, or with that most ingenious instrument devised for the purpose, by M. Belloc. I shall first describe the steps of the operation as performed with the assistance of the gum elastic catheter, a correct comprehension of which will facilitate my description of Belloc's instrument. Having prepared a plug composed either of folded lint or of a piece of sponge, about the size and shape of the thumb, it is to be tied firmly in the centre by a piece of sound twine, about eighteen inches long, the knot being tied in the middle so as to leave two equal lengths of the twine hanging at either side; you then pass through the eye of the gum elastic catheter from fifteen to eighteen inches of ligature silk, and pass the catheter almost directly backwards, along the floor of the nares, until it reaches the pharynx; the patient is then directed to open wide the mouth, and with a long dressing forceps *one* end of the silk is caught, and drawn forward out of the mouth, whilst the other, on the withdrawal of the catheter, hangs out of the nose; the portion of silk that is outside of the mouth is now firmly attached to one end of the twine that has been tied round the plug, and by pulling on the

end of the silk, that hangs outside the nose, we draw it also out through the nostril, and are in a position to proceed to place our plug *in situ*; grasping the plug between the index and middle finger of the left hand, we guide it back into the pharynx, and then up into the posterior nares, whilst with the right hand we draw forward the string that appears through the nostril, and pull the plug firmly into the required position; then the anterior nostril is to be plugged, and when that is done the string is to be again pulled firmly to secure the plug tightly in its position, then rolled round a small quill of lint, and secured with sticking plaster in some convenient position about the face. The string that hangs out through the mouth is to be fastened in a similar manner; taking care, however, that it is to hang loosely, in contradistinction to the nasal end, which is to be firmly on the stretch, so as to secure the retention of the plug in the desired position in the posterior nares.

Now, in description, all this seems simple in the extreme, but, in practice, it is anything but simple; a more troublesome proceeding, as this but too frequently proves, exists not amongst the minor operations of surgery. The patient, terrified at the loss of blood, half choked with the rush of blood down the throat ensuing on the position he is compelled to assume to allow the surgeon to catch and draw forward the string, and especially so, if of tender years, becomes unmanageable, and even in the most docile it is not always an easy task to lay hold of the string, situated as it is far back in the pharynx. To obviate this inconvenience, M. Belloc has invented an instrument in size and shape resembling a number 7 silver catheter, but of exactly one-half the length. At its extremity it is furnished with an eye, which, however, is not attached to the body of the catheter, but to a spring that runs down within the catheter. Along this spring is a stylet, which can be drawn out so as to double the length of the instrument, and which, by a screw, can be attached to the end of the spring. When so arranged, by pressure on that portion of the instrument that is outside the nostril, the spring is protruded in the pharynx, carrying with it the eye armed with the ligature; this, in virtue of the curvature of the spring, presents in the anterior portion of the mouth, and thus enables the operator with ease to catch the ligature, and then the subsequent steps of the operation are exactly those already described. This instrument, with difficulty described, will be at once understood by the most cursory inspection, and will be found a most valuable adjunct in performing this troublesome operation. Still even it requires some dexterity on the part of the manipulator. In that which I possess the *curve* of the instrument is too great, and if it be carried too far backwards, the eye, on pressure, will not come forward on the dorsum of the tongue, but will, on the contrary, go *down the throat*, and thus complicate instead of facilitating the operation. We should, in its employment, not seek to *see* it at the back of the pharynx, but, *when we judge that it should be thereabouts, pass down the button, and we shall have the gratification of seeing the armed eye travelling along the dorsum of the tongue, and can then readily seize the string that will facilitate our every other step.*

Having secured the plug in its proper position in the posterior nares, in the majority of cases of epistaxis, is not alone sufficient to arrest the hemorrhage, it still will be essential to plug the anterior nares, for this reason, that it very rarely happens that we can place the plug on the *exact bleeding point*, and if we fail in so doing, all that we shall have succeeded in effecting is the occlusion of the passage backwards, leaving that forwards open for the escape of the blood; if this, though, be also shut up, the effused blood is retained in the shut sac so formed, coagulates, and becomes itself the plug by which the further hemorrhage is arrested.

Presuming that the operation has been properly performed, and so far has proved successful, our next consideration will be, how long are these plugs to be let to remain? After twenty-four hours the anterior ones may be removed, but that in the posterior nares must not be interfered with *until suppuration becomes well established.* There are two periods when we shall experience difficulty in removing the plug; when we try to do so at too *early* a

period, before suppuration has been set up, or when we allow them to remain for too long a time, when we shall find, as the result, that granulations of luxuriant growth will have sprung up, and struck root into the plug, thus establishing firm connexions between the foreign body and the Schneiderian membrane, which can only be severed with considerable difficulty, a proceeding frequently attended with no trifling hemorrhage. Mr. Smyly, the respected senior surgeon to the Meath Hospital, informs me that he once saw the late Professor Macnamara experience considerable difficulty in removing a plug that had been permitted, by the surgeon who had inserted it, to remain an undue time without taking steps to remove it. If, on the occurrence of suppuration, the surgeon proceed to remove the plug, its extraction will be accomplished with facility and *safety*, for now there will be no danger of a recurrence of the epistaxis.

Authors on epistaxis write of this operation for plugging the posterior nares as if it were one of the most harmless in the entire range of surgery. In my opinion it is an operation that only should be had recourse to under the gravest circumstances, when every other method of arresting the flow of blood has been put in force and failed; when, in fact, it becomes a question with the surgeon whether to have recourse to it, or submit to the loss of his patient. Why I entertain this opinion I shall now briefly state. In the first place, even when most expertly and successfully performed, it is a proceeding most repugnant to the patient's feelings, and most incompatible with his comfort; the *malaise* that attends it is very considerable, the sufferer being, the whole time that the posterior nares are plugged, compelled to breathe through his mouth, which enforces an open and consequently a dried up, parched, condition of that organ: the sensation of the foreign body in this situation is indescribably unpleasant, so much so that we frequently experience considerable difficulty in inducing our patients to submit to it for the necessary length of time. Still, were this all, I should not write in such strong terms of disapprobation. But it is not all. Mr. Fleming informs me that the late Professor Colles entertained a very decided objection to this operation, founded on the fact of his having been called in to see a patient in whom tetanus ensued on this *trifling* operation, and which this distinguished surgeon attributed alone to the presence of the plug, acting as a foreign body, in the posterior nares. Nor is this the only and most serious danger; a very slight cause may loosen and detach from its position the plug in the posterior nares, when it will fall down upon the epiglottis, and produce almost immediate death. In such an occurrence as this the value of the piece of twine coming out through the patient's mouth will be recognised; in such an event it can at once be seized by a bystander, or by the patient himself, the plug instantly withdrawn, and the patient thus preserved from certain immediate death. In the article on epistaxis in Costelloe's Practical Surgery, I find the able writer deprecating the employment of this string, although, in a subsequent portion of the same article, he alludes to this danger, and gives a case on the authority of M. Martin St. Ange, where a fatal result, consequent on this very accident, occurred in the medical wards of the Hotel Dieu. The late Professor Porter informed me of the occurrence of a similar accident, which terminated fatally, in this city, whilst he was a student: and whilst I myself was serving my time in the Meath Hospital to the late Sir Philip Crampton, I witnessed, in a patient whose posterior nares had been plugged by the late Mr. Rynd, a similar occurrence, when the patient's life was only saved by the greatest promptitude, and where I experienced the greatest difficulty in removing the plug from its perilous situation, *because of the non-presence* of this, in my opinion, most necessary string. The accident, most fortunately, occurred during the late Mr. Porter's visit to the ward, when, of course, plenty of assistance was at hand. Had it occurred at any other period of the day, the result, in all probability, would have been far different, as, I need scarcely say, that the patient himself was physically debarred from describing what had occurred. In addition to these grave evils, I have seen erysipelas of the face subsequent to and consequent on the plugging of the anterior nares, and very frequently I have witnessed a most

unpleasant ozæna persistent for some weeks after the plugs have been removed. Fortified with such experience, then, is it, that I feel myself justified in asserting, that plugging the posterior nares should be the surgeon's "last appeal."—*Dublin Quarterly Journal, Feb.* 1862, *p.* 53.

ALIMENTARY CANAL.

66.—ON TRACHEOTOMY.

By Andrew Melville M'Whinnie, Esq.

[Between the lower edge of the cricoid cartilage and the upper border of the isthmus, a space exists in many subjects covered neither by muscles nor thyroid body. In a certain number, however, no such space exists at all. Occasionally an artery runs along the lower edge of the cricoid, and another along the upper edge of the isthmus of the thyroid gland, and sometimes a small artery runs vertically between these two. Sometimes a congeries of veins occupies this space. Still these obstacles are rare and trifling compared to those met with lower down towards the sternum.]

We learn from surgical anatomy that, in an operative point of view, the situation immediately below the cricoid cartilage is a most desirable one. Its advantages were pointed out by Baron Boyer, and it has since his time been frequently selected by surgeons of eminence in this country.

With regard to operations on this or any part of the respiratory tube, simplicity in the construction of the instruments to be employed is very desirable. The same remark applies also to surgical apparatus generally, for emergencies in other regions of the body; the anatomist well practised in the use of the scalpel is glad here to lay aside complicated instruments, to return to those which are most simple and ordinarily within reach.

In most operations undertaken for the relief of dyspnœa, anæsthetic agents are inadmissible. The instruments required are simple and few—namely, a fine narrow scalpel with thin ivory handle and forceps, a stout hook, with proper canulas or elastic tubes. One or two efficient assistants, provided with retractors, should be ready carefully to keep the parts asunder as they are separated by the operator. After the division of the integuments and fascia has been effected, the thin ivory handle of the knife, assisted by the nail of the left forefinger, which feels for any irregular artery, will make its way safely between the vessels, muscles, and other textures, whilst the operator carefully keeps to the middle line, which, together with the proposed extent of the outward incision, may have been previously traced on the skin with ink. The Dublin surgeons lay claim, and I believe deservedly so, to the introduction, as far back as the middle of the last century, of the hook to facilitate bronchotomy—an indispensable aid, without which no operation on the windpipe should be undertaken. I am convinced that we have seen patients lost from the surgeon not making use of this simple instrument; for could the trachea, in spite of the bleeding, have been at once seized, raised above the pool of effused blood, and quickly opened, especially in young people, the hemorrhage, so alarming and embarrassing from the venous congestion, would have ceased, at the same time that the dyspnœa which occasioned it would have been relieved on the free admission of air. A hook, single or double, serves as soon as the trachea has been fairly exposed, to raise, steady it, and bring it forward, whilst the requisite incision is being made to receive the canula, as well as during its introduction.

The bronchotomy hook should be of much greater strength than the ordinary tenaculum. In reference to this point, Mr. Jowers, of Brighton, observes to me that in the difficult operation in the case of contracted neck following cut-throat a tenaculum was used to seize the trachea, but unfortunately this instrument was too slender to hold the constantly moving part at the bottom of a very circumscribed space, and the patient died before a sufficiently strong hook could be procured. For such purposes, the form of hook, com-

bined with a director, as designed by Dr. Edwards, of Cheltenham, figured and described in "Druitt's Surgery," is well adapted, and I believe has met with very general approval. The same fixedness in the staff of Dr. Edwards's instrument, as he observes to me, renders a security in those convulsive movements of the trachea that we have so much to dread whilst we operate: "Staff and knife go up and down together with those tracheal movements which otherwise would of themselves expose vessels or other parts to danger with an unprotected knife."

Should there unfortunately be a flow of blood into the trachea, the hook is again of essential service; for, by still retaining his hold with that instrument, the surgeon is enabled to command and keep in sight the orifice that he has made, and by enlarging it freely and without delay, affords perhaps the only chance of saving his patient. In the "Mémoires de l'Académie Royale de Chirurgie" it is recorded that an alarming effusion of blood having taken place into the tube during the operation of tracheotomy, Vigili, of Cadiz, with presence of mind, immediately enlarged his incision by slitting up several additional rings. The extensive aperture thus made gave a ready egress to the blood filling the air-passage, the expulsion of which, like any other foreign matter, was favoured by inclining the head of the patient forward and downward. This inclination of the neck forward converts, especially in young people, the simple slit in the tracheal rings into a gaping orifice, favourable for the evacuation of their contents.

Dr. Gibb informs me that the above good practice was followed recently under his own eye by a surgeon, who immediately and instinctively as it were, enlarged the incision he had made into the trachea, when the blood was at once forcibly and safely ejected—a measure still more likely to be effective than the example of presence of mind and devotedness exhibited by M. Roux, who saved his patient from impending asphyxia by applying his lips to the wound, and by forcible inspirations sucking out the effused blood.

The hook is also of great assistance during the restlessness of the patient and his struggles for breath, in fixing the respiratory tube in the event of its being deemed a proper precaution to remove portions of any of its rings. An eminent contributor to the "Medico-Chirurgical Transactions" admits that on one occasion a sufficiently free opening was not in this way made into the windpipe, and, in the absence of proper attendance, a fatal result was the consequence. It must be a maxim, especially when an artificial tube is not employed, that the aperture in the air-passage should be in communication the most direct possible with the free external wound, not only for the easy passage of the air, but for the ready escape of all irritating matter, and for the cleansing of it away; we may in this way likewise prevent emphysema, which we have known to cause death in more than one instance during tracheotomy.

The Baron Dupuytren, in his clinical lecture after the removal of a haricot-bean from the trachea of a little girl, laid great stress, too, on the importance of not at once approximating the edges of the wound for fear of bringing on emphysema. At first, the cellular tissue will be permeable, and afford a ready passage to the air; but, at the end of some days, glued by the inflammation and rendered compact, this becomes no longer possible, so that the healing of the wound may now be hastened by bringing together its edges. It is certain that where there has been no disturbance of the cellular texture by previous dissection, and there is, therefore, direct communication between the air-passage and the external gaping wound, as in cut-throat, the occurrence of emphysema is comparatively rare.

Trocars of various kinds we have all known to answer their purpose, but they are not always at hand in emergency, and are now rarely used. Even in our large hospitals, commanding as they do apparatus of every description, they appear to be in great measure abandoned. We are acquainted not only with cases proving the difficulty and failure in attempts to introduce them, even in good hands, on account of the occasional smallness of the windpipe, its extreme mobility, and the resistance offered by ossification; but we also know of even fatal consequences from their employment. I am told by a gentleman, that in an instance which recently occurred in the metropolis,

the trocar unfortunately missed the trachea, and, passing close to the subclavian artery and œsophagus, penetrated the pleura, and caused eventually fatal inflammation of that membrane.

In the belief that the record of untoward circumstances occurring in these as in other surgical emergencies should not be withheld, if they conduce to future care and caution, a well-known London surgeon confesses to me his own bitter and disastrous experience of the use of the trocar on an occasion which, inasmuch as the effects were directly fatal, was more tragical than the last instance. He was called to a woman suffering under acute laryngitis. In endeavouring to perforate the windpipe with a trocar, the instrument, during the violent convulsions of the patient, was forced into some wrong direction—perhaps transfixing the trachea—when emphysema instantaneously appeared; the whole neck and face at once became enormously and frightfully swollen by being blown up, and suffocation immediately ensued. It is needless to add that the distress of the operator caused him to renounce the use of the instrument for the future.

When foreign bodies are ascertained to be lodged in the lower part of the windpipe, we are usually enabled, cautiously and deliberately, by careful dissection under chloroform, to expose the trachea low down to a considerable extent, in order to make the very free opening into the tube essential in such cases. Here, in the absence of intervals of very urgent symptoms, time is allowed to avoid any vessels, and effectually to control all hemorrhage, before the rings of the tube, which have been thoroughly exposed and laid bare, are incised from below upwards. We trust to a forcible expiration alone for the expulsion of the foreign substance through the aperture, which may here include some five or six, or even more, of the tracheal rings. The treatment of cases of this kind has often reflected the greatest credit on surgery. But in the large majority of cases of dyspnœa, where the symptoms are pressing and desperate, the surgeon is compelled to act more promptly; the use of chloroform is out of the question; time is not permitted for a delicate dissection amongst important parts; and, regardless of hemorrhage, he is obliged to proceed with his operation. It is therefore of the greatest moment to consider and determine at once which is, in the greater number of emergencies, the most eligible spot.

The opportunities of observation and experience long enjoyed in no inconsiderable field have enabled me to adduce some evidence in favour of the selection of the upper part of the trachea as being, as before stated, a most desirable situation—that indeed which, as in the first case related in this paper, the operator was compelled to choose. This portion of the trachea, at the same time that it is nearest to the surface, is also that which is most free from obstacles to its approach, and, except where foreign bodies are lodged near the bronchi, is shown to be most safe and efficient. Mr. Lawrence, in his lectures published more than thirty years ago, gives it a still greater preference; for he observes "that by making a perpendicular division of the cartilaginous rings immediately below the cricoid cartilage, we procure an opening not only adequate to the purpose of respiration, but free enough for any purpose for which the operation of tracheotomy may be necessary, including even the presence of extraneous substances in the air-passages." "The operation," he adds, "is not one of great difficulty, and we can hardly wound any part of importance."

Should the space so obtained in this situation be not sufficiently ample, and risk be apprehended from cutting the ring below, the scalpel may be directed upwards through the cricoid cartilage with advantage, thus converting simple tracheotomy into the operation (first proposed by M. Boyer) of crico-trachéotomie—a *procédé* which has frequently been adopted by English surgeons. An extension in this manner of the cut from the trachea through the cricoid cartilage enabled one of the surgeons of the Hôtel Dieu to extract a button-mould which had become lodged in the left ventricle of the larynx; and the museum of St. Bartholomew's Hospital contains a preparation showing that the artificial opening so procured has been further enlarged, with advantage, by the removal of a segment of the cricoid cartilage.

Hitherto the isthmus has been referred to mostly in its hypertrophied state, in connexion with operations undertaken for the relief of dyspnœa. In its normal condition, it lies over a varying number of tracheal rings, usually upon the second, third, and fourth; but it may consist of a very narrow slip, or cover nearly the whole of the front of the trachea down to the sternum. Seldom, in its natural state, is there any discernible projection of this part, such as I perceive in the bust of Cæsar, amongst the Townley marbles before adverted to. Although it is desirable to leave it intact, too much importance, perhaps, has been attached to it as obstructing the progress of operations. No case has come under my notice where it has occasioned difficulty; and I have seen dissections showing that, although included in the incisions in tracheotomy, no bleeding of any consequence has ensued. Sebatier considered that a division of this part was of no moment in the mesial line. If necessary, this central portion may be partly detached and pulled down in operations upon this part of the trachea. The isthmus, as my colleague, the late Mr. W. P. Ormerod, remarked, is not so firmly adherent to the trachea at its upper and lower edges as to prevent its being scraped from its attachments there with the handle of the knife.

The following abstract is from Hyrtl's observations, which especially relate to the vessels of the thyroid body, and are practical and important.

The professor asserts that it is untrue that the arteries of the thyroid gland freely communicate with each other; neither do those of the left side anastomose freely with those of the right; nor do the superior anastomose freely with the inferior. In the fœtus, up to the end of the third month, the thyroid gland consists of two distinct portions, which afterwards gradually become united in front of the trachea; so it is not difficult to understand why there should not be any very free communication between the arteries of the opposite sides. The veins communicate much more freely with each other than the arteries.

That an orifice made through the tracheal rings, immediately below the cricoid cartilage, is favourable for the constant wearing of an artifical tube is exemplified in Preparation No. 12, Series 25 (Pathological), St. Bartholomew's Hospital Museum. The almost complete closure of the glottis necessitated the continued employment of the canula, through which respiration was easily carried on to the time of the patient's death, twelve years afterwards.

Of artificial tubes I shall here scarcely speak. In some cases it is well known they are readily borne, but in others it is difficult, or even impossible to retain them. Like many other foreign bodies, the amount of irritation they produce varies considerably.

M. Dupuytren, on the occasion of removing a fish-bone, an inch and a quarter in length, from the upper part of a woman's larynx, by means of a forceps introduced through the fauces, remarked on the extreme difference in the sensibility of the same organ in different individuals. The irritation, in general so violent where foreign bodies are present in the air-passages, was here so inconsiderable that the patient had been but little inconvenienced by the bone remaining fixed in the throat, within the epiglottis, for eight or ten days.

One patient will wear either a metallic or elastic tube permanently; another will altogether dispense with it, respiration being then wholly carried on through a persistent opening in the air-passage.

A preparation in the Sussex County Hospital shows the upper aperture of the larynx nearly obliterated just below the chordæ vocales by a firm gristly substance. The ring of the cricoid cartilage is so contracted that it is reduced to a mere chink laterally. Immediately below this tracheotomy was performed, and the patient, during the last few years of his life, breathed entirely through the artificial opening in this situation. During this period the patient came repeatedly to the Brighton Hospital to have the edges of the orifice pared, and in this way my friend, the late Mr. John Lawrence, maintained the passage free without the necessity of the man's wearing any instrument.

As regards the construction of a tracheotomy tube which may be required to be worn permanently, I may just mention that I have seen one well adapted to its purpose, the contrivance of Mr. Anderson. He politely desired a lady to call upon me wearing one, and which she had done for two years, in the trachea, close to the cricoid cartilage. This instrument, which afforded her great comfort, combines with it the advantages of a respirator, for the silver canula is furnished with a shield, or helmet pierced with small openings.—*Lancet, Feb.* 8, 1862, *p.* 143.

67.—ON STRANGULATED HERNIA.

By Thomas Bryant, Esq., Assistant Surgeon to Guy's Hospital.

[Previous to the discovery of chloroform, our aids to the taxis were opium, bleeding, and the warm bath.]

The practice which has now eclipsed in value the means already mentioned is the administration of chloroform; it doubtless possesses in itself all the advantages of the older forms of practice, and none of their evils; under its influence if the taxis is to succeed, the slightest manipulative efforts will suffice; if the taxis fails, no other path for reduction but by operation lies before us. By its use delay is not occasioned, symptoms are not masked, as may be done by opium, and the danger of long-continued and repeated taxis is annulled. Amongst the seventy-eight instances of reduction by the taxis of an inguinal hernia which I possess, fifteen were completed under the influence of chloroform; and it must be remarked that in nearly all these instances all other means had been attempted and had failed, reduction having been effected by the surgeon who had been called down to perform herniotomy. In none of these cases did any bad or fatal result take place; indeed, convalescence was speedily re-established.

If then, such success as has been already quoted can be relied on, viz., the reduction by the taxis under the influence of chloroform of nearly 20 per cent. of those cases in which all other means had been employed and had entirely failed, it appears to be a natural and fair conclusion to which we are at once led, namely, that we should, in all cases of strangulated inguinal hernia of any standing, primarily employ the means which have been proved to be so successful, and not waste time by such delay as necessarily follows the use of other remedies when we possess one so incomparably their superior, which embodies in itself all their virtues and none of their evils.

On Strangulated Inguinal Hernia.—One third of all the cases of strangulated inguinal hernia require operative relief. This conclusion is unmistakable, and is a corollary to the assertion, that two-thirds can be reduced by means of the taxis; this number also, I believe, would be materially lessened if chloroform could be primarily used in all instances.

I possess the notes of 44 cases in which herniotomy has been performed; 23 of these, or 52 per cent., proved fatal.

In 9 of these the *sac was not opened*, of which 2 only, or 22 per cent., died.

In 35 the *sac was opened*, of which 21, or 60 per cent., died.

Comparing the two classes of cases, it would appear that those in which the sac was opened are by far the most fatal, the proportion rising from 22 to 60 per cent., being nearly three times as great.

Reviewing the cases in which the *sac was not opened*, two were in infants, patients of Mr. Cock.

[The following conclusions are drawn from a large number of cases, and are the concluding portion of a long and able article on hernia:]

1. That of all the cases of strangulated inguinal hernia, 64 per cent., or about two-thirds, are reducible by the taxis, the remaining third requiring operative relief.

2. That in at least 19 per cent. of the cases returned by the taxis success has been obtained by the influence of chloroform when all other means had been attempted and had failed.

3. That death takes place in 3·8 per cent. of the cases reduced by the taxis, and in 52 per cent. of the cases operated upon.

4. That of the cases of strangulated inguinal hernia—

24 per cent. are "congenital."
5 " "recent," and strangulated in their first descent.
70 " "old," that is, have existed for years.

5. That in cases of *inguinal hernia becoming strangulated on their first descent*, or "*recent*" cases, reduction by the taxis is seldom accomplished; that the sac is, as a rule, required to be opened, and that a fatal result is unfortunately the rule.

6. That in cases of *strangulated "congenital" hernia*, more than half are reduced by the taxis; that in 85 per cent. of the cases requiring operation the sac will require to be opened and a fatal result ensues in about 38 per cent.

7. That in cases of "old" hernia, that is, where the hernia has existed for years (twenty years being the average), 70 per cent. are reduced by the taxis; in 77 per cent. the sac will require to be opened, and 50 per cent., or half the cases operated upon, terminate fatally.

8. Reduction by the taxis is most successful in "old" strangulated hernia, reducing more than two-thirds; one half of the cases of strangulated "congenital" are also amenable to this treatment, and in the "recent" cases it is seldom successful.

9. That herniotomy is most successful in "congenital" hernia, about two-thirds recovering; half the cases of "old" hernia operated on recover; but this operation is almost always fatal in "recent" cases.

10. That in herniotomy for inguinal hernia the sac is required to be opened in 80 per cent., and unopened in 20, and that death takes place in 22 per cent. of the latter class of cases, and in 60 per cent. of the former.

The latter conclusions, brought out by the careful consideration of the cases of strangulated hernia requiring herniotomy, renders the second a point of greater importance. In the cases of herniotomy it has been shown that more than half the cases operated upon proved fatal; it has also been shown that in the cases reduced by the taxis success had been obtained in at least 19 per cent. by the use of chloroform when all other means had been attempted and had failed. Indeed, in all these instances the attendance of the operator had been requested. It will hardly be thought an unfair thing to consider these fifteen cases so treated as cases rescued from the danger of herniotomy, and a large proportion of them consequently from death. If the use of chloroform as a means of securing the reduction of a strangulated hernia had not been recognised, the whole of these cases would have been operated upon; and counting these 15 cases together with the 44 which required herniotomy, 59 would have been the figure upon which this paper would have been based. It may fearlessly, then, be stated that chloroform has rescued at least 25 per cent. from the dangers of herniotomy. No mean triumph, indeed, and certainly sufficient to encourage the practice which I trust some day to see general, namely, the primary treatment of all cases of strangulated inguinal hernia (not very recent) by chloroform, such a practice being doubtless the safest, most speedy, and effectual in securing the successful application of the taxis.

On Strangulated Femoral Hernia.—Femoral hernia is less common than inguinal by 74 per cent.; as a rule, it makes its appearance between 50 and 70 years of age; it is far more liable to strangulation, and to strangulation on its first descent, than inguinal, 44 per cent. of the cases of strangulated hernia being of this nature; and that when strangulated, 73 per cent. require herniotomy, not 27 per cent. being reducible by taxis.

It has been stated that scarcely 27 per cent. of these cases of strangulated femoral hernia are reducible by the taxis, this fact standing in marked contrast with the cases of strangulated inguinal hernia, in which two-thirds, at least, are reducible by such means. Out of 142 cases of strangulated femoral hernia, 38 only are arranged in this class; 4 of them died, or 10·5 per cent., the mortality of these cases being nearly treble that of inguinal hernia.

Analysing the 38 cases, it will be seen that—

10 cases, or 26·3 per cent. were of "recent" origin; that is, were strangulated on their first descent; all recovered.

28 " or 73 " were of older date, having existed months or years; 4 of them died, or 14 per cent.

One of the successful instances had been operated upon the same side one year and a half previously for strangulated hernia. The warm bath and the taxis were the chief means employed in these cases, opium also having in some few instances been administered. In one only was reduction successful under the influence of chloroform when other means had failed. This point is worthy of remark, more particularly when contrasted with the success obtained by the use of these means in inguinal hernia; the taxis, it has been shown, is by no means successful in reducing a strangulated femoral hernia, and chloroform is also a much less valuable adjuvant in securing such an end than it is in inguinal. The taxis should be very cautiously applied in cases of strangulated femoral; force is far more dangerous in this form than it is in the former, although in both it is indefensible. If chloroform is given, a very slight manipulation will, as a rule, suffice; powerful pressure in both forms cannot be too strongly reprehended.

[It is very necessary to make a distinction between the cases of femoral hernia, which become strangulated on their first descent, and that large class of cases in which strangulation has only appeared months or years after the hernia has first taken place. These cases differ at every point, and the more that difference is looked for, the greater appears to be the necessity for the distinction. The following summary shows the distinction clearly:]

Conclusions concerning cases of RECENT *femoral hernia becoming strangulated.*

1. That they form 21 per cent. of the whole number of cases of strangulated femoral hernia, and that 45 per cent. are fatal.

2. That 32 per cent. of these cases are reducible by the taxis, and with general success.

3. That herniotomy is required in 68 per cent., and of these 66 per cent. subsequently die.

4. That of the cases operated on, the sac was opened in 38 per cent., death following in these cases in 87 per cent.

5. That the sac was not opened in 62 per cent., the death rate in these falling to 54 per cent.

Conclusions concerning cases of OLD *femoral hernia becoming strangulated.*

1. That they form 78 per cent. of the whole number of cases of strangulated femoral hernia, and that 30 per cent. are fatal.

2. That 25 per cent. of these cases are reducible by the taxis, and that 14 per cent. of these die.

3. That herniotomy is required in 75 per cent., and that death follows in 36 per cent.

4. That of the cases operated on, the sac was required to be opened in 62 per cent., death taking place in 45 per cent.

5. That the sac was not required to be opened in 58 per cent., the death rate in these being only 22 per cent.

Comparing these two tables of conclusions concerning the cases of strangulated "recent" and "old" femoral hernia which the facts previously given unquestionably authorise, the following deductions appear necessarily to follow.

1. That at least one-fifth of the cases of strangulated femoral hernia are "recent," and that these cases are 50 per cent. more fatal than the cases of strangulated "old" hernia.

2. That the taxis in recent cases is the most successful, and that it is seldom followed by a fatal result.

3. That herniotomy is less seldom required in these "recent" cases, but that when it is, it is nearly twice as fatal.

4. That the sac is not required to be opened so frequently in "recent" as in "old" cases, by at least 28 per cent. The proportions between the cases

in which the sac was opened or not in the two classes of cases being exactly reversed.

5. That where the sac is opened, death takes place more frequently in "recent" cases by at least 33 per cent., or one-third; and in "old" hernia the mortality is twice as great.

The practical deductions to be gained by the consideration of these facts are not unimportant. They increase our anxiety to return a "recent" strangulated femoral hernia by the taxis and thus to do away with the necessity of herniotomy. They also show us that the taxis should be employed more cautiously in "old" hernia. They satisfactorily prove to us the benefit of operating without opening the sac; the difference in the mortality, both in "recent" and "old" cases, falling in the former 33 per cent., in the latter 23 per cent. The cases in which the sac was opened being in the "old" twice as fatal.

It must be observed, also, that the facts revealed by the analysis of these cases are by no means calculated to give the most favourable aspect to the operation without opening the sac, as it includes the cases of several of my colleagues, who are not converts to the benefits of the practice, and who invariably open the sac. It is fair to believe, then, that if the result of the practice as given is favourable comparatively, it would be still more apparent if the same practice had been carried out in all cases; as it is, however, it appears to be strongly in favour of the minor measure, whether we consider the cases as a whole, or divided up into what I maintain to be the only correct plan, the "recent" and "old" cases of strangulated hernia. To classify these two classes of cases together evidently misleads, and I believe all surgeons who will compare the two tables previously quoted, will at once understand the wisdom of their separation, and also the necessity.—*Guy's Hospital Reports*, *Vol.* vii. *pp.* 49, 60, 62, 65.

68.—ON OPENING OR NOT OPENING THE SAC IN STRANGULATED HERNIA, WITH PRACTICAL CONCLUSIONS.

By Thomas Bryant, Esq., Assistant Surgeon to Guy's Hospital.

[From an analysis of a great number of cases at Guy's Hospital, the author, in the previous part of the paper, shows most conclusively the superior success attending operations in which the sac is left unopened. He says:]

Taking the cases of inguinal hernia in which herniotomy was required, death took place in 60 per cent. of the cases in which the sac was opened, and in 22 per cent. only in which it was left alone; the difference being nearly 40 per cent.; the cases in which the sac was opened being nearly three times as fatal as were those in which it was left untouched. In femoral hernia the same conclusion appears to be equally manifest, the death rate in the two cases differing 20 per cent. When the sac was opened, the mortality being 50 per cent., and when left unopened only 30 per cent. If, however, in the cases as a whole this conclusion is tolerably clear, when the division of the cases into the "recent" and "old" is made it becomes still more prominent, and the value of the minor operation stands out more boldly. In strangulated "recent" inguinal hernia, the difference in the operation does not appear to have much influence, a fatal result too frequently takes place in both. But in the cases of strangulated "old" inguinal hernia, the difference is immense, the death rate descending from 60 per cent. when the sac is opened to 16 per cent. when it is left untouched; the difference being 44 per cent., or nearly four times as great; and in congenital hernia the practice is equally beneficial. In femoral hernia the same conclusions are apparent. In "recent" cases, when the sac is opened, the mortality is 87 per cent., and when unopened but 54 per cent. In "old" cases, where the contents of the sac were exposed the mortality was 45 per cent., and when not, but 22 per cent.; the mortality in the former being twice as great.

These facts appear strong, and argue all one way, and that is against the

practice which demands more to be done in an operation than is absolutely essential; it may, I think, be certainly said that in all cases of inguinal, femoral, or umbilical, recent, old, or congenital—the cases are more successful when the sac is not opened, and, as a consequence, the surgeon is hardly justified in taking an unnecessary step by opening the sac when no benefit, but some harm appears to result.

It may be a satisfactory thing on the part of the surgeon to know the condition of the hernial contents, but practically such knowledge is of little use; and unless it can be proved that such an inspection is of any value, the opening of the sac must be regarded as unnecessary.

In another place, it was fairly shown that in no single fatal case could the opening of the sac have had the slightest influence in retarding or preventing the fatal result. Thus the dead, by a negative argument, aid in recommending the minor operation. The tables given certainly argue in favour of not opening the sac, and the fatal cases told also the same tale; the conclusion therefore remains to be drawn, that if no good result can be shown to take place by opening the sac, that death is more common, and cannot be retarded or prevented by such a practice—the rule for the operator should be to attempt the lesser operation, feeling that if that fails he can always resort to the major measure.

Many of the advocates for opening the sac confess that in a large inguinal hernia, and in umbilical, the danger to life mainly depends upon the amount of manipulation that the exposed strangulated bowel undergoes, and even express an opinion that in these cases the practice of opening the sac is to be condemned.

In cases of femoral the same injury must certainly take place, and although the bowel is, as a rule, less in extent, the evil must be the same in kind, although perhaps less in degree. In surgery, the rule for the operation is surely this, that no step should be taken unless an absolute necessity exists; that is, unless some positive advantage can be obtained. In opening the sac, in all cases of strangulated hernia, this positive good is still undeclared; and as the weight of evidence tends to the opposite line of practice it must be condemned.

Practical Conclusions.—The careful consideration of the pages which have just been concluded have naturally some positive tendency in fixing or indicating the practice which is to be adopted in the treatment of strangulated hernia.

Inguinal Hernia.—In the inguinal, the fact which appears to be most prominent is the success which follows the cautious application of the taxis; and if that taxis is employed under the influence of chloroform, very general success may reasonably be expected. In the earliest hours of strangulation, success may follow the taxis without such means; but if many hours have passed away, and the symptoms of strangulation are manifest, chloroform should be employed. Baths, opium, and ice have been shown to be unnecessary, although in former times they have been most serviceable. But as all the advantages which these means afford are embodied in the use of chloroform, and all the evils of their use have been dispensed with, the primary employment of the means which has been so beneficial cannot be doubted. In cases, therefore, which have been strangulated many hours, let chloroform be at once administered; and when the patient has been fully brought under its influence, the taxis should be resorted to; the surgeon applying it with the confidence conferred by the knowledge that, in extreme instances, success has followed its use when all other means had failed, and with the reasonable hope that in less severe cases success must follow. I am so far impressed with the value of this agent in reducing a strangulated inguinal hernia, that I believe there are few cases in which success will not be obtained, if the patient is brought completely under the influence of the anæsthetic, and the taxis is fairly applied. The force required to reduce an inguinal hernia under the influence of this anæsthetic is comparatively nothing, the gentlest manipulation proving sufficient. In an earlier page it has been shown that, by its use, in at least 20 per cent. of the cases reduced by the taxis success had been obtained

when all other means had been attempted and had failed; and, with this success, it is not unfair to believe that in other cases less severe a greater one can be secured.

In the strangulation of "recent" inguinal hernia, the extreme fatality of the operation of herniotomy renders the necessity of its reduction by the taxis more urgent; and the fact, that a fatal instance after reduction under the influence of chloroform is not recorded, is an encouragement to its use. In "old" hernia, also, the fact that a large proportion of its cases are successfully treated by the taxis renders this treatment by chloroform more valuable, as it raises our expectation that the cases for operation may be diminished, that few will require herniotomy.

If herniotomy is required, what operation, therefore, should be selected? The tale told in a previous page is tolerably clear; operate, if possible, without opening the sac. The difference of mortality between the two cases have been shown to be great (50 per cent.); and although it is true that in a large proportion of the cases the sac is required to be opened to allow of the reduction of its contents, that point tells nothing against the practice which advises primarily the minor operation.

The largest proportion of strangulated inguinal hernia being of the "old" kind, that is, taking place in subjects who have had a rupture for twenty or more years, may perhaps explain the fact that so many cases are irreducible without opening the sac. In these instances the omentum is, as a rule, down and adherent, the strangulated intestine being generally situated behind or within an omental sac. In such cases, it is not difficult to understand how it is necessary to expose the parts, and to unravel them, to allow of the reduction of the strangulated bowel.

Treatment of Strangulated Femoral Hernia.—In the treatment of inguinal hernia, it has been already stated, that the hope with which the surgeon proceeds to reduce the tumour by the taxis is very great, and that it becomes greater when the patient has been brought under the full influence of chloroform. Indeed, it has been written that there are few cases which are believed to be incapable of reduction by such means. In the treatment of strangulated femoral hernia a very different tale has to be told: but few cases are reducible by the taxis, and the mortality of these is very great. The use of chloroform in such instances also cannot be so highly recommended. The taxis, as a means of reducing a strangulated femoral hernia must be looked upon with considerable suspicion, and indeed I believe that treatment to be only applicable to a small class of cases.

In the strangulation of a "recent" femoral hernia it has been shown to be the most successful, a larger number of these cases being capable of reduction by the taxis, and with a less fatal result than in the "old." The great mortality also of these "recent" cases after herniotomy renders the treatment by the taxis of greater value, and increases our anxiety to reduce the tumour without an operation.

In strangulated "old" femoral hernia, but few cases have been shown to be successfully treated by the taxis, and of these the mortality is very great; the operation of herniotomy is also less fatal; it becomes therefore a question whether it would not be better to put aside the taxis altogether in those cases, and operate at once, than run the risk of injuring the strangulated bowel by the taxis. The danger of the taxis in femoral hernia is always great, and the greatest caution should be employed in its application. Violence cannot be too strongly reprehended. The mortality of the cases after reduction helps to enforce this fact, and to insist upon the moderation with which it should be applied.

In the early hours of a strangulated femoral hernia, and more particularly in "recent" cases, the taxis may be employed, but this only with extreme caution and moderation, a few minutes only being sufficient if success is to be obtained; a longer continued period of pressure must do harm.

If many hours have elapsed after strangulation, and the case is of an "old" character, the taxis must be dispensed with, or at any rate employed but gently when the patient has been brought under the influence of chloroform

preparatory to an operation. In some few cases it may then succeed; but the hope of success is slight.

When herniotomy is required, the operation without opening the sac appears to be the most successful, a difference of 20 per cent. existing between the two classes of cases. In strangulated "recent" examples, this difference becomes greater, the majority of the cases being relieved by this operation; the mortality sinking from 87 to 54 per cent. In strangulated "old hernia" the same practice is indicated, the mortality in the cases in which the sac is opened being 45 per cent., and when not opened only 22 per cent. The operation of not opening the sac is thus clearly the most successful, and should be carried out whenever applicable.

The not opening of the sac is the chief point to be attended to. If the hernia can be reduced without opening the fascia propria, as suggested by Mr. Gay, and recommended by Mr. Birkett in his "minimum" operation, so much the better; but I imagine the difference of a layer of fascia more or less can have no influence on the result of an operation, unless it be to expose the hernial contents. The object is not to expose or manipulate an injured and probably an inflamed bowel; and carrying out this principle, the line of incision, or its depth, may be left to the fancy or discretion of the operator.

Umbilical Hernia.—In this variety, the same principles of treatment which should influence our practice in the former cases appear applicable. Reduction by the taxis is on the whole successful, and in obstinate cases the use of chloroform should be employed. When herniotomy is required, if the hernia can be returned without opening the sac, the attempt should be made, the mortality of these cases after operation, as a rule, being very great; indeed, the only successful example which I have quoted is the one in which the contents of the sac had not been exposed. If this is impossible, the practice followed in the case quoted might be followed, namely, the opening of the neck of the sac just sufficiently far to allow of the division of the stricture, but not the exposure of the hernial contents. By such means the real danger of opening the sac, namely, the exposure and manipulation of the inflamed bowel is done away with, and a greater success may consequently be anticipated. In all cases, however, the not opening the sac would appear to be the best; and I am at a loss to know a single reason why this rule should not be carried out. When reduction without exposing the interior of the sac cannot be effected, the sac must be opened; but such a step must be regarded as an evil, and as being unnecessary if reduction can be effected without it.—*Guy's Hospital Reports, Vol.* vii. 1861, *p.* 79.

69.—ON THE TAXIS IN STRANGULATED HERNIA.

By Frederick Le Gros Clark, Esq., Surgeon to St. Thomas's Hospital.

[The patient was about 30 years of age and had had hernia for some time. He had worn an ill-constructed truss.]

The symptoms of strangulation were, at first, very acute, and the tumour very tense and tender; he had vomited. The warm bath gave relief, and allowed of the taxis being employed without much pain; but the house-surgeon, failing in reducing the rupture, ordered ice to be applied, and sent for me. I found the external aperture, through which the rupture protruded, large and prolonged over the sac, and was surprised, considering the tightness with which the neck was girt, that the taxis occasioned so little pain. For some time no effect seemed to be produced; yet the circumstance I have just mentioned encouraged me to persevere much longer than it is my custom to do. The patient vomited at this time; and when I was about to relinquish any further effort, a gurgling sensation induced me to continue. I also changed my position to the opposite side of the patient; and after bidding him try if he could accomplish what I could not, I ultimately succeeded

in effecting the reduction. A considerable quantity of chloroform had been administered, but without any apparent effect.

There is nothing uncommon in the above detail, and yet there are points of practice illustrated which are of great importance in your surgical treatment of hernia:—1. Under what circumstances and for how long should the taxis be persevered in? Rough manipulation is to be deprecated under all circumstances; and I believe to prolong the taxis when the symptoms are urgent and the tumour is very sensitive, is fraught with much risk, and is not unfrequently the cause of failure in success of a subsequent operation. But, in instances such as the present, where the strangulation was recent, and the local suffering scarcely appreciable, though the tightness of the stricture and the sympathy of the stomach left no doubt of the existence of strangulation, it is right to persevere. It is proper in all cases, but especially, as in this, where the neck of the tumour is large, to use the fingers and thumb of one hand to direct the hernial contents towards the abdomen; and it is well to vary the side and exchange hands, as you may thereby succeed in giving a better direction to your pressure. Moreover, you may often learn something from the patient's mode of helping himself; and it is interesting to watch with how much tact and delicacy he will apply the taxis. Change of position, also, by raising the patient's legs, and by directing him to turn on one side or the other, should not be neglected. 2. What are the signs of probable success in the taxis? Diminution in the tension of the tumour is one, and a gurgling sensation, as if the contents of the bowel were passing beyond the stricture, is another; though these may sometimes occur without a favourable result. When these indications are present, gentle uniform pressure will often accomplish more than manipulation. 3. When, in such a case as the above, should an operation be resorted to? I do not think there is the same urgency in the absence of all tenderness in the tumour or pain in the abdomen, as where these symptoms are present, especially when such tenderness and tension have been in a measure relieved by the means employed. Yet, when the general symptoms are urgent, it would not be wise to delay the operation, even should there be an amelioration in the local condition; and especially so if the contents of the sac are ascertained to be intestine. It is scarcely necessary to add that these remarks apply only to previously existing ruptures, in which strangulation is recent; for the absence of pain may otherwise indicate a fatal loss of sensibility in the hernial contents.—*Med. Times and Gazette, Jan.* 25, 1862, *p.* 75.

70.—ON THE RADICAL CURE OF REDUCIBLE HERNIA.

By Redfern Davies, Esq., Birmingham.

[The author commences by saying that he has followed almost exclusively the method devised by Mr. Wood. He believes it to be the most successful and least painful known. The following is the way in which he performs the operation.]

The patient lying on his back, and his bowels having been that day opened, chloroform is administered if wished for. An incision, one inch and a-half below the spine of the pubes, is then made for about three-quarters of an inch, the *integument* of the scrotum only being cut through; the lips of the incision are now successively seized hold of, and by a blunt-pointed pair of scissors a dissection is made beneath the integument, for a space of two inches around the external opening of the inguinal canal.

The conjoined tendon is now to be felt for by the finger of the left hand, which pushes before it some fascia. The needle recommended by Mr. Wood is then carried on the finger to the external abdominal ring, through which it is to be passed for a distance of about half an inch, or it is thrust through all the soft parts above. When the point has emerged through the integument, the needle is threaded by a silver wire, and then withdrawn with the wire.

The first and most difficult portion of the passing the wire is then accomplished.

The finger now seeks for the external pillar of the ring, and on it the needle, bearing a loop of the wire already introduced (*i.e.*, the wire simply bent on itself), is carried about half an inch within the canal, and all the soft parts above it are transfixed. The extremity of the needle appears on the surface of the abdomen, bearing a loop of wire, and it easily glides down the *free* end of the wire when the loop is held.

And again, upon the finger having found the internal pillar, the needle bearing the wire is carried and made to pierce the tissues above, and a little within the canal. The needle having been disengaged from the wire is lastly withdrawn.

The free extremities of the wire are then passed through an aperture in an oval piece of thickish glass (three inches long by two broad), and the loop through another aperture a quarter of an inch distant, and having been pulled together as close as can be, they are twisted over each other, which securely fastens them.

The testicles having been supported by some tow, and the parts washed, all is done.

During the day of operation some varicocele appears, which depends upon the veins of the cord being pressed upon by the disturbance of the parts and the consequent swelling, and remaining but for a few days, indicates only that the operation has been efficiently done, and requires no notice to be taken of it.

The loss of blood during the operation amounts to about half a tea-spoonful.

The wires may be unfastened on the eighth day, and removed entirely on the next. The punctures and incision, from which some pus will exude, should be treated by water dressing; and when healed (in about one week's time), the patient may be allowed to get up, and is perfectly cured.

The propriety of applying a spica bandage and compress, is now a very much debated point. We know that continued pressure induces absorption, so that it is reasonable to infer that the constant pressure of a spica bandage and compress, will so act upon the recently introduced tissues in the inguinal canal ring, and that, eventually, there will be produced as bad a state of things as there was before any operation was attempted.

However, practically, such is not the case. In my former operations (those after Wützer's method) I used the bandage with a graduated compress, most extensively, and, though not perfectly satisfied with it, I did not find those effects produced, which might have been expected from a theoretical consideration beforehand.

Believing, however, that for a time the abdominal walls must be weaker in the inguinal region than elsewhere, and that concussions had better be guarded against, I now use a simple bandage, and by this means cover in firmly the whole of the lower half of the abdomen.

The rationale upon which this operation acts, is, I believe, as follows: A portion of fascia having been freed from its adjacent part is, by means of the wire, fixed in the canal and rings, so as to form a plug which prevents the gut, &c., from escaping out of the abdomen. By this means, it acts in just the same way as Wützer's operation does; and the only difference between them is, that, in Wützer's, union is attempted between two tissues which were not intended by nature to unite: and in Wood's, parts are united (and that subcutaneously) which are accustomed to be so, naturally. Again, the rings, &c., instead of being forcibly distended, as in Wützer's method, are made of less size, by having their pillars, &c., drawn firmly together, and kept so, by becoming united with the fascia drawn into them.

Wood's operation may be looked upon as simply gliding into another place, tissues which have a natural affinity one for another; and by this means, causing a canal, &c., which has become distended by pressure, to recover its natural calibre.

The reasons that have led me to prefer this operation, &c., to Wützer's,

even with the improved instrument I have invented, are: Firstly, its immense superiority, as founded upon the practical observation of more than fifty cases upon which I have operated in this way. Secondly, by the consideration that a wooden plug, (even with the addition of a movable lower portion,) can never adapt itself to the tissues like soft parts. And thirdly, that there is, and must be, great difficulty in inducing two surfaces to unite one with another, which were never intended to do so by nature—viz., the causing two layers of invaginated scrotal integument to adhere together.

The improved instrument above alluded to I have already described elsewhere; but I now again figure and describe it. Where Wützer's operation is adopted, in cases of scrotal and femoral hernia, with the tissues relaxed, and the rings of large size—say capable of admitting two, three, or more fingers—considerable difficulty and disappointment is experienced in endeavouring to effect a cure by one operation; and it has occasionally happened to others, and to myself, to be obliged to repeat the process.

From practical acquaintance with the subject on the living, and repeated trials and experiments upon the bodies of the dead, who have been affected with hernia, I have been led to believe that the cause of such failures is not attributable to any fault in the theory of the method, but to a defect in the instrument; and, upon the following grounds, submit to the decision of practical test that they would be obviated by the adoption of the accompanying mechanical improvements.

Upon examining a case of rupture in which the operation for the radical cure has failed, (supposing, of course, that it has been properly managed, together with the proper after-treatment,) the rings and the canal will be found to be obliterated, probably to some three-fourths of their extent, or there may be only an aperture which will, with difficulty, admit a crow's quill; and thus, though the patient may be greatly benefited, and with the aid of a truss resume his duties, a *radical cure* has not been effected.

That portion of the canal and rings which have been blocked up, is invariably that which is nearest the abdominal walls. "The gut slips down behind the plug," are the terms in which both surgeons and patients express the mishap which has occurred, and the reasons for this are, I believe, as follows: The anterior or superior layer of the invaginated integument is subjected to, not only the pressure of a wooden plug in Wützer's instrument, to keep it in apposition with the opposed surface of the canal and rings, but also to the direct pressure of the compressor. The compressor exerts its influence exclusively upon the parts included between it and the upper surface of the said wooden plug, and in no wise affects the posterior parts—viz., the posterior layer of invaginated integument, and upper surface of the canal and ring, whose sole chance of being kept in apposition depends upon the accuracy with which the plug fits the canal, &c., as a whole.

The floor of the canal, &c., especially where the tissues are lax, as generally occurs in old and large ruptures, does not present in the same manner an opposing resistance to the wooden plug, as does the compressor; and thus, should the two former be not very accurately adapted the one to the other, adhesion cannot even be expected to occur.

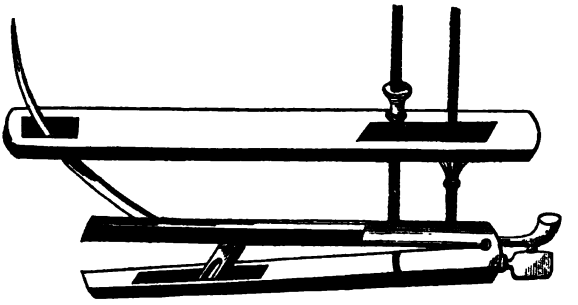
The mouth or internal opening of the canal is funnel-shaped, with the posterior surface the more sloped; consequently, if there be a weak point it will be there, and is sure to receive all the shocks of the gut during the process of cure.

Besides, it is evident that a *cylinder*, even closely applied to the rest of the extent of the canal, cannot fill up its funnel-shaped mouth, but must have an interspace which will be on the posterior surface.

And again, it is not always practicable to introduce to a sufficient extent, a solid plug, which would best fit the internal ring, on account of the resistance of the other tissues to its passage; besides entailing an endless variety of such plugs.

By the adoption, however, of the principle I now propose, viz., a plug, whose lower half is capable of expanding, those difficulties are severally overcome.

A glance at the diagram will render its application at once evident; it will be seen by turning the handle of the dilating screw, and thus causing the lower half of the instrument to expand, that the pressure upon the parts included between the upper portion of the plug and the compressor, is left in exactly the same relations and conditions as in the usual instrument; but, that a force is exerted upon the posterior portion of the invaginated integument, canal, and rings, which it gently, but firmly, retains in complete apposition one with the other.



By reason of the greatest point of its expansion being at the extremity of the instrument and gradually tapering, two objects are accomplished; first, the funnel-shaped mouth and the internal opening is filled with a plug, whose sides are inclined to its own—the invaginated integument being, as it were, modelled upon it; and secondly, the rest of the canal is, at the same time, subjected to no undue pressure.

The principle adopted by Mr. Spencer Wells, is likewise made available, viz., having the transverse diameter of the instrument much greater than the antero posterior, whereby the shape of ring is altered, it being converted into a mere chink, and thus affording an additional security against the descent of the gut; and so leaving as small an amount of space as possible between the opposed surfaces of the *doigt de gant* to fill up when the instrument is removed. A thin india-rubber finger-stall caps the end of the instrument preventing any soft parts getting between the blades.

With regard to the metallic uprights and screws, they are so arranged that the amount of pressure exercised by the compressor may be applied just as is required; an excess may be precisely thrown, when desired, upon the part pierced by the needle, or upon the edge of the *cul-de-sac*, by alternating the screws; or a general and equal pressure may be exercised upon all the parts included between the compressor and the plug.—*Dublin Quarterly Journal, Feb.* 1862, *p.* 60.

71.—ON A NEW OPERATION FOR THE CURE OF UMBILICAL HERNIA.

By Richard Barwell, Esq., Assistant Surgeon to Charing-Cross Hospital.

[In by far the larger number of cases, umbilical hernia is of no importance whatever in infants, but in some the ring does not close, and the chance of spontaneous cure becomes less and less. Thus, in a child of two years old, with an umbilical hernia, not diminishing in size; in the not uncommon navel rupture after childbirth; and in the case of all adults, the operation to be proposed affords a perfectly safe and indeed the sole means of cure.]

There is a condition in infancy which not only warrants this operation, but renders it highly desirable, even although the hernia might very likely get well without such interference. This rupture sometimes produces, without any strangulation, an amount of colic or pain which seriously affects the patient's health; the child is constantly writhing, drawing up its legs, and

screaming; it will not suck quietly, is frequently sick, and gets constantly thinner; the abdomen is hard and tense; the hernia increases in size. These symptoms resist, or are alleviated only for a very short time by medicinal treatment. Such condition is, I believe, produced either from a portion of the colon itself having passed out of the abdomen through the navel, or from there being included in the hernia some piece of the omentum so near to the colon as to bind down that intestine. It is all very well to talk or to write about constantly keeping back such a hernia, and never allowing its protrusion; in practice, we know that such an indication cannot be fulfilled. A surgeon may fasten the bandage perfectly well, but in half an hour or so the child cries, drags in the belly, strains, or makes some other sudden movement, and out comes the hernia, when the screaming and kicking recommence, aggravated probably by the nurse's attempts at bandaging. It is for such cases as these that I recommend the operation in infancy, rather to save the constant pain and restlessness of the child, the daily and nightly wear and tear of the mother, than with the mere view of curing the hernia. The infants upon whom I have operated for this very cause slept well, fed well, and gained flesh from the day of the operation.

The method of performing the operation in sufficiently simple; but a few details should be known. When the sac has been slit up, so as to expose the crevice-like opening into the abdomen, the peritoneum is seen passing into it, crumpled up into folds, exactly as a pocket handkerchief whose middle had been crammed into a small keyhole would be puckered. At the upper end lies the tough remnant of the umbilical vein, and all around outside the sac is a quantity of cellular tissue, more or less loaded with fat, covering and concealing the white tendinous edges of the ring. It is desirable to clear away some of this encumbering tissue to bring into sight the place where the needles are to go, and also to prevent, what may otherwise happen, the mere sewing up of this loose tissue, leaving the real opening unclosed. In cases where the hernia is large, and this material plentiful, it is better to cut away nearly all that tissue, and of the sac also (which I have found to be a perfectly harmless proceeding), otherwise there will be included within the wire, and, perhaps, squeezed into the opening, a lump of soft material that will retard, or altogether prevent healing. All this having been properly arranged, the sutures are to be placed—two, three, or even four may be used, according to the size of the ring. In children, I have, except once, found two sufficient. A Price's needle is the most convenient instrument. Sufficient distance is to be left between the edge of the opening and the entrance of the needle, and a considerable thickness of tissue must be included in the sutures. It is of course most essential to keep the contents of the abdomen out of the way of the needle; and this is best managed by passing the finger as far as possible into the opening, and letting the point of the needle come out of the tissues against it, and then guiding that point carefully across, so as to render any wounding of the viscera impossible. In one case I slightly pared the edges of the opening. I now believe this to be unnecessary, its peritoneal lining being sufficiently ready to adhere.

Altogether the operation has been, in my hands, so successful and so perfectly free from any severe effects, that I can unhesitatingly recommend its adoption in suitable cases.—*Lancet*, *Nov.* 16, 1861, *p.* 471.

72.—ON THE MEDICAL TREATMENT OF STRANGULATED HERNIA.

By W. Blower, Esq., Bedford.

A stout lusty young man, by trade a butcher, who had for some time been affected with an inguinal hernia, but had not worn a truss, strained himself by lifting a heavy pig into a cart, and occasioned a further protrusion of bowel. He attempted to reduce the hernia, but without success; in a short time symptoms of strangulation manifested themselves, and when I saw him

three or four hours afterwards they had become extremely severe. The tumour was tense, painful, and very tender, the constriction and dragging at the epigastrium were distressing, and the vomiting of stercoraceous matter frequent. I sent him six pills, consisting of ten grains of calomel and three grains of opium, and directed that he should take two immediately and one every half-hour subsequently, and also that he should drink a cup of strong coffee without milk or sugar occasionally. When I saw him again, about an hour and a half afterwards, he appeared to be convalescent; the hernia was soft and reducible, and all the unpleasant symptoms had disappeared. He stated that after drinking the first cup of coffee the sickness stopped, and the uneasiness at the epigastrium ceased. And as soon as a truss could be procured and fitted on, he resumed his ordinary occupation.

I have treated several other cases of strangulated hernia in the same manner, and with a similar successful termination; except in one, in which, however, the plan was imperfectly carried out. I was induced to adopt this mode of treatment, by the result of some cases where calomel and opium were administered to prevent the ill-consequences of delay, when the operation was unavoidably postponed for a time. Thus, in a case of strangulated femoral hernia, where the usual accessible means of reducing it had failed, and an operation appeared unavoidable, I requested the assistance of the late Mr. Hurst; but, as he was out and not expected home for a couple of hours, I gave to my patient calomel and opium as a palliative until his return, and at the expiration of that period the hernia was soft and reducible by the slightest touch. In another case I called in Mr. Hurst, but he also failed in reducing the hernia, and deemed an operation indispensable; but as, in consequence of another engagement, he was obliged to leave for about two hours, the operation was deferred until his return, and the same treatment was adopted as a temporiser, with a like curative result. And during the latter period of my practice, this was the usual course pursued by me in all cases (and they were not a few), and, with one exception, with uniform success; and in the case where it failed, the pills were not given as directed, therefore the method ought not to be considered responsible for not succeeding.

The object, in the medical treatment, is to produce a state of muscular relaxation and weakened vascular action. In this condition, the obstacles to the reduction of the hernia depending upon muscular contraction are removed, and the inflammatory processes are checked, so that not only the hernia is rendered reducible, but the tendency to death—the general termination of this affection, if left to itself—is stopped. Many plans for accomplishing these objects have been proposed and used, and not unfrequently with success; but according to my experience, the calomel, opium, and coffee treatment is the most convenient, the least objectionable, and the most successful method, except in children, where the hot bath is the best remedy.

I have tried *bleeding to syncope*, and have succeeded in reducing the hernia, whilst the patient has been in this state; but the loss of the requisite quantity of blood has sometimes been followed by unpleasant consequences. Thus, in the case of a strong agricultural labourer, in whom strangulated inguinal hernia had existed about eighteen or twenty hours when I first saw him, and the symptoms had become very violent, I bled him; but he did not fall down until the third pint basin was nearly filled. The hernia was easily reduced whilst he was in a state of syncope; but it was followed by anæmia and disordered action of the heart, and it was two years before he was able to work again. He lived in a lone cottage, about five miles from Bedford, and, as the disease was very urgent and the lancet was the only accessible remedy, it was obliged to be employed.—*British Med. Journal, Jan.* 11, 1862, *p.* 36.

73.—ARTIFICIAL ANUS RESULTING FROM SLOUGHING HERNIA.

[The patient was 35 years of age, and had left scrotal hernia of forty-five hours' strangulation. The sac was found sloughing, together with the omentum. No intestine was observed. On the third day fæces passed freely from the wound, and on the eleventh he died.]

The lower part of the ileum, which had been the seat of strangulation, was completely divided, the lower end, opening directly into the sac, was contracted, and the upper, fixed at the side of the opening, had no free passage externally.

This example of artificial anus, the result of a sloughing hernia, is a fair sample of a class. Having been left to Nature, the local repair was good, although a free passage from the upper bowel did not exist. The gradual sinking of the patient is the general termination of these cases, exhaustion, as a rule, being the cause of death.

Such cases as the one just quoted afford us some hints for practice, and the question occurs as to the best mode of treating a sloughing hernia; whether it is best to lay the parts open and stitch the bowel to the edges of the wound, or leave the whole to Nature, having freely exposed the gangrenous intestine. In a paper in the "Guy's Reports" for 1856, I showed that the weight of evidence went to prove that the latter practice is the best, and subsequent experience has confirmed me in that opinion. It appears better as a primary point of practice, and also affords the best chance for a subsequent recovery. —*Guy's Hospital Reports, Vol.* vii. 1861, *p.* 55.

74.—ON THE PATHOLOGY AND TREATMENT OF HEMORRHOIDAL AFFECTIONS.

By T. J. Ashton, Esq., London.

Formerly, surgeons were in the habit of excising hemorrhoids, and, as might be expected, the operation was always attended with profuse bleeding, which in many cases proved fatal. In order to obviate this serious objection to the plan, Dupuytren had recourse to searing the cut surface with irons heated to redness, a proceeding which must have greatly added to the painfulness of the operation. Latterly, an attempt was made to revive excision, and to substitute for the actual cautery the application of nitric acid to the bleeding surface. This would certainly be productive of as much pain, without the security from hemorrhage obtained by the use of the hot iron. Sir Astley Cooper lost several patients from hemorrhage, and then abandoned excision. Mr. Syme, referring to Sir Astley's cases, remarks, "If other practitioners had been equally candid, we should doubtless have had more testimony as to the danger of this operation." However, there is sufficient evidence before the profession to deter any prudent surgeon from adopting it. With the intention of obviating the danger arising from excessive bleeding, removal of internal piles by means of a platinum wire, heated to incandescence by the galvanic current, was suggested and tried, but in practice was not found to possess the advantages imagined. Besides, I presume the burning by this means would not be less painful than by Dupuytren's method, and certainly the operation would be less expeditious. From time to time escharotics of one kind or another have been extolled, and had a short-lived reputation; but failing to realize the advantages their advocates advanced, they have successively fallen into disuse, excepting two which are useful in some few cases, and to which I will refer again presently. M. Amussat has recommended what he terms the circular cauterization of the base of the hemorrhoidal tumours, which is effected by peculiar shaped forceps, having grooved blades filled with caustic potash and lime; but English surgeons will not be inclined to have recourse to the plan when told that to allay the pain caused by the operation it is necessary to let a jet of cold water flow on the

parts for several hours, and that one patient suffered so much that he sat in a bath for a whole week. Another plan for the removal of hemorrhoids and other growths emanated in Paris, and became a fashion for a time, but happily, in England at least, is now little practised. I allude to their ablation by that crushing, lacerating, and unscientific machine, the écraseur, which, in appearance and operation, suggests the idea of belonging rather to the torture chamber of bygone ages than of being an instrument of modern surgery. M. Nèlaton reports that many who have been operated on by it are now the victims of traumatic stricture of the rectum.

When speaking of escharotics, I mentioned there are two that may be employed in the treatment of some cases of the affection under consideration: they are the pure nitric acid and the deutonitrate of mercury. These are of use only in the variety of hemorrhoidal tumour in which the morbid tissue does not rise much above the surrounding mucous membrane. Either may be applied in the following way:—The bowels having been previously unloaded, the disease is brought into view by everting the mucous membrane, or by the use of the speculum ani. The acid is then freely applied to the florid granular surface, which is converted into a white eschar. Great care must be taken not to allow the acid to come in contact with the integument, or severe pain will be occasioned. Chalk and water is used to neutralize the excess of acid and prevent injury to the adjacent tissue. I apply the acid by means of a small piece of lint wound round the end of an eye-probe, which answers better than a spatula, glass brush, or other contrivance. The plan of treatment was suggested by Mr. Cusack, and in 1843 Dr. Houston published some observations on it in the 23rd volume of the "Dublin Journal." It is a method very applicable to the particular cases I have just mentioned, but even in those does not always succeed.

In 1858, I operated on a young lady, patient of Mr. Chappell, of George-street. Previously she had lost considerable quantities of florid blood per anum, and had become perfectly anæmic and highly nervous. A vascular excrescence, about the size of a fourpenny-piece, existed on the posterior part of the rectum, with which a large arterial branch communicated, and could be felt strongly pulsating. I applied nitric acid freely, and to all appearance effectually. Her health greatly improved, and she remained free from all local disease for about three months, when considerable hemorrhage again occurred, and an examination revealed a recurrence of the vascular growth. Being satisfied no advantage would ensue from again applying the acid, I had recourse to the ligature, with complete success, there being no return of the disease.

I have seen much mischief caused by attempts to destroy large internal hemorrhoidal tumours with the acid, for the removal of which the ligature is the only proper mode of treatment, and which I shall now consider.

There is a popular idea that the operation by ligature is attended with danger to life, and this impression is also shared by those of the profession whose experience in these affections is limited. Sir B. Brodie, with his characteristic candour, records losing three patients after this operation; and these cases are quoted, or rather misstated, by those who advance some peculiar but not very original mode of treating hemorrhoids. Two of these patients had albuminuria, and died of disease of the kidney; but at the time they occurred the important pathological alteration of structure in the kidney, indicated by the presence of albumen in the urine, was not known. In the other case the patient's constitution was so shattered that Sir Benjamin at first refused to operate; and it was only after fully pointing out the danger, and the patient insisting the operation should be performed, that he consented to do it. These cases militate in no way against the operation by ligature, but forcibly illustrate the necessity of looking beyond the local affection; and if the presence of serious organic disease is detected, we must abandon all thoughts of a radical cure, and have recourse to palliative means only. From experience I can affirm that the application of the ligature is a perfectly safe and effectual mode of treatment, provided due attention has been previously given to the constitutional condition of the patient, and that

the abdominal, thoracic, and cephalic viscera are free from serious organic disease. The ligature I prefer is a thin hempen cord well waxed, and one half stained so that when divided the respective ends are readily seen. The needle I generally use is fashioned like a nævus needle, but having a greater curve, and with this the tumour is transfixed from above downwards. In some cases the tumour is better transfixed from below; then a needle less curved is better.

The failure of medical treatment, and the certainty that the disease will become seriously aggravated the longer it exists, render its removal by operation absolutely necessary. Various causes influence a patient to seek the aid of the surgeon: it may be the annoyance and discomfort of the constant mucous discharge; the frequent desire to defecate induced by the presence of the tumours; the great discomfort occasioned by the constant prolapsus; the frequency and severity of the pain experienced in some cases of this disease; or the debility, palpitation of the heart, and other serious constitutional effects resulting from frequent and excessive hemorrhage. It is premised that, previously to operating, the bowels have been thoroughly emptied; the tumours are then made to protrude by the patient sitting over some warm water and straining, or by the use of an enema. To apply the ligature, the patient should lie on a bed, with the nates projecting over the edge, and the knees drawn up. Provided there is but one tumour, and that is small and pendulous, it may be included in a single noose applied round its base; but I have met with no case in which I have not deemed it better to pass a double ligature through the tumour. I have heard of cases where large tumours have been included in a single noose, which, after partially cutting through them, has slipped off, rendering a second, and even a third, application necessary—with what suffering to the patient I leave to be imagined. The tumour being of the size ordinarily met with, an assistant takes hold of it with a vulsellum, or with a pair of forceps, and draws it well down, so as to bring as much of it into sight as possible. If it encroaches on or implicates the margin of the anus, the lower part must be incised, which will save the patient much pain, and the separation of the ligatures will be expedited. The surgeon then passes the forefinger of either hand to the upper margin of the tumour, by which he regulates the entrance or exit of the point of the needle, according as he transfixes from above or below. The needle being passed, the ligatures are drawn through, and then divided; next they are to be tied, and they should be drawn as tight as possible, and secured by a double knot. Sir Astley Cooper recommended that they should not be drawn tight, with the intention of causing less pain; but this was a great error, and he actually produced that which he was desirous of avoiding. The object of the operation is the destruction of the morbid portion of the mucous membrane by cutting off all vascular and nervous supply; and this is necessarily more speedily and effectually accomplished by drawing the ligatures as tight as possible. Care must be taken that the ligatures are accurately applied to the base of the hemorrhoidal tumour, so as to include the whole of it without encroaching on the healthy tissue. This is managed by means of a pair of forceps, or by a three-pronged sharp hook. All tumours that exist should be operated on at the same time; for if they are not, inflammation and intense suffering will ensue, besides leaving the patient doomed to a return of his former misery. It is advised by some authors, and even by a recent writer, that when several tumours exist not to operate on more than two. If this recommendation is acted on, the result will be what I have just mentioned. After the operation, from thirty to sixty minims of the tincture of opium should be prescribed, which has the effect of tranquillizing the patient; but the chief object aimed at is to constipate the bowels for a few days. On the third morning, an aperient should be administered, unless the bowels act of their own accord; subsequently, if necessary, they must be moved by the use of an enema or some mild purgative every second day. The ligatures usually come away between the fourth and eighth day; and the healing of the ulcers left may be expedited by the application of a stimulating ointment, which is readily and painlessly effected by means of a glass syringe having

a jet with a large aperture. For the first few days the diet should be nutritious, but not solid; after the bowels have been relieved, solid meats may be allowed. The patient need not be confined to bed many days; but I insist on the recumbent position being observed till the cure is complete.

Having detailed the plan of treatment of hemorrhoids, in conclusion I will briefly recapitulate the principal points. That many cases will yield to judicious medical treatment; that when it fails, presuming the patient is free from serious organic disease, the hemorrhoidal tumours may be removed with perfect safety, and a moral certainty of a successful result, provided that any defects of the constitution have been remedied and the bowels freely unloaded previously to the operation being performed. That in cases of external hemorrhoids and internal ones implicating the integument, excision and incision is the only proper operative treatment. That to internal hemorrhoids the ligature should be used, except to the vascular excrescence of the mucous membrane I have described, and to which nitric acid may be applied. By acting on these principles, patients may be relieved from these affections effectually and with perfect safety, and that pyæmia, tetanus, and other serious consequences are not by any means likely to occur.—*Lancet, April* 12, 1862, *p.* 379.

75.—CASE OF SUCCESSFUL REMOVAL OF THE ENTIRE TONGUE FOR CANCER OF THAT ORGAN.

By Thomas Nunneley, Esq., Lecturer on Surgery in the Leeds School of Medicine, &c.

(Read before the Royal Medical and Chirurgical Society.)

The subject of the case related in this communication is a man, aged 35, who states that, with the exception of the disease in the tongue, he never had a day's illness. In early manhood he was not very steady; but for the last twelve years he has been so. He has been married many years, and has had eight children, the youngest being only six months old; all of them have been strong and healthy. There is no reason to suspect any syphilitic taint. For some years he has been employed on a railway, and latterly as a guard. From being a large, strong man, owing to the distress and pain he has suffered and the inability to masticate food, he has become thin, weak, and anæmic. The disease, at first slow, had latterly made more rapid progress, and having not unfrequently been compelled to pass the whole day without food, owing to the pain the attempt to take it occasioned, he was wishful to submit to any operation which afforded a chance of not dying from starvation, which he anticipated would otherwise be the result. He had been subjected during nearly the whole of the three years the disease had existed to treatment of various kinds, both general and local, all of which he thought had been rather injurious than beneficial.

The disease, which was first perceived on one side of the tongue, had invaded the whole structure of the organ as far as the base. In the centre of it was a deep ulcer with irregular edges; the margin of the tongue was tuberculous and irregular, and the whole substance was dense, unyielding, and very hard; the size was somewhat, but not very greatly, increased; there was a constant offensive taste, owing to the discharge from the ulcer; the speech was considerably impaired. As removal of the entire organ, though difficult and dangerous, afforded the only means of recovery, and as the patient was quite willing to submit to the risk, the operation was determined upon.

Having experienced the difficulty of effectually strangulating any considerable portion of the tongue, and the distress it occasions, Mr. Nunneley was unwilling to employ ligatures for the whole, and resolved upon using the écraseur, first cutting an opening for it between the lower jaw and the os hyoides as deeply as could be done without fear of hemorrhage, and also with the object of having as little structure to crush through as possible. In accordance with this, a small transverse incision was made just anterior to the os hyoides through the integuments, mylo-hyoid and genio-hyoid muscles, by which a

curved needle, sufficiently large to allow the chain of the écraseur, which was attached to it, to follow easily, was carried from the central line obliquely towards the left side of the base of the tongue into the mouth, and out of the mouth at a corresponding point on the right side, through the same external aperture by which it had entered. The chain thus carried across the extreme base of the tongue was then attached to the racket-bar, and the écraseur set in motion, when, owing to the defective rivets connecting the link of the chain, before any strain whatever was put upon the instrument, two of them dropped asunder. All attempts to make them hold were ineffectual, and it had to be abandoned. Threads of strong, fine whipcord were at once carried along the same course as the chain had followed, and tied, both as single and double ligatures, with all the force that could be exerted.

At first the patient suffered very greatly; he could neither swallow nor talk. Nutritive enemata, containing acetate of morphia, according to circumstances, were given, with great comfort and advantage. On the fourth day he was much improved. On Oct. 10th, though the ligatures had cut so far into the base of the tongue that they could neither be seen nor felt, yet, as the organ was regaining a more natural appearance, a very strong ligature of silk whipcord was tied round the base as tightly as possible, and another cord carried through the fissure cut in the base round under the frænum so as to constrict the sublingual attachments, with the effect of apparently well and effectually strangulating the organ. He again improved so much as to require but little morphia, and was able to swallow liquid food with comparative facility, until the night of the 15th, when hemorrhage to the extent of a pint of blood came on. Though this was arrested by a solution of permuriate of iron, it recurred frequently, and though in smaller quantities, and always at once checked by the solution of iron or tannic acid, he was so much reduced that it was determined, on the 17th, to place another ligature round the base of the tongue. This effectually arrested the bleeding for fifty-four hours, when it again returned so freely that ligatures were carried round both the base and under the frænum in the same manner as on the 10th. On this occasion, however, a new material (which was exhibited to the Society) was advantageously used. There was no more bleeding until the 23d, when it again recurred, to some extent, three different times, and necessitated the reapplication of the ligatures in both situations. Not the least pain was experienced on this occasion, showing the nerves had already been divided. There was no subsequent return of the hemorrhage. On the 30th two other ligatures were applied, no pain whatever being caused in so doing. On Nov. 2nd, as the tongue was still firmly attached, though apparently cut through at the base, a twisted double steel suture wire was carried round it under the frænum, and, by means of a suitable apparatus, twisted well home until it broke, without, however, cutting through the tissues. This plan was repeated on the 3rd with a much stronger wire, (a strong piano-forte string,) which also gave way before completely severing the connexions. A repetition of this on the 4th cut through the sublingual tissues, and the tongue was free. Neither pain nor the loss of any blood occurred in any of the last four or five operations. There was so little wound to heal that it was well the next day, proving that, as suspected, the base had been cut through for some few days. A mass of knotted thread and wire was removed on the following day; but two or three threads were so deeply imbedded, and so fast, that many days elapsed before the last was taken away. The knots were perfect, showing how great an amount of force had been exerted without complete strangulation being effected; proving also that, though the base of the tongue be completely cut through, the sublingual connexion alone is sufficient to maintain the vitality of the organ.

From this time the patient may be said to have been well. The next day he ate with great gusto a hearty dinner of roast duck. He rapidly improved in flesh and strength. The facility with which he can masticate and swallow is surprising; indeed he says that he can do so much better than for the two previous years. The deglutition of liquids, as might be supposed, is more impaired than that of solids. The sense of taste is enjoyed in a very con-

siderable degree, while the power of articulation is great beyond all expectation. He can pronounce every letter of the alphabet, many of them perfectly (all the vowels), most of them distinctly. The three there is the most difficulty in are K, Q, and T, which are difficult and indistinct in the order they are named, K being much more so than T. In conversation he can be readily understood, if not excited and hurried; if he be, some words are indistinct, otherwise his power of articulation is sufficient for all purposes of intercourse, so much so that it is probable he will be appointed master of one of the smaller stations on the railway.

After referring to the danger which he believes must always be incurred by following the plan of extirpation practised by Professor Syme, which inflicts so great an amount of mischief not directly involved in the mere removal of the tongue, which itself is sufficiently serious, and to the pain, delay, and danger of hemorrhage incurred by the use of thread ligatures, the author concluded by recommending in preference the écraseur, introduced by a submental opening made by a knife through all the tissues, which would not occasion much bleeding; and, when from any cause this instrument is not employed, after a deep fissure has been made in the base of the tongue by a submental ligature, an earlier use of strong wire ligatures than was adopted in this case.

MR. MOORE did not think that the author had clearly shown the disease to be cancer. The signs, as hardness, ulceration &c., were common to several diseases of the tongue. Mr. Moore alluded to one case in which extensive ulceration and hardening of the tongue was cured by arsenic. The duration of the disease in the author's case, and the fact that it did not affect the glands, were in opposition to the view that the disease was cancer; and the members could not, from the evidence brought before them, confirm the author's diagnosis. At the same time, it must be remembered, that a person may see many things which, to his practised eye, are convincing, and yet which he does not think it necessary to put on paper. He did not think that the operation would be done again. In operations on the tongue the risk might be diminished and pain saved by previously dividing the nerves and securing the vessels.

Mr. PAGET said that the chief point in the paper was to show that the whole tongue could be safely removed. It showed also in this case how great difficulties arising in an operation might be overcome. He (Mr. Paget) thought that a good substitute for the chain would be found in whipcord, and it had this advantage, that it might be altered in its course by needles so that it would cut on a curve. He also could not agree with the author in his diagnosis. The description of the disease seemed to be that of tertiary syphilis rather than of cancer.

Mr. NUNNELEY said, in reference to the supposed syphilitic nature of the affection of the tongue, that there was no history whatever of syphilis, although the patient owned to having had gonorrhœa. The patient's family were quite healthy. The man had been on one occasion salivated, but no improvement followed. He (Mr. Nunneley) could not understand how it could be supposed that the case was one of syphilis.

At the close of the meeting it was suggested that it would be of interest if the patient were to be carefully examined to ascertain if any evidence of syphilitic infection could be discovered. The man was undressed and very carefully examined by Mr. Henry Lee and other members, who were perfectly satisfied that there was no indication of any such taint.—*Lancet*, *Dec.* 21, 1861, *p.* 594.

76.—ON DIVISION OF THE GUSTATORY NERVE, AND ON LIGATURE OF THE LINGUAL ARTERY, IN THE TREATMENT OF CANCER OF THE TONGUE.

By Charles H. Moore, Esq., (Abstract of a paper read before the Royal Medical and Chirurgical Society.)

The principal object of the author was to revive an operation which had been devised by Mr. Hilton, once practised by him in the year 1850, and though of much value, never again performed. (Guy's Hospital Reports, second series, vol. vii., page 251.) Amongst the many sources of the peculiar painfulness of cancer of the tongue, irritation of the fifth nerve could be assigned as occasioning the pain of so much of the tumour as was in front of the fauces; the tenderness of the ulcer, the pain in the regions of the parotid, ear, temple, and crown of the head, and the excessive secretion of saliva. All these being traceable to the encroachment of the disease on the gustatory branch of the fifth, the section of that nerve between the disease and the brain should relieve them.

The operation, as practised by Mr. Moore, consisted in cutting through all the soft structures on the inside of the ramus of the jaw by an incision, commencing immediately behind the last molar tooth, and extending three-quarters of an inch in a direction towards the angle of the jaw. The only structures which could be divided by such an incision were the mucous membrane and a part of the mylo-hyoid muscle, with the gustatory nerve descending forward between them, about half an inch from the tooth, and nearly at a right angle with the direction of the incision. It was advisable to operate with a curved knife, as the alveolar ridge might shield the nerve from the edge of a straight one, and also to cut outwards quite to the bone.

The author had operated in five cases of cancer of the tongue, three of which were given at length. The relief was immediate. Salivation, the pain and tenderness of the tongue, and the reflected irritation of the fifth nerve, were all gone at once. Soreness of the wound, with swelling, remained for some days; but after that the patients took food, swallowed, and spoke with comparative ease. They slept, and improved in general health. The tongue in each case was absolutely insensitive on the side operated on from the anterior pillar of the fauces forward, and no sapid substances aroused taste in those parts. One patient frequently, on awaking from sleep, found himself chewing the cancerous mass in his tongue between his toothless gums, but the compression occasioned him no pain. The relief was permanent so far as the gustatory nerve was concerned; but when the disease invaded the area of the glosso-pharyngeal new pain arose. One patient had been operated on in August, and had no return of pain up to the present time—a period of three months. In the last-mentioned case the author had attempted to benefit the patient, in whom extirpation was unsuitable, by withdrawing from the tumour its nourishment of blood, in addition to its nervous influence. He accordingly tied the lingual artery on the side of the disease two days after he had divided the gustatory nerve. The ulcer became paler after the operation, but neither sloughed nor healed, and in five weeks the whole tumour was perceptibly smaller than before the operations. From that time the tumour began again to increase. The author had desired, upon the renewal of the growth in the tumour, to tie the opposite lingual artery also; but the patient was content with the amount of ease which he was enjoying, and indisposed to incur a renewal of the pain which had followed the first operations.—*British Med. Journal, Dec. 7*, 1861, *p.* 618.

77.—ON CLEFT PALATE.

By A. G. Field, Esq., Brighton.

[In the commencement of his paper the author says that he can scarcely imagine any defect of the hard palate so great, but that steady perseverance,

with a moderate amount of skill, on the part of the surgeon, and a fair amount of fortitude and patience on the part of the patient, may not lead to a perfect cure. There has been a little controversy as to whom the credit is owing, of having first proposed the use of the periosteum of the hard palate to supply a congenital partial deficiency of that part. Mr. Field says:]

My own experience has led me to conclude that the principal reason of failure is too much having been attempted at one time—Nature has been asked for too much at once, and so has refused the demand altogether, which she would readily have yielded in instalments. I think all who have operated much in this direction will bear me out in this assertion, that unless the opening in the hard palate is very small indeed, its closure must be the result of more than one operation.

Another point it is desirable to notice. The idea of separating the periosteum for the purpose of strengthening the flaps in the operation of staphyloraphy is certainly not new. My first instructions in operations on the palate were received from my friend, Mr. George Pollock, many years ago, and I well remember he particularly urged the necessity of keeping the instrument close down upon the bone while flaps were being formed, and taking all that could possibly be got to make them with, including as much as possible of periosteum, and for this purpose he frequently used a blunt instrument, precisely like one which I have myself employed for the last six years. I do not know that it matters very much to the cause of surgical science whether this improvement in the operation originated in Germany, or in England, but as it has been considered of sufficient moment to claim the attention of your readers, I beg to state that it was known to me many years ago, and that I derived my knowledge from the source I have mentioned.

Case of Congenital Cleft of Hard and Soft Palate.—Complete Closure by Operation.—F. W., aged 19, had congenital fissure of the palate, extending through the velum forwards to the incisive foramen. The space was about an inch wide at the junction of the hard and soft portions, but the walls of the hard palate were nearly vertical, and covered by a good thickness of soft structures. The divided halves of the uvula approximated each other, but did not meet on an effort at deglutition being made (partly by the action of the divided azygos uvulæ, I suppose), though very widely separated when at rest. He suffered the usual inconveniences from this defect. His voice was nasal, and his utterance very indistinct. Fluids always escaped in a greater or less degree through his nose when he attempted to swallow them. Altogether the case was an excellent one for operation, so after improving his condition by extra diet for a few days, I operated on May 12, 1856. This was the first congenital case I ever attempted, and I did what I would not again, inasmuch as I closed the hard and soft palate at one time; the consequence was a prolonged operation and its attendant ills. The levator palati was divided by passing a knife through the soft palate, and the palato-pharyngeus was cut through by means of a blunt-pointed pair of scissors, while traction was made on the flaps by a thread passed through them, the ends being held in one hand, while the other was employed in the division of the muscle. Some considerable quantity of blood was lost during the operation, more than should be, owing to the extent of surface exposed at one time; but the bleeding was controlled as it occurred, partly by holding lumps of ice in the mouth, and partly, when necessary, by pressure with the tip of a finger. The flaps were united by eight silken sutures.

Twelve days after, all the sutures having been removed, the soft palate had united firmly excepting the uvula, and some union had also taken place in the fore part of the fissure, but an opening still existed through which a finger might be passed into the nose.

June 20, 1856. The edges of the opening were pared with a small scalpel, lateral incisions were made quite down to the bone, and the parts included between these incisions separated from the palate processes (certainly including much of, if not all, the periosteum). The opening was then closed by three silk sutures, the lateral incisions being filled with cotton wool.

The uvula, after the necessary quantity of mucous membrane had been removed, was united by means of one suture passed through its lower part, from right to left, a split shot being attached to each extremity. The uvula united, but a diminished opening remained in the hard palate, for the closure of which the operation of June 20, with slight modifications, was twice repeated within the succeeding two months, and it was not till I put in practice the principle which I advocated in this journal on a former occasion, that I finally succeeded in completely closing the fissure. This happy result was attained in the following manner:

I quote from the published report of another case:—"Having completed the other steps of the operation in the same way as before, the parts were carefully dried and washed over with a solution of gutta percha in chloroform, and a thin sheet of gutta percha was applied over the whole surface. This I did because I believed failures in my previous efforts to obtain complete union depended on a portion of mucus being drawn from the nose between the edges of the flaps in the act of swallowing, which it is impossible for the patient wholly to avoid. This belief was strengthened by observing, after the failure of a previous operation, that a shred of mucus was drawn further into the mouth after each time the patient closed it for the purpose of swallowing the saliva which collected while she held her mouth open for examination, and I imagine it occurs in the following manner: When the first part of the act of deglutition has been accomplished, and the morsel has passed beyond the reach of the tongue, the upper surface of that organ is left accurately applied to the roof of the mouth, and its return to the position it usually occupies is effected by the genio-hyoglossi muscles, which being inserted near the median line would, by their action, depress the middle of the tongue first, whilst its sides were still in contact with the palate; by this means a degree of suction would be produced whereby the loose edges of the flaps would be liable to displacement, and to have the nasal secretions drawn down between them into the mouth, preventing union of their surfaces, however accurately they may have been adapted.—*Med. Times and Gazette, Jan.* 18, 1862, *p.* 57.

ORGANS OF URINE AND GENERATION.

78.—ON CONGENITAL STRICTURE OF THE URETHRA.

By James Syme, Esq., Professor of Clinical Surgery in the University of Edinburgh.

[The patient was 22 years of age and stated that from his earliest recollection he had had great pain and difficulty in passing water. He had been repeatedly treated by dilating instruments but the symptoms produced by the stricture invariably returned, and when in Australia a collection of matter formed in the perineum, and burst, urinary fistula remaining.]

On examination, I found that most of his urine passed by the perineal opening: and that anterior to the scrotum, about two inches and a half from the orifice of the urethra, there was a very tight stricture, which would not allow the smallest bougie to pass, and only admitted with difficulty a slender probe, of which the point was considerably smaller than that of one in ordinary use. At the seat of contraction, the urethra was thickened so as to form an oval shaped swelling, of the size of an almond, and of firm consistence, similar to that which usually attends resilient or unyielding strictures in the anterior part of the canal, but of more than ordinary extent. From the indication of obstinacy thus afforded, together with its history, I concluded that the case did not admit of benefit from any attempt at dilatation, and therefore resolved to divide the part by a free incision. With this view, I employed the slender instrument that had been passed through the stricture, and which, gradually enlarging from the point backwards, carried a groove on its thicker part. Into this I inserted the point of a knife below the urethral thickening, and pushed it forwards, so, as I thought, to effect complete division of the stricture. Instruments of moderate size were then passed into the bladder,

showing that there was no other obstruction in the canal, but not with the perfect facility and freedom usual after such an operation. I trusted that what remained of the stricture would yield to dilatation, and was no less sorry than surprised to find that little improvement resulted from what had been done, as the urine did not issue with more freedom from the orifice of the urethra, and continued to escape not only by the fistulous opening in the perineum, but also by the small opening which I had made.

On September 6th, feeling that the urethra was thickened at each extremity of the incision, I again introduced a director, and pushed the knife along its groove in both directions. No decided change was thus produced; and, notwithstanding the continued use of bougies, which still encountered difficulties in their introduction, the state of matters appeared so unpromising that I began to fear there must be an obstacle to recovery in some other part of the urinary organs. But, before abandoning the case as hopeless, I resolved to try the effect of cutting through the whole constricted part of the canal in such a way as would more certainly ensure its complete division; and, therefore, instead of pushing the knife along the groove against the thickened texture, which by its toughness might elude the edge, however keen, I introduced the blade between the integuments and the urethra, so as to enter the stricture beyond the anterior extremity of the part concerned, and then cut downwards, so as to penetrate with certainty through the whole extent of induration. A full-sized catheter was then introduced, and retained three days.

This was done on October 29th, and produced a very different result from those of the two previous operations. The urine soon resumed its proper course; the fistulous openings closed; and instruments of the full size passed without the slightest difficulty or feeling of constriction. At the end of a month, the patient was in every respect perfectly well, and left the hospital without the slightest trace of his life-long misery.

Of the points which this case illustrates, the one most important, in a practical view, is the risk of failure which it indicates from the tough texture being pushed aside instead of freely cut by the knife. It seems not improbable that many results of the operation may thus have been rendered unsatisfactory; and I would therefore suggest that the method adopted in the circumstances just related should be always employed. For this purpose, the groove of the director should extend about an inch into the thick part; and I have had the instrument made so accordingly.—*British Medical Journal, Jan.* 4, 1862, *p.* 1.

79.—ON PRACTICAL LITHOTOMY AND LITHOTRITY.

By Henry Thompson, Esq., Assistant Surgeon to the University College Hospital.

[From various sources Mr. Thompson has collected and availed himself of no less than 1500 cases. The interest of the subject of lithotomy seems to have sprung up afresh during the last few years, seeing that, in the writings of fifty or sixty years ago, no mention is made of any other operation than the ordinary lateral operation.]

The various procedures employed under the name of lithotomy must be ranged in two separate classes—namely:—

1st. Operations by which the bladder is reached from the perineum; and among these I shall notice several principal methods, which will sufficiently include minor modifications.

2nd. An operation performed above the pubes, and known as the high or supra-pubic operation.

I. *Operations performed in the Perineum, or Perineal Lithotomy.*—These are of various kinds, but all may be classified either as lateral or as central operations.

Lateral operations are those which are confined within one of the lateral divisions of the perineum. The incisions are directed between the central and lateral muscles of the perineum. They necessarily approach the pubic ramus,

the pubic artery and its branches, and are directed transversely to these latter near to their origin from the arterial trunk. They involve one side of the prostate gland—it may be nearly to its full extent; while in children, and in exceptional adult cases, they go beyond it.

Central operations are those in which the incisions are limited to the central part of the perineum, are made in the line of the raphé itself or transverse to it, and lie mainly between the anus and the symphysis pubis. They do not approach the rami or the great vessels, nor do they run transversely near to the origin of the branches from the pubic artery. In no case do the incisions reach the external limits of the prostate gland. The distinction is important, and has an intimate relation to all that follows. It indicates a principle by which to distinguish and classify all the proposals made for the performance of lithotomy in the perineum.

Lateral Operations.—There is one proceeding only which will be regarded as the type of this class—namely, the operation at present most generally practised in this country, and universally known as the lateral operation. Although in different hands some of its details vary, the general outline and character are the same in all.

Central Operations.—The "median" operation:—The stone was removed by a median incision in the perineum, made close to and parallel with the raphé in the old Marian operation during the sixteenth century and long afterwards. But the far greater success which the lateral method realised led ultimately to total extinction of the Marian. The disastrous results of this procedure were due, however, to the violent laceration of the neck of the bladder and urethra by which room was made for the extraction of the stone, and not to the median incision of the urethra, which was all that the knife effected in that operation. This distinction was, nevertheless, lost sight of by most lithotomists, until the Italian surgeons at the commencement of the present century, drew attention by their success to lithotomy in the median line. Mr. Allarton brought it into notice in this country, publishing in 1854 his own method of performing the operation, and it has been generally spoken of here as "Allarton's" or as the "median" operation.

Dr. Buchanan's method, in which he uses an angular staff, first employed by him in 1847, may also be classed under the head of central operations, although it is capable of becoming a lateral operation when required.

The bilateral operation of Dupuytren—so named because the incisions, both external and internal, involved equally each side of the middle line—is, notwithstanding its name, a central rather than a lateral operation.

The medio-bilateral—a combination of the two preceding operations, as the term implies—was made known by Civiale in 1836, having previously been practised by him, and subsequently by himself and others to a considerable extent, in France.

Lastly, the recto-vesical operation, which is a continuation of the median operation from the perineum into the rectum.

The problem has been presented for years, and is still presented with increasing force: Is the lateral operation the best mode in all cases of extracting through the perineal region a calculus from the bladder? That its results have not satisfied the expectations, or at all events the wishes, of many surgeons, is evinced by the numerous efforts which have been made from time to time, by the most experienced operators, to discover a better method. Witness the recto-vesical operation, the bilateral and the median operations, and the medio-bilateral of Civiale; to say nothing of the endless lesser modifications which have been many times proposed, forgotten, and reproduced. All of these appear to be the result of a conviction, which has gained ground during the last fifty years, that certain dangers incident to incisions made in the lateral division of the perineum may be avoided by confining the incisions mainly to the centre. It has been believed that severe hemorrhage may be thus avoided—that the capsule of the prostate will not be divided, and consequently that the cellular interspaces between the pelvic viscera are less likely to be opened by incisions in the centre than on one side only of the perineum. Unquestionably it must be agreed by all that the anatomical

necessities of the region demand that in the preliminary incisions of all perineal lithotomy, lateral or central, the upper and outer parts of the perineal space are to be avoided, and that the deep ones should be confined within the limits of the prostate gland. But there is another necessity, not an anatomical but a vital one, which must also be complied with, and is no less important—namely, the fact that the internal opening must be sufficiently free to admit the stone to pass without the exercise of so much force as to endanger the neck of the bladder. It is the clashing of these two opposed considerations which will always maintain a difference of opinion and of practice in lithotomy. One school will be most influenced by the dangers to which anatomy demands attention; another school will be most impressed with the vital injury which the tissues suffer when the incisions have been too limited. The difficulty is to reconcile them; to find precisely the safe medium of action. It must vary in different cases, and the judgment of the operator must decide the question in each.

It was under the influence of anatomical considerations that Dupuytren was led to devise his "bilateral" operation. To avoid the course of the pudic and its branches, the bulb, and the rectum, and, by two minor incisions springing from the central axis, to avoid any division of the prostatic capsule, he planned the procedure first performed by him in 1824.

It was on similar grounds that Civiale practised a method which he describes in his "Parallèle" (1836). It is remarkable that this operation appears not yet to have been described in this country, especially considering its near relation to the median operation, and the fact that this has been so much discussed of late years. I have witnessed the successful performance of it in his hands; and have myself performed it in a case which occurred during the autumn of the past year, and which turned out exceedingly well. My colleague, Mr. Erichsen, held the staff; and I did the same for him a few days afterwards in a case of his own, in which he performed it also. It should be stated that, in his last edition of his well-known work, Mr. Erichsen has suggested precisely this mode of operating as an improvement on the median, although he had not at that time put it in practice.

A staff with a median groove is introduced, and firmly held against the pubes by an assistant. An incision, about an inch and a half long, is made in the raphé of the perineum, in front of the anus, and carefully carried down towards the staff, endeavouring to avoid the bulb, until the membranous urethra is reached. This is opened by an incision sufficient to admit the end of a double-bladed lithotome, resembling that of Dupuytren, but straight instead of curved, into the groove of the staff. The lithotome is pushed steadily onwards into the bladder; then opened and withdrawn directly outwards in the middle line, dividing the prostate horizontally as well as the deep fascia in its course. The breadth of the internal or horizontal incision is less than in Dupuytren's operation, in which the two blades of the lithotome were extended so as to be an inch and a quarter to an inch and a half apart. Civiale advises they should ordinarily be one inch apart, and never, or very rarely, more than inch and a quarter.

The Median method, known as Allarton's operation, consists in a longitudinal incision in the raphé through all the tissues between the apex of the prostate and the skin, usually made by transfixing with a straight bistoury, and cutting upwards. The operator dilates the neck of the bladder with his finger; and Mr. Allarton has recommended, if this be insufficient for the purpose, the use of Dr. Arnott's hydraulic dilator. A branched metallic dilator has also been employed, where the stone is large. It appears, however, very undesirable to use mechanical apparatus to dilate the structures at this spot, and I believe it to be far safer to make an additional section when necessary.

Of all the cutting operations for stone, it is unquestionable that the median still presents that in which the bladder is reached with the smallest amount of section by the knife, and it appears to me to become dangerous just in proportion as injury by laceration, or over-pressure under the name of dilatation, is superadded to the incisions. These latter involve the bulb to a small degree,

which is the only structure of importance divided by the knife besides the prostate, and this latter is only slightly notched at the apex in the ordinary mode of performing the operation. But when the deeper parts of the wound feel more than usually rigid and unyielding, or when the stone proves to be larger than was anticipated, it seems desirable to make an additional incision for the purpose of affording more space. Unless this be done, the opening is certainly confined, and does not safely afford room for any but stones of moderate size.

After all, the anatomical axiom laid down at first must not be forgotten—viz., that any operation the incisions of which lie altogether between the anus and the symphysis pubis, even although aided by some lateral section, do not afford an opening sufficiently capacious for the safe removal of really large stones. Contrast the want of space in this part of the pelvic outlet, caused by the converging pubic rami, with the room which exists in one of its lateral divisions, and the truth of this assertion will be manifest.

Dr. Buchanan of Glasgow called attention in 1847 to the advantages of operating in the central part of the perineum instead of the lateral, and of employing a staff of rectangular form on which to make the incisions. This is introduced into the urethra, and by means of the left forefinger in the rectum the angle is made to correspond in situation with the apex of the prostate, so that the gland can be felt just beyond, between the finger and the staff; the latter being well depressed, the angle is brought near to the surface, and is readily felt in the perineum. The staff is then carefully maintained in this position by an assistant; the operator, keeping his finger still in the rectum, enters a bistoury opposite the angle of the staff, and therefore immediately in front of the anus; he holds it in his right hand with the palm upwards, the blade horizontal and its edge directed to his right; and he pushes it straight into and along the groove as far as the stop at its extremity. He thus enters the bladder at once, taking care to keep the blade parallel with the horizontal or grooved portion of the staff throughout the whole of the thrust. Next, he withdraws the bistoury slowly, and, as he does so, cuts outwards and downwards a distance rather more than equal to another breadth of his blade, and then directly downwards to the same extent, describing in this manner a curved line equal to about one-fourth of a circle round the upper and left side of the rectum, in which his finger still remains.

Thus far it is, as expressed by Dr. Buchanan himself, about equal to the half of Dupuytren's operation. If the stone is large, an incision made on the right side of the prostate has the effect of converting the proceeding very nearly into the operation referred to. But it is also easily capable of extension in another way; since if the incision by the side of the rectum and through the prostate be continued much beyond the extent described in the original direction, it becomes a lateral operation placed very low in the pelvic outlet. I have given it in some detail, because there is no description of it in our language: I am indebted to one from the French, kindly sent me by Dr. Buchanan, which, he says, is the best existing.—*Lancet, Feb.* 15, *and March* 1, 1862, *pp.* 167, 218.

80.—ON THE CAUSES OF DEATH FOLLOWING LITHOTOMY.

By Henry Thompson, Esq., Assistant Surgeon to University College Hospital.

[A careful consideration of numerous facts leads the author to take views of this question which differ from those generally held respecting it. Undoubtedly the first and most frequent cause of death in the adult is acute inflammation of the tissues, especially of the loose cellular tissue around the neck, base, and sides of the bladder. This may be caused by urinary infiltration, owing to mechanical violence, or to too deep incisions, or by want of reparative power in the patient.]

1. By mechanical violence inflicted in the removal of the stone, especially when the opening is of insufficient size.

The majority of authors affirm that infiltration of urine is the most common cause of death—a statement that I venture not only to call in question, but to regard as the source of serious error in practice. Infiltration of urine is one of the causes of suppuration and destructive inflammation of the perivesical cellular tissue; but it is by no means the universal one. The doctrine based on this belief is as follows:—If the internal incision passes beyond the limit of the prostate in any direction, so as to open up the cellular interspaces behind the deep fascia, urine is almost certain to find its way into them; and, if it does so, fatal inflammation will result: in order, therefore, to avoid this danger, the internal incisions must be extremely limited. But, sound as the principle is to keep the internal incision strictly within the prostate in adult patients, in practice I am satisfied that the desire to limit it has been carried to an extreme degree; and that another and not less serious danger of arousing inflammation of the same cellular tissue has by this very means been increased—I refer to the danger which attends an attempt to drag the calculus through an opening of insufficient size. From what I have seen of the practice of lithotomy in various hands, in town, in the country, and abroad, I am persuaded that insufficient internal incisions are equally dangerous with those which are too free; and that the tendency of the present day is toward the former extreme. The purely anatomical view of the subject appears just now to be in the ascendant. The *vital* attributes and dispositions of the organs involved are not sufficiently regarded. The student is taught to fear beyond all things an approach of the knife to the peripheral limit of the prostate; and, in overdread of cutting it, he barely divides the prostate at all. Hence the no less dangerous injury which results from violence inflicted by the forceps and by the stone upon the neck of the bladder, and from the powerful traction upon it, which injures, often irreparably, the loose cellular connexions in which the viscus is imbedded—connexions which are delicate in structure and loosely applied for the purpose of permitting free extension of its parietes to the varying condition of size which its function as a reservoir of urine demands. Destructive inflammation of these delicate structures is easily produced by the forcible dilatation and the dragging downwards of the neck of the bladder which insufficient incisions render necessary. Inflammation once extending through these structures rapidly invades the peritoneum, which, very probably, is more frequently implicated in this manner than by any other cause.

In connexion with this subject there is a very significant fact, the bearing of which we shall presently examine, viz., that while it is certain that the boundaries of the prostate are almost invariably overstepped by the knife in children, infiltration of urine very rarely occurs in their cases. Happily also, infiltration does not necessarily follow such incisions in the adult; but they render it more likely to occur. That the prostate has often been completely divided with impunity for the removal of large stones is certain; and the risk incurred from that cause is unquestionably serious. Danger is always great in a ratio proportioned to the size of the calculus; but this arises quite as much from the violence inflicted in removing it as from the depth of the incisions employed.

Let it not be imagined from these remarks that any one can deprecate more strongly than myself the making of an incision in the prostate more deeply than the size of the stone demands; but I am very certain that it is safer to extend the incision, when the stone cannot be extracted without exerting violence, than to inflict the injury which such a proceeding necessarily involves. The advocacy of small internal incisions by Scarpa, who laid down as an axiom that an incision of five lines into the prostate, with dilatation, sufficed for the extraction of a stone of more than ordinary size, and by Sir B. Brodie, in his admirable lectures here, as the *sole or chief* means of preventing urinary infiltration, has greatly influenced professional opinion on this subject. And I believe the effect may have gone beyond the intention of its authors; since the force with which this particular source of danger has been insisted on by almost all subsequent writers, has led many to regard it as the only, or at least the main, evil to be feared in the operation; and

thus, perhaps, has indirectly occasioned the oversight of danger in the opposite direction. I wish here to point out that in shunning Scylla we may encounter Charybdis, and that a great obstacle to successful lithotomy lies on either side of our path, and not on one side only; that we must preserve the neck of the bladder equally from too deep an incision on the one hand, and from the mechanical injury necessitated by one which is too limited, on the other. I fortify my position by reference to the significant fact that the most successful operators have been those who advocated sufficient incision as less dangerous than violent extraction. Thus Mr. Martineau, who is well known to have cut eighty-four cases at Norwich with only two deaths, writes, in that brief and simple account of his method which he presented to the Medical and Chirurgical Society in 1821,—"Should the stone be large, or there be any difficulty in the extraction, rather than use much force, while the forceps have a firm hold of the stone, I give the handles to an assistant .....while the part forming the stricture is cut, which is easily done, as the broad part of the blades becomes a director to the knife; and, rather than lacerate, I have often repeated this enlargement of the inner wound two or three times."

At the same time it is always to be remembered that the neck of the bladder is susceptible of dilatation to a very considerable extent, if it only be *gradually exerted*. It yields first to the pressure of the finger as it passes through immediately after the knife; secondly, it dilates further in the act of sliding in the forceps upon the finger; and lastly, it gives away still more when the forceps is withdrawn containing a stone between the blades, especially if it be a large one. This, its susceptibility of becoming dilated, is of the utmost value to the lithotomist. Indeed, if it did not exist, and largely too, none but small stones could be withdrawn through any incision limited to the prostate only. But in order to take advantage of it, the dilatation must be made slowly and gently. If done hastily, harshly, and forcibly, it is not dilatation which has been accomplished, but rupture. And by "rupture" I do not mean the mere enlargement of the wound in the prostate and neck of the bladder, which probably is often legitimately occasioned, but the rupture of the surrounding cellular connexion with the numerous veins and the capillary network which traverse them—results of an extremely dangerous character. In this way inflammation of the cellular tissue, pelvic abscess, or phlebitis may be set up: suppuration is produced in a situation where the pus finds its way to the peritoneum, and not to the surface; and when this state of things exists, a deep incision would have proved a safeguard, rather than the contrary, by affording exit to the confined matter. It is wholly impossible, then, to overrate the importance of slowly and gently dilating the neck of the bladder and the incisions which have been already sufficiently made, and giving abundance of time in the act of introducing the forceps and especially in that of withdrawing the stone. If there be any single proceeding in connexion with the practice of lithotomy, no matter what is the operation performed, which demands more than any other, care, attention, and self-command, I should say it is the manner in which we traverse with instruments the wound in the neck of the bladder.

2. Death after lithotomy may result from the rapidly spreading inflammation produced by urinary infiltration into the cellular interspaces between the pelvic viscera, when they have been opened up by too deep incisions.

This result, although undoubtedly occurring sometimes, does so much less frequently, I believe, than is usually supposed. It is true that at a post-mortem examination, after a large stone has been with difficulty extracted, the cellular connexions of the neck and base of the bladder are found to be broken up; sloughs of the connective tissue appear bathed in fluid, seropurulent and urinous; and marks of peritonitis, especially severe in the pelvis, are observed. But there is good reason to believe that in most cases urinary extravasation is not the primary cause of the inflammation, but that inflammation has been the occasion of the urinary extravasation. Cellulitis, produced by violence, has first destroyed the connexions in the manner described above, and then the urine has rapidly infiltrated the disintegrated

tissue, and has lighted up a virulent peritonitis, or intensified a previously existing one. Such appears to be the true explanation of the phenomena which mark the progress of events during the period, more or less brief, which follows the operation in many fatal cases.

Infiltration by no means necessarily occurs when urine passes over the newly made section of cellular *spaces*, so called; in fact, cellular interspaces between muscles and between viscera do not exist, except when made by the anatomist for the necessary purpose of demonstrating the planes of cellular tissue which unite adjacent organs, and facilitate freedom of movement between them. I very much doubt if urinary infiltration ever occurs when they are otherwise uninjured, in a person of fair vigorous health. To judge from the language held respecting this subject, one would imagine that hollow intervals existed between the organs in question, over which urine had only to be poured in order to be drained mechanically into them. No such thing exists. In the child, where these interspaces are of the loosest and most delicate kind, and where the bladder is active, powerful, and irritable, urine is constantly poured out after this operation over the visceral interspaces which have been freely divided; nevertheless, with what extreme rarity do we meet with urinary infiltration in the child! But once inflame this cellular tissue, destroy its healthy character, or even perhaps let the patient be of unsound health, or one in whom "the flesh never heals well," to use a common phrase, and then we have the condition in which urinary infiltration may take place with rapid and fatal effect.

I am aware that this doctrine is opposed to the generally received notions on this subject. I speak with great respect for existing views; but careful pathological study of the subject by the bedside of the dying patient, and at the subsequent autopsy, has convinced me that the true cause of death in the majority of cases, the cause that it most behoves the operator to guard against, is violence in opening up the internal part of the wound, and laceration of the tissues there, and not the primary passage of urine into the intercellular connexions about the neck of the bladder. Happy is it if it be so, since the first it is in his power to avoid; the second is a danger which would be often inherent in the operation, and unavoidable with a stone much above the average size. That it is not an inherent necessity in the operation seemed to me to be indicated in an early study of this difficult and important subject by the fact that forty or fifty consecutive cases of lithotomy might be cut, and have been cut, without a single casualty. If it were a fact that cellular interspaces could not be cut without the greatest risk of life, how could such a result be possibly accounted for? It was necessary to suspect the existence of another cause; and here the comparison of living phenomena with anatomical appearances led me to conclude that the cause is avoidable, and not inherent. That, to sum up the subject, in the great majority of cases, the cause of death is due to unneccessary violence inflicted on the neck of the bladder and parts adherent, causing destructive inflammation of the connective tissue and of the network of minute bloodvessels which pervades it, and that then, and not until then, does infiltration of urine occur, when it rapidly and frightfully augments the already existing danger. In some cases a small quantity of poisonous fluid, associated with or resulting from decomposed urine, probably enters the circulation by absorption, and produces those depressing constitutional symptoms which always accompany this accident, and which are dangerous in proportion to the diminished capacity of the kidneys to eliminate them from the blood, and of the constitution itself to overcome the shock which it invariably sustains in these circumstances. Lastly, in all or nearly all instances, the irritating fluid soon reaches the peritoneum, and, if the powers of life are not already exhausted, it gives rise to fatal peritonitis.—*Lancet, March* 8, 1862, *p.* 245.

81.—ON LITHOTOMY IN THE FEMALE.

By F. Le Gros Clark, Esq., Surgeon to St. Thomas's Hospital.

Stone in the bladder in the female is of comparatively rare occurrence, and the opportunities you have of witnessing its removal are therefore comparatively few. As I operated on a little girl not long since, I take the present occasion to refer to this subject.

In many instances a stone may be removed from the female bladder without any cutting; that is, when there is such proportion between the size of the urethra and the foreign body as to admit of the passage of the latter through the former without any serious laceration. The dilatability of the female urethra is very great, and I have removed a stone of considerable size without either cutting or laceration. In the present case this proceeding was out of the question, as the patient was a child of five years old and the stone was evidently a large one. This child had never retained her water, and had suffered from symptoms of calculus from her birth.

I operated in the following way:—Having passed a small three-pronged dilator into the urethra, I opened the instrument and put the passage on the stretch. I was thus enabled to pass a straight blunt-pointed bistoury into the bladder, and to divide its neck and the urethra to a sufficient extent to introduce the forceps and remove the stone. The incision I made was downwards and outwards on the left side; in fact, in the same direction as in operating on the male. Indeed, I made the operation resemble, as nearly as may be, that in the other sex. The female urethra is the analogue of the membranous part of that passage in the male, and was thus reached at once without an external incision, which is, of course, requisite in the male. This little patient had not a single bad symptom. Freedom from suffering seemed a new existence to her. The urine continued to flow without power of retention when her mother removed her home. I have since seen her, and have examined the parts as well as the contracted aperture of the vagina would allow. The child is able to retain her water pretty well at night; but in the day time and when up she seemed to have no power of retention. I believe that the urethra is still laid open into the vagina, and that though the aperture is gradually contracting, it is still too large to admit of the urine being retained. If this continue, some effort must be made to afford relief; but I trust it may not be needed. The stone—lithic acid—weighed nearly an ounce, and is 3¼ inches in its smaller circumference and 4½ inches in its larger.—*Medical Times and Gazette, Nov.* 9, 1861, *p.* 471.

82.—ON LITHOTOMY.

By F. Le Gros Clark, Esq., Surgeon to St. Thomas's Hospital.

Few, perhaps, if any, of the great operations of Surgery, have been the subject of more experiment than that of lithotomy. This was justifiable until certain principles were ascertained, and proved by extended observation and competent authority; but I venture to doubt whether the present state of our knowledge, and the general success of the usual lateral operation, warrants further attempts to deviate from the received mode of proceeding: I mean that of making a definite opening through the left side of the prostate, and reaching it by a corresponding opening in the left side of the perineum. It is true that the operation is not uniformly successful; but what capital operation is? and it must be borne in mind that the highest skill cannot secure success, where so important a viscus as the bladder must be laid open. Different surgeons give a preference to various instruments to effect their object; but the matured experience of the most trustworthy bears testimony to the soundness of the principles on which Cheselden's operation is founded.

In the few remarks I propose to make on lithotomy by the lateral section, I will confine myself to the consideration of two methods of reaching the

bladder,—that in which the staff is held throughout by an assistant, and that in which the operator takes the staff into his own hand after opening the urethra. I will then state my reasons for preferring the latter operation, and describe the instruments I use.

The former method of operating offers the advantage of leaving both hands at liberty,—the one to hold the knife, the other to guide and assist in cutting. In this mode of cutting into the bladder, a staff with a large curve, and grooved at the side, is required, and a dexterous assistant is also needed, otherwise I apprehend there must be more risk of injuring the rectum from the prominence of the curve which the staff presents in this direction. The urine, also, is prevented from escaping by the introduction of the left forefinger through the opening in the prostate. I have never practised this operation, but am informed by those who have that these are its special recommendations, and certainly they are great.

In that method of operating in which the operator takes the staff into his own hand, and depresses it as he enters the bladder, he is more independent of assistance in this most important stage of the operation; and the co-operation of the two hands thus employed in effecting this object compensates, I think, for the disadvantage of not being able to guide the point of the knife into the bladder. I do not, however, profess to advocate one operation in preference to the other. I have confidence in that method which I have practised, and therefore I adhere to it; whereas, I know nothing, practically, of the other.

The desiderata in the lateral operation are: 1. To make an external, free, and well-depending opening. 2. To open the urethra as far back as convenient. 3. To make a definite and sufficient opening in the prostate. 4. To take care that all cellular tissue and fascia intervening between the external wound and prostatic opening be freely divided. The object of a free external opening, carried well down by the side of the anus, is to facilitate the ready drainage of the urine. The urethra should be opened as near to the prostate as possible, as nothing is gained by its division further forward, and there is certainly greater risk of hemorrhage if the bulb be laid open; I suspect, however, that this is more often done than is usually supposed. A definite opening through the prostate I consider very important; and, if that opening be sufficient for the introduction of the forefinger, it is sufficient for the removal of a stone of any ordinary size. But I attach no less importance to a free and entire division of every intervening texture between the prostatic and external openings; and this is best effected as the knife is withdrawn, by making it clear its own way to the bottom of the external wound. In this way a free track for the discharge of the urine is secured, without which there must be risk of lodgment and consequent mischief. This is most likely to occur within the first day or two, for afterwards the spaces between the areolar mesh are blocked by adhesive matter; but at an early period, if the urine have any spot to rest in, immediate mischief results; the infiltration goes on, and from this cause and the inflammation which is set up, from contiguity, in the neighbouring areolar tissue, serious consequences speedily ensue. The simple flow of the urine over the divided track does not seem to entail trouble, but its lodgment, I believe, invariably does.

I use a staff of moderate curve, and grooved on the back. The curve does not exceed two and a-half inches, the rest of the instrument being straight. The urethra is not thus distorted, as I apprehend it must be with a staff of large and long curve, or with a straight staff. I have it held nearly at right angles to the body, with the point within the bladder; I thus cut on the straight part of the staff, or just where the curve commences, and have not to depress it very much as I cut into the bladder. I use, for my first incision, and to cut into the urethra, an ordinary scalpel, and then exchange this instrument for a scalpel fixed in a strong and firm handle, and with a broad belly, and a button point which runs freely in the groove of the staff. At one time I cut throughout with the same knife—of course a sharp-pointed one; but I am satisfied there is some risk, in doing so, of wounding the bladder; and I regard the few seconds which are lost in this exchange of knives as nothing in comparison with the risk mentioned, although it may be trifling. With the knife I have mentioned, I am able to

make a definite and sufficient opening through the prostate, without cutting it as I withdraw the knife: and in this way I believe I combine the chief advantage of the gorget with the greater manageability which recommends the knife. After this section the bladder retains sufficient water to allow of the ready seizure of the stone. I need scarcely add, that the forefinger of the left hand should be passed through the prostatic opening and the stone felt, before the staff is withdrawn: the finger then serves to guide the forceps into the bladder.

After the removal of the stone, it is the custom with some surgeons to introduce a tube into the bladder to secure a passage for the urine. I do not see any objection to this plan, nor do I see any advantage to be derived from it in ordinary cases; the urine will still drain by the side of the tube. But it is very useful where there is hemorrhage, as the wound may be safely plugged around a tube; whereas it would be extremely unsafe to plug the the wound without such arrangement. The tube may also be advantageously used where the prostate has been contused by the removal of a large or rough stone, and its probable swelling, and consequent blocking of the opening, may be anticipated.

The general treatment of patients who are the subject of lithotomy is no unimportant element of success. It is to be remembered that in opening the bladder in a comparatively healthy person, we inflict a serious organic injury, and that a certain amount of shock must be expected as the consequence, followed by reaction. The skin will become heated, the pulse quickened, and the tongue furred. This condition is to be looked for, and not regarded with apprehension. The previous evacuation of the bowels allows of their being left at rest for two or three days afterwards. The diet should be simple and unstimulating for a time; in some cases, however, stimulus is required early, especially where the patient has been habituated to it. Gin is a favourite remedy among the poor, when anything is amiss with the urinary organs, and even young children are not unfrequently dosed to a considerable extent with it. I have often noticed some little tenderness in the iliac fossa, without any further symptoms to create alarm. I have occasionally had hemorrhage to control: this has been effected either by pressure on the pudic artery, or by plugging the wound after the introduction of a tube. In one case of secondary bleeding, induced by continued vomiting after inhaling chloroform, I nearly lost a little child. In one instance a communication was established between the rectum and wound, in consequence of laceration by the large angular fragments of a hard stone, which broke in the forceps: this wound entirely closed. And in one case, where the stone was very large, and the patient deformed, abscess of the prostate burst into the rectum about a week after the operation. The communication between the rectum and bladder in this case was permanent, and too high up to attempt any operative interference. The patient, however, informed me that he was not seriously inconvenienced by this false passage. Beyond the above, and the loss of a solitary case—an infant of twenty months with a disorganised kidney—I have no special experience to record: all of my other cases have recovered. The largest stone which I have removed—indeed, here are two of nearly the same size—weighs more than a quarter of a pound, and the smallest, which I have also brought to show you, weighs only five grains. Yet these small stones often occasion great suffering; and I may here remark that, as a rule, smooth stones, such as the lithic acid, are the source of greater pain than rough ones, such as the mulberry. I suppose in one diathesis the bladder must be more irritable than in the other, or it becomes more protected by a copious secretion of viscid mucus. I may also observe that the patients who have suffered most pain, *cæteris paribus*, make the best recovery. —*Med. Times and Gazette, Nov.* 9, 1861, *p.* 472.

83.—ON VARICOCELE.

By Henry Lee, Esq., Surgeon to the Lock Hospital.

[The chief symptoms produced by a dilated condition of the spermatic veins are pains and a feeling of weight and dragging in the affected side, and defective nutrition of the testicle, which becomes softer than natural, and sometimes it becomes permanently atrophied.]

In whatever part of the body pain and imperfect nutrition occur, in consequence of a varicose condition of the veins, they may be remedied in the same way; namely, by obliterating those vessels which are larger than natural, and by thus compelling the blood to pass by those channels only which retain their natural dimensions.

The plan adopted for this purpose is simple and effectual. A needle is introduced beneath the vein or veins to be obliterated, and a 8-ligature is passed over its extremities. The vein or cluster of veins is thus included between the needle which is below it, and the ligature which is over the skin. The ligature is then tied so as to produce a slight degree of pressure on the vein, and by this means its cavity is temporarily closed by acupressure. Another needle is then passed under the vein at about an inch distance, and the vessel is there closed in like manner. The vein or cluster of veins thus included between the two needles is entirely separated from the rest of the circulating system, and may then be divided or otherwise obliterated without risk. Subcutaneous section of the veins is generally all that is necessary, and is preferable to any other mode of operating, as it effectually obliterates the vessels without leaving any open wound.

At the expiration of about four days, when union is established in the divided parts, the needles are removed, having served their temporary purpose. In three or four more days, the patient may usually go about his ordinary occupation.

The operation now described was first performed by me for the enlarged veins of the spermatic cord some years ago, upon a patient in the Lock Hospital; and the success in that case was so complete that I have performed the same operation upon a very considerable number of patients since.

After an operation such as is now described, the divided veins become puckered up in a hard knot, which effectually prevents the veins from again becoming enlarged at this part. The divided veins are permanently obliterated, and the circulation is carried on by those that have not been interfered with.

The pain which arises from the distension of the veins ceases as soon as the union is complete; and in several cases in which atrophy of the testicle had taken place, marked increase in the size of the gland has followed the operation.

There is one complication sometimes met with which the simple subcutaneous division of the veins does not remedy: this is a pendulous condition of the testicle. The testis sometimes hangs so low that it interferes with some of the motions of the body, and is liable to be injured by any sudden exertion. In such instances, there may or may not be a varicose condition of the veins, but the skin and subjacent tissues are always redundant. For this troublesome complaint, the late Mr. Briggs, in the old Lock Hospital, occasionally removed a considerable portion of the skin of the scrotum, and the cicatrix left maintained the testicle in something like its natural position. This operation succeeded well enough when the case was not attended with enlargement of the veins; but when it was, the removal of the skin could have little or no effect upon the varicocele. At the time to which I refer, no one thought of cutting deeper than the skin, as no means were then in use which would effectually control hemorrhage from the spermatic veins in case they were wounded. Mr. Briggs's operation, then, was well adapted for a case of pendulous testicle where the veins were not enlarged, but not for a case accompanied by varicocele. A case presented itself some time ago, in which the testis hung so low, that when the patient lay upon his back, it would hang over on the outer side of the left thigh. There was in this instance, a

well-marked varicocele. Knowing that I had the means by acupressure (which I have now employed in this way for upwards of eight years) of controlling the dilated veins, it appeared practicable to combine Mr. Briggs's operation with that for obliteration of the veins. Accordingly, the needles were introduced as far apart as possible, and after the veins were secured by the 8-ligature passed over the ends of the needles, the intermediate portion of skin, together with subjacent tissues, containing the enlarged veins, was removed, leaving a very considerable gap. No hemorrhage occurred. The n edles were then drawn towards each other, and by this means the edges of the wound were brought together and maintained in position. The wound healed in about three weeks.

The operation for varicocele by simple subcutaneous incision has now been frequently performed; and the only inconvenience that has in any instance occurred, has been some hemorrhage into the areolar tissue in the neighbourhood of the divided veins. This depends upon the fact that a vessel is sometimes divided which has not been secured by acupressure. In order to avoid this occurrence, care should be taken to compress the same veins above and below the incision, and not to divide any that are not thus compressed. If one of the spermatic arteries be divided which has not been included between the upper needle and its ligature, then some blood will escape, and a swelling will will be formed above the line of incision; if, on the other hand, a vein be divided which is not included in the lower ligature, then the effusion of blood and the swelling will take place in the lower part of the scrotum. The remedy in either case is the same. As soon as the swelling begins to appear, and there is good reason to believe that it depends upon effusion of blood, a fresh needle is to be introduced beneath the bleeding vessel, and a 8-ligature passed round its ends. A very moderate degree of compression is sufficient. If it is an artery that is bleeding, the fresh needle is introduced above the subcutaneous incision; if it is a vein, the needle must be introduced below this point. But in either case, care must be taken that the bleeding vessel is included. If, in the first instance, the needles and the incision are carried on to the same depth, and include only the same vessels, then no hemorrhage will occur.

In all the above mentioned proceedings, care must be taken to avoid the vas deferens. This may always be recognised by its peculiar hardness and irregular surface, and can be easily separated from the veins before the needle or the knife is passed beneath them.—*British Medical Journal, Dec.* 7, 1861, *p.* 602.

84.—ON THE RADICAL CURE OF VARICOCELE BY SUBCUTANEOUS LIGATURE OF THE SPERMATIC VEINS.

By JOLLIFFE TUFNELL, Esq., Surgeon to the City of Dublin Hospital.

Velpeau's plan was to "introduce two pins, one before and the other behind the veins, which are then compressed by carrying a ligature around the extremities of the pins, the varicocele being included between them." After this, was Velpeau's simpler plan of passing a pin behind the veins, and working a twisted figure-of-8 suture over them; then we have the twisted sutures of Sir Benjamin Brodie, modified by the subcutaneous incision of the veins between them, as practised by Dr. Lee; the passing of threads through the veins as setons, and their obliteration by the introduction of a heated awl, or wire, as suggested by Mr. Fergusson; and the double ligature of Mons. Ricord. All of these have severally been employed; and the very variety must be sufficient to prove how unsatisfactory in the majority of cases had the previous modes of treatment proved, and how each surgeon, in consequence, had endeavoured to obviate pre-existing objections.

It was with this view that I adopted the annexed modification, the success of which will, I trust, justify its general use. The plan I advocate is simply an improvement upon Mons. Ricord's double ligature, by adding to the double loops for the enclosure of the veins a pair of *retracting guides*, by which the

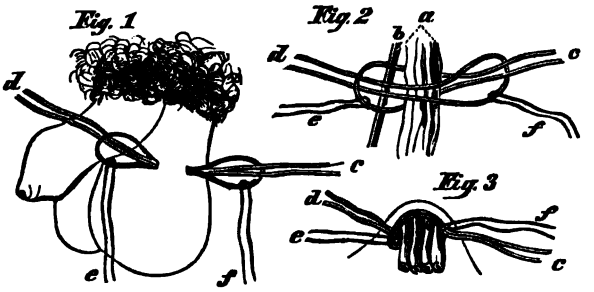
surgeon is enabled to withdraw the compressing medium at any moment, thus holding as it were a check-string upon inflammation in his hand, producing an adhesive phlebitis, and stopping short of the suppurative form.

With this latter view, I employ also fine iron-wire as a substitute for the ligature of silk; metal being less irritating to the parts engaged.

Before resorting to the plan which I am now about to advance, I had found that in each instance in which I had used Ricord's loops, suppuration in the scrotum occurred. Since employing the retracting guide, no such annoyance has taken place.

The method, then, that I advocate is the following:—In any given case of varicocele that comes before me, I interrogate the individual, and ascertain whether any remedial means have as yet been adopted; and, if not, I recommend palliative treatment, viz., attention to the bowels, so as to prevent, by regularity of action, any lodgment in the descending colon: the use of cold water sponging night and morning, to constringe the vessels and dartos of the scrotum; and a bag truss. I do not advocate the use of pressure to the dilated veins at the external abdominal ring by means of the spring truss; or the puckering in of the loose skin of the scrotum, and the confining it in a steel ring, as sometimes adopted—my experience being to the effect, that these remedies only irritate the patient, and that they are both inferior in action to a well-fitting common bag truss. If these simple measures fail to afford such an amount of relief as will satisfy the patient, and he persists in having more effectual means adopted, then I employ the following proceeding, after satisfying myself of the non-existence of organic disease of the kidney:—

I prescribe an aperient, to evacuate the contents of the intestines, of the compound powder of jalap combined with calomel. I confine the individual to bed for a day, and then deligate the veins in the manner recommended by Ricord, and illustrated in the accompanying sketch, taken from Bernard and Huette's work, modified as will be described.



An assistant separates and holds aside the vas deferens. The bundle of veins is then isolated, and taken up in a fold of the skin, the first loop of wire being carried behind the veins (Fig. 1, *d*). A second loop of wire is then passed in the opposite direction, in front of the veins, through the same orifices as the first (Fig. 1, *c*). The bundle of veins (Fig. 2, *a*) is thus included between the two loops, the free extremity of each wire being passed through the loop of the other, as seen in Fig. 2, *c*, *d*. The wires are then drawn in opposite directions, until about as much of each loop remains projecting from the scrotum as is seen in Fig. 1—and the amount of the projection of the loop made equal on either side. Two portions of iron-wire are now taken, each four inches in length, and doubled in the middle as closely as possible. One of these is passed through either loop, and drawn to its centre. The ends are then twisted. These wires form the *retracting guides*, and give the surgeon the most complete control over the vessels of the cord, enabling him at any moment that he wishes to take the ligature from off the veins, and remove all source of irritation from within the scrotum. The main wires (Fig. 3, *d*, *c*) are now to be drawn *forcibly* until the vessels of the

cord are completely strangulated. This done, the ends of the wire are brought up and attached to a ratchet, and thereby kept tensely strained; or else twisted around the extremities of a piece of strong watch-spring, bent back into an arc, the effort of which to restore itself keeps up continuous tension upon the cord.

With reference to the length of time during which it may be necessary to maintain this constriction, in order to produce sufficient adhesive phlebitis, and yet avoid the suppurative form, this must entirely depend upon the constitution of the individual. The guides for the surgeon will be the density of the swelling of the veins below the ligature, between the epididymis and the wires; the feeling of firmness to the touch; and a slight blush upon the integuments of the scrotum. These symptoms, conjoined, will denote that such a degree of inflammation has been excited as will produce obliteration of the venous trunks. The main wires are now to be cut across, and the ratchet or steel spring detached. This done, the figure-of-8 loop encircling the veins is to be opened, by withdrawing the wires through the medium of the retracting guides. All source of irritation being now removed, further inflammation is prevented. No pus is formed in the veins, which can be carried up into the circulation; or external to the veins, in the scrotum, which can give rise to the formation of abscesses in its cellular tissue. A little lead-wash to the scrotum, and rest upon the back in bed for a few days, with low diet and attention to the bowels, completes the cure. The patient, before rising, should be fitted with a suspensory bandage to support the parts. He may then resume his ordinary business or occupation.—*Dublin Quarterly Journal, Nov.* 1861, *p.* 333.

85.—THE WIRE SETON IN HYDROCELE.

By Dr. James D. Gillespie, Surgeon to the Royal Infirmary, Edinburgh.

[Dr. Gillespie was at first led to make trial of the wire seton on theoretical grounds, believing that adhesive inflammation only was likely to be excited by the introduction of metallic wires. The sequel proved that violent and indeed dangerous suppurative action might arise from the employment of such a seton. He says:]

Professor Simpson instituted a number of carefully conducted experiments, to show the impunity with which metallic substances might be incarcerated in living tissues; and, reasoning from analogy, concluded that wires passed through a hydrocele were likely to cause merely adhesive inflammation, and thus effect a speedy and simple cure. The result of extended investigation into this plan of treatment has proved that hydrocele may be cured, and both speedily and easily cured, by such a method; but it has no less satisfactorily established the fact, that a certain, and no small per centage, of cases, notwithstanding every precaution, will suppurate.

[After giving a narrative of ten cases treated by this method, Dr. Gillespie makes the following remarks:]

The wire seton, when all due care is taken, may speedily and safely effect a radical cure in hydrocele; but it is also as unquestionable a fact, that alarming suppuration may ensue, though the utmost possible attention has been bestowed on the case. Several of the cases I have narrated establish the fact that suppurative action may come on days after all fear of such a contingency had passed away. Indeed, in most of the cases detailed the wires were not retained so long as was deemed expedient in Dr. Young's patients. Here I may allude to one reason given by Dr. Young for failure in my first case, when I was obliged to have a second time recourse to the seton. He says, "This case might have succeeded after the first operation, had the wire seton been retained longer, and five wires doubled used in place of two." Unfortunately for Dr. Young's suggestion, the printer had mistaken two for ten, and *five wires doubled* were exactly the constituents of the seton! How retaining these longer would have saved the patient from

the very dangerous suppuration that ensued, I leave it for himself to explain!

It is necessary, however, before condemning the use of the wire seton in hydrocele, to show that a better and safer method exists, which brings me to a comparison between this mode of procedure and the use of injections.

Injection of almost any stimulating fluid may cure a hydrocele; and if time permitted, I could point out numberless substances that have been employed, and have had their day of notoriety and praise, but for my present purpose it is not necessary to allude to any other fluid than the tincture of iodine, which has deservedly usurped the greatest share of favour in the present day.

It is difficult to collect facts with regard to the efficacy of this mode of treatment, as at present employed, for it is not now apparently considered of sufficient importance to write special notices about. What I believe to be much the best method of employing it is, to inject from half a drachm to two drachms of the tincture slightly diluted, and allow it to remain in the sac. This plan was first proposed by Dr. (now Sir J. Ranald) Martin, of the East India Company's service, has been advocated by Professors Liston, Syme, and Ferguson, and other eminent surgeons, and is the method which has been generally adopted in the Royal Infirmary here. It is now about twenty years since I first saw this mode of injection used in hydrocele, and I cannot recall any one case of suppuration resulting from the operation, though I have a vague impression that such an unusual and unfortunate complication once did occur. That sloughing of the scrotum has sometimes resulted I admit; but I am satisfied, that such an accident was occasioned by the mode, which was formerly practised, of distending the sac with diluted tincture, and then drawing it off—occasionally some of the injection escaping into the cellular tissue, and causing the disastrous consequences to which I have alluded.

As regards the efficacy of injections, it must be said, that sometimes, but very unfrequently, they fail; but the patient is not in a worse state than he was before, which cannot be said with justice of the seton, for when too much inflammation arises, most alarming symptoms may supervene.

I trust I may not be mistaken as regards my hostility to the wire seton. I am fully impressed with the fact, that metallic wires are not likely to induce suppuration; the principle of the operation is correct, so far as it goes; but certain counteracting circumstances have been overlooked, which I believe have mainly tended to excite suppurative action, and which no skill on the part of any operator can prevent. When a seton of any kind is passed through a shut sac, of such an irritable nature as the tunica vaginalis, and allowed to remain for hours, communication with atmospheric influences must result, and I believe that this is the true reason, quite independent of what the seton is constituted, which may give rise in some cases to suppuration.

The puncture made by the trocar and canula soon heals, shuts up the cavity, and thereby explains, I should say, the comparative impunity from suppuration, when injections are employed. If metallic substances are to be used for the cure of hydrocele, it therefore appears to me that a safer method would be, to draw off the fluid by the ordinary trocar and canula, and then insert through the puncture a probe, or some small metallic body. This in time might excite sufficient adhesive inflammation, and at all events be only half as likely as the seton to cause suppuration. In the time of Pott, a somewhat similar idea was proposed, but laid aside as inconvenient and sometimes dangerous. The hydrocele was tapped in the usual way, but the canula was allowed to remain in the sac till sufficient inflammation was supposed to have ensued. But the canula being a hollow body, enabled the interior of the cavity to communicate directly with the external air, a very important disadvantage, I should say.

I merely throw out this suggestion for those who are dissatisfied with the treatment by iodine injection; for I shall conclude by stating, that I am my-

self perfectly well contented to abide by that long valued and, in my experience, sufficiently successful and safe operation.—*Edinburgh Medical Journal, Jan.* 1862, *p.* 630.

86.—*How to Relieve Pain in Diseased Bladder.*—The presence of urine, and more especially of uric or phosphatic calculi or concretions at the *basfond* of a diseased bladder, sometimes produces violent pains in the bladder, and renders all movement painful. In such cases if the patient be placed on an inclined plane, which, by raising the lower part of the pelvis, throws the contents of the bladder towards the upper and posterior part of the cavity (which is much less sensitive), relief is almost immediately produced, even though other means have been tried in vain.—*Rev. de Thér.; and British Med. Journal, Feb.* 1, 1862, *p.* 119.

SYPHILITIC AFFECTIONS.

87.—SYPHILIS CONVEYED BY THE VACCINE LYMPH TO FORTY-SIX CHILDREN.

[The unfortunate occurrence to be described occurred at Rivolta, in Piedmont. The account is taken from an Italian Medical journal, and the facts are authenticated in a letter to the editor of the Lancet, by Dr. Pacchiotti.]

Towards the latter end of May last, M. Cagiola, a surgeon, vaccinated Giovanni Chiabrera, aged eleven months, and in good health, with lymph obtained in a tube sent from Acqui. The operation was performed in the ordinary manner, and with, as M. Cagiola affirms, a very clean lancet. On the tenth day after this, forty-six children were vaccinated with the lymph contained in the vesicle of the child Chiabrera; and ten days after these latter operations, seventeen other children were vaccinated from the lymph of one of the forty-six infants just mentioned.

Hence we have sixty-three vaccinated children, forty-six of whom were more or less affected with syphilis within two months after the first operation. In the first series of forty-six vaccinations there were thirty-eight cases of syphilis, besides little Chiabrera, the child vaccinated with the lymph contained in the tube; and in the second series, comprising seventeen infants, seven were affected. The child Chiabrera was in a state of marasmus on the 7th of October, and the infant from whom the second series of seventeen had been vaccinated, died a month after the operation.

These facts having come to the knowledge of the Medical Congress at Acqui, from statements made by Dr. Ponza, it was agreed that a committee, elected from amongst the members of the Congress, should proceed to Rivolta to inquire into these melancholy occurrences. From the able report of Dr. Pacchiotti we extract the following particulars.

The investigations of the committee were considerably aided by the unwearying exertions of Dr. de Katt, practising in the village. It has been found that of the forty-six children affected with syphilis, the cases of only twenty-three could be accurately noted, as the parents of the children neglected to call in medical aid at the proper time. These twenty-three cases were, however, sufficient to enable the committee to come to a clear diagnosis. In the whole forty-six cases, the symptoms of syphilis appeared, on an average, on the twentieth day after vaccination—namely, varying from ten days to two months. Sometimes the vaccine vesicle, just on the point of cicatrizing, inflamed, and became surrounded with a red, livid, and copper-coloured areola, and then spread and suppurated anew. At other times when the cicatrix was complete, an ulcer would form upon it, the crust of which would fall off and fresh ones be produced. With some children the vesicles looked bad from the first, and were accompanied by a general eruption, which the

country people considered as small-pox, and the characters of which the medical men of the neighbourhood were not always able to ascertain. On the 7th ult., it was discovered that seven children had died without treatment, and before attention had been directed to this unfortunately fast-spreading contamination; three were in danger, and fourteen recovering, after having been subjected to a specific treatment. Thirty-eight at that period were under treatment, which consisted of frictions with mercurial ointment in the groins, axillæ, and on the limbs, with small doses of iodide of potassium in sarsaparilla syrup.

The principal symptoms noted by the committee were: mucous tubercles on the verge of the anus and genital organs; sores on the lips and fauces: swelling of the lymphatic glands in various regions; syphilitic eruptions of different kinds; loss of hair; secondary ulcerations of the prepuce; deep tubercles of the cellular tissue; gummy tumours, &c. Two children out of the twenty-three were in a wasting condition, and suffering from syphilitic cachexia; while some of the mothers had mucous tubercles on the nipples. In fact, the twenty-three cases are carefully related in the report, all the children having been seen by the members of the committee.

As to how the disease thus came to spread amongst these infants, the committee refrain from coming to a hasty conclusion, and ask for time to solve the mystery; the more so as these facts tend to no less than a complete upsetting of opinions hitherto held as very trustworthy. Thus the belief of two diseases not having the power of developing at the same time upon the same individual falls to the ground, as well as the non-contagious nature of the secondary symptoms of syphilis.

Dr. Pacchiotti, the author of the report, indulges in commentaries on this sad case, and throws out, with extreme humility, various explanations, though trusting completely to none. He invites discussion and reflection on the phenomena which have been observed. Nor does he fail to record that such transmission has been before noticed. Dr. Parola has mentioned in his work "On Doctrines connected with Vaccination," a case reported by Tassani, of Milan, in which a boy, whose father had at the time secondary sores on the scrotum, was vaccinated from a healthy child. From the vesicle of this boy fifty-six children were vaccinated; out of whom, thirty-five were, in a few months, syphilitic, and had diseased their mothers. On the other hand, it should be noted, that lymph from eight of these thirty-five syphilitic children was used to vaccinate a second series of thirty-four, and *none* of the latter showed any syphilitic symptoms. Another case (which was brought before courts of justice) runs thus:—In 1846, many revaccinations took place in the town of K——, where a surgeon revaccinated about ten families on account of an epidemic of small-pox; and the punctures, in three or four weeks, degenerated into syphilitic ulcers, followed soon afterwards by secondary eruptions. Hübner, in 1852, vaccinated thirteen children; of whom the greater part became syphilitic, though the rest escaped. Experiments have been undertaken by Pitton, Boucher, Ceccaldi, and Lecoq, which prove the transmission of syphilis through vaccination; whereas other experiments made by Schreier, Montain, Bidart, and Taupin show, on the other hand, that vaccine lymph obtained from a child, evidently labouring under hereditary syphilis, produced no evil effects upon those vaccinated with it. The reporter further alludes to an important thesis of M. Viennois, "On the Transmission of Syphilis by Vaccination;" and to the chapter on the same subject in the book of M. Rollet, of Lyons, entitled "Clinical and Experimental Researches on Syphilis."

From the facts related above, Dr. Pacchiotti deduces the following rules:—

1. Examine carefully the child from whom the lymph is taken.
2. Try to learn the state of the parents' health.
3. Choose, in obtaining the lymph, such children as have passed the fourth or fifth month, as hereditary syphilis, in general, appears before that age.
4. Do not use the lymph after the eighth day of the existence of the vesicle, as the lymph on the ninth and tenth days becomes dull by mixture with pus, which latter may be of an infectious nature.

5. In taking the lymph with the lancet, avoid hemorrhage, as there is less danger with pure and transparent lymph.

6. Do not vaccinate too many children from the same supply.—*Lancet, Nov.* 16, 1861, *p.* 485.

88.—CASE OF SYPHILITIC DISEASE APPEARING AFTER VACCINATION FROM A SYPHILITIC CHILD.

By Dr. N. J. Haydon, Bovey Tracey, Cornwall.

In the summer of 1843 I was called, as the Medical Officer having charge of the sick poor of the parish and borough of Bodmin, Cornwall, to attend two young children of different families, and living about a quarter of a mile distant from each other. The children were each of them from 9 to 10 months old. The history of their illness being precisely similar, one description will apply to both. On the first introduction of the compulsory vaccination system, the Guardians of the Bodmin Union entered into a contract with one medical man to perform the vaccinations for the whole Union. This gentleman, in the discharge of his duty of Public Vaccinator, attended at the appointed room in Bodmin, and on that particular day vaccinated these two children, taking lymph from the arm of a child he had vaccinated the preceding week; he appeared (from the most careful personal investigation which I made of this matter at the time) to have vaccinated no other than those two children on the day in question, and to have taken lymph from no other child but the particular one alluded to. Between the second and the third week after the vaccination had been performed, I first saw the children. They were literally covered with large phlysacious pustules, the irritation was most intense, and, between rubbing and scratching, the head and nates were raw and ulcerated. No treatment had any avail, and both these poor children died a few days after I first saw them. Being at once impressed that the disease of these children was syphilitic, I made the most careful investigation I could into the whole matter. In both families there were other older children perfectly healthy. The parents in both cases were labourers, of most healthy appearance, and of good character—were then most certainly, and I have no cause to doubt ever had been, free from syphilitic taint. The respective mothers of both children carried their infants themselves to be vaccinated; they saw the operation performed, and they saw the child from whom the lymph was taken; they told me the name of the child, and where it lived. As medical officer of the borough of Bodmin, this child and its mother were both known to me. The mother had been, and in fact then was, on the town, and I had attended her for syphilis. At that very time she was diseased. I examined her child; it had, as far as I could see, no primary syphilitic sores, but it had numerous syphilitic eruptions about its body, pustules about its nates and trunk, and copper-coloured leprous spots. The child was between two and three years old, and under treatment it recovered. The public vaccinator lived at a distance from Bodmin, and could not have known the character of the parties from whom he took the lymph.

Since the above case occurred just nineteen years have passed, and in that period I have vaccinated at least 4000 persons, but I have never witnessed an analogous case. It has occurred to me three times at the least to have to vaccinate children whose parents I knew suffered from syphilis, but no peculiar difficulty followed, and the children, *quoad* the vaccination, perfectly recovered. I would never, however, either myself attempt, or recommend any other person to attempt, the converse, and vaccinate a healthy from a diseased child. Whether disease is capable of transmission by the lymph alone, or whether a portion of blood from the diseased subject must also be introduced so as to produce disease, I cannot say; but it is most undeniably true, that in all quarters, both amongst the rich and amongst the poor, there is a strong belief that any disease which may exist in a given child can be communicated to another given child by vaccinating the one from lymph taken from the other.

Very soon after the occurrence of the cases above detailed, and, if I recollect rightly, in consequence of them, the vaccination system in the Bodmin Union was altered, and each medical officer of a district was appointed the public vaccinator for his own medical district, so as to insure, as far as possible, a correct knowledge, on the part of the vaccinator, of each subject before him.—*Med. Times and Gazette, March* 29, 1862, *p.* 316.

89.—ON THE CALOMEL VAPOUR BATH.

By Henry Lee, Esq., Surgeon to the Lock Hospital.

[For many years back the author has discontinued the use of mercury internally, in the treatment of syphilis at the Lock Hospital. Sir Benjamin Brodie says: "You may patch up the disease by giving the remedy internally, but it will return over and over again."]

It will, indeed, seldom happen that in private practice the surgeon can induce his patient to continue the internal use of mercury sufficiently long to cure real syphilis. The patient either "catches cold," or has an attack of diarrhœa, or finds that he is losing flesh, or feels languid and uncomfortable, and incapable of much bodily or mental exertion; and then he discontinues his medicine, and, feeling subsequently better, is convinced that he acted properly. But, as observed by Sir Benjamin Brodie, if, in a case of well-marked syphilis, a short course of mercury only be given, "the disease is sure to return; you have then to repeat it; and again the disease reappears. Thus, you have repeated courses; and not only is the system weakened by the mercury, but the disease, when it does return, assumes a more formidable character than before." (*Lectures on Pathology and Surgery*, pp. 248-9.)

To remedy the evils attending the prolonged internal use of mercury, the plan of introducing the medicine into the system through the skin, by means of mercurial frictions, was introduced; and although this plan, like other modes of administering mercury, was carried to a great excess, and much abused, yet, as practised by the late Mr. Pearson, when, by mature experience, he had learned the proper use of the remedy, it proved by far more successful than any other plan previously employed.

By introducing mercury into a patient's constitution in this way, by inunction, its deleterious action upon internal organs is avoided. The amount absorbed into the blood produces its influence equally throughout the system, and is not conveyed direct to the liver, as when the medicine is administered internally.

But, great as the advantages of the inunction of mercury are as compared with its internal administration, it, nevertheless, is attended with certain inconveniences which prevent its very general use. The inunction of mercurial ointment, so as to ensure the proper effect of the remedy, requires considerable labour and perseverance on the part of the patients, and it is with difficulty that they can be induced to continue its use for any length of time. It sometimes produces a pustular eruption of the skin, which proves troublesome. It is a dirty remedy, and patients of the upper class will avail themselves of any excuse to avoid its use.

Fumigation of the surface of the body, by means of certain mercurial preparations, possesses the advantages of inunction without some of the objections to which this is liable. But, like other modes of using mercury, it was tried in a variety of ways before a safe and efficient mode of administration was adopted.

In the years 1786-7, Mr. Pearson had a fumigating machine constructed according to the directions given by M. Lalonette. This apparatus, although it was thought to be new at the time, differed in no material respect from that described by Nicholas de Blegny, in the year 1683.

Mr. Pearson made a considerable number of experiments with this fumigating machine, and found that the gums became turgid and tender very quickly, and that the local appearances were sooner removed than by the other modes

of introducing mercury into the sytem. But, to counterbalance these advantages, it was found that the mode of treatment adopted induced debility, and that ptyalism was often excited rapidly, and at an early period Mr. Pearson found that he was, consequently, obliged often to discontinue his course of treatment.

Sir Benjamin Brodie's experience coincided with that of Mr. Pearson. He found that it was difficult to regulate the mercurial action; and he observes that by using mercurial fumigation "you may affect the system too much or too little; and you may be taken unawares by the patient's gums becoming all at once excessively sore." (*Lectures on Pathology and Surgery*, p. 246.)

From observations and comparative experiments which I made at the Lock Hospital during the years 1855-6, I feel satisfied that the irregular results noticed by Mr. Pearson and by Sir Benjamin Brodie, depended upon the difference in the chemical composition of the powder used for the purposes of fumigation, both before and after it was raised into a state of vapour. The gray oxide of mercury (the preparation generally used) varies much in colour as obtained at different shops. Some specimens will not volatilise at all at the temperature produced by an ordinary spirit-lamp under a metallic plate. Other specimens of a lighter colour volatilise quickly enough. When the darker specimens are sublimed, they are decomposed in a greater or less degree. A deutoxide of mercury is formed by the addition of an equivalent of oxygen from the air; and if the temperature be much increased, then the oxygen is driven off altogether, and metallic mercury is sublimed. Under these circumstances, with a mercurial preparation of uncertain composition, and undergoing different changes according to the degree of heat applied, there is no wonder that very different effects should have been produced in different cases. With some samples of the gray oxide it is necessary to use a considerable quantity of the powder in order to ensure any effect; with other samples, the same quantity produces much more action than is desirable. The gray color of the powder depends upon the admixture of a certain proportion of calomel with the protoxide, and the temperature at which any particular specimen will volatilise will depend upon the relative proportions of the two. The bisulphuret of mercury, again, which has been extensively used for the purposes of fumigation, gives off, when exposed to heat, a vapour, probably the sulphurous acid gas, which has sometimes caused very considerable irritation of the lungs; and all forms of mercurial fumigation have, in consequence, been condemned. Now, all the inconveniences above-mentioned may be avoided with certainty, by using a mercurial preparation which is always of the same chemical composition, which does not irritate the lungs, and which is not liable to be altered by an increase of temperature. Such a preparation is calomel. We have here a definite chemical compound. It is altered in composition, neither by heat, nor by moisture, and may be relied upon when used for the purposes of general fumigation, for producing its action, as certainly as any medicine administered internally. A very small quantity (as compared with the other mercurial preparations) will ensure the required effect, and this may be regulated and controlled with great precision. —*British Med. Journal*, *Jan.* 18, 1862, *p.* 53.

DISEASES OF THE SKIN.

90.—ON RINGWORM AND VEGETABLE PARASITES.

By Dr. Thomas Hillier, Assistant Physician to the Hospital for Sick Children.

Mr. Hogg maintains that parasitic vegetation is not the cause, but the result of disease, on the following grounds:—"1. The general law that vegetable parasites only attack bodies in a state of lowered vitality." "2. The growth of fungi is not necessarily pathognomic of a special disease, because they have been observed in all kinds of chronic skin-diseases." Mr. Hogg has discovered a fungus in psoriasis, lepra, eczema, spilus, ichthy-

osis, and lichen. "3. Competent observers have not been able to find them in the diseases which they are believed to engender." "4. As to the results of inoculation, the experiments are two few and irregular to afford any basis for argument *pro* or *con*. (*Trans. Microsc. Journal*, 1859.)

In reply to these statements, it may be said that, in tinea tonsurans and tinea favosa, at any rate, all competent observers find a fungus, with the exception of Mr. Erasmus Wilson, who sees the phenomena, however, but interprets them differently. In the chronic diseases to which Mr. Hogg refers, if a fungus sometimes exist, its presence must be regarded as accidental, and its extent not at all proportionate to the amount of disease, being entirely absent even in aggravated cases of these affections, I have searched in vain for a fungus in several cases of psoriasis and lichen. But in those diseases in which the fungus is never absent, and where the changes produced by the microscope are those most obvious both to the naked eye and to the microscope, we may certainly give the parasite a more prominent place in the pathology of the disease; and it is only reasonable to regard it as the determining cause of the disease, whilst we admit constitutional and local states as predisposing causes.

Tinea tonsurans and tinea favosa cannot exist without their respective fungi. Pityriasis versicolor is always accompanied by a fungus; but pityriasis rubra and pityriasis capitis exist usually, if not always, without a fungus. Herpes circinatus is commonly accompanied by a fungus, and probably some cases are due to the fungus; whilst other cases of this affection are found without the fungus, so that we cannot say positively that it is always a parasitic disease. Sycosis is probably always parasitic in its origin. In reference to alopecia areata or tinea decalvans, some observers, including MM. Gruby, Bazin, Hogg, and Lowe, discover a fungus which has been called *microsporon Audouini*. Dr. Jenner and Mr. Hutchinson fail to detect it, and I have been equally unsuccessful. That a fungus *sometimes* attacks the hairs in these cases, must be admitted on the authority of such observers as those mentioned; but we must also maintain that it is frequently absent; so that I would class this disease with the non-parasitic diseases, in which a fungus is occasionally found, but does not determine the existence of the disease.

The evidence derived from treatment is very strong in favour of the parasitic theory, although it must be admitted that the argument applies partially in reference to some diseases where no parasite has been detected; namely porrigo larvalis, or the contagious impetigo of the scalp. Alopecia areata, too, will generally yield to solution of sulphurous acid. This may, however, be due to its irritant properties, and not to its power of destroying vegetable life; as blistering is more efficacious than the sulphurous lotion.

Parasiticides will, I believe, in all cases cure tinea favosa, tinea tonsurans, sycosis, and pityriasis versicolor. The length of time required to cure tinea tonsurans is owing to the impossibility of getting parasiticides to the bottom of the hair-follicles in which the fungus has embedded itself. At the *very outset*, it may be cured rapidly. Newly attacked parts, before the hairs were much attacked, in many of my cases, were cured in a few days.

Dr. Lowe (*Botan. Trans., Edin.*, 1858) endeavours to prove an identity between the fungus in tinea favosa and the *aspergillus glaucus*. He is inclined to concur with Dr. Fox as to the common origin of all the human fungi. He says that *trichophyton tonsurans* is only the sporula form of *achorion Schönleinii*. He placed a mass of cells from favus in a solution of brown sugar, and at the end of five weeks he found complete plants of *aspergillus glaucus*, with the fructification in its characteristic form. Remak, who made similar experiments on the development of the achorion obtained no results from favus-sporules placed in a saccharine solution, but found that those placed on apple germinated and put out processes in twenty-four hours; but on the *sixth* day the apple was covered with *penicillium glaucum*. He says: "At this time, the surface of the apple changed its colour in a surprising manner to brown; there appeared on the entire surface a rapid growth of *penicillium glaucum* or other kinds of mould, which very much

interfered with the distinguishing of those fungi which belonged to the favus from those which belonged to other kinds of mould that has sprung up from unobserved sporules." He also states, that on the fourth or fifth day he had observed in the interior of the piece of crust a powdery substance evidently arising from a decomposition of this favus-fungus. In the saccharine solution he found the crust of favus "take the form merely of slender filaments of mycelium; and by the side of it a powdery mass, which probably owed its origin to the decomposed parts of the favus-fungus." (*Diagnost. und pathogenetishe Untersuchungen*, Berlin, 1845, pp. 200–2). Dr. Lowe thinks that the favus in Remak's experiments originated the *penicillium*; and that it, as well as *aspergillus*, is capable of originating the disease.

Is it not much more probable that the fungi in both cases were developed, not from the sporules placed there by experimenters, but from unobserved sporules, such as exist nearly everywhere? As Dr. Lowe says, the *aspergillus* is an extremely common fungus growing upon decay; and Remak took the view without hesitation.

Diagnosis is not difficult; at any rate, in the second stage. The roundish patches with clipped hair, roughened with branny scales and prominent hair-follicles, are phenomena not found in any other complaint. The smoothness of the patches distinguishes alopecia areata and the faintness of the hair-follicles; if any hairs exist on the patch, they are pale, and of the most downy description, not thickened and dark in colour.

The thick yellow crust of favus is characteristic, as is usually the peculiar smell. In the very onset of tinea tonsurans, you may not be prepared for so serious a disease, if you are not looking for it; erythematous rings, or raised spots, with branny scales, do not occur on the scalp from any other cause, so far as I know.

If the scalp have been much irritated, pus may be formed, and scabs cover the diseased patches; but there will generally be some portions which have retained the characteristic appearance of the tinea tonsurans. When pustules do form, so far as I have seen them, they are but small, and not followed by thick scabs.

It is not very often that vesicles can be discovered in cases of tinea, although it was called herpes tonsurans by Cazenave. The microscope comes in to make the diagnosis absolute, by exhibiting the peculiar fungus and the diseased hairs.

Treatment.—The main indication is to get rid of the parasite, just as in scabies to destroy the acarus. A variety of substances may be used, all of which have this effect. I have tried a large number, such as corrosive sublimate either in ointment or in solution, solution of sulphurous acid, applied on lint under oiled silk, nitrate of silver either solid or in strong solution, ointment of acetate of copper, blistering applications, and preparations of iodide of sulphur; or a mixture of one ounce of sulphur ointment, to two scruples of ammonio-chloride of mercury, as recommended by Dr. Jenner, I find none which answer so satisfactorily as the compound sulphur ointment of the Hospital for Skin Diseases:—

℞. Sulph. sublim., ℔. ss; hydrarg. ammonio-chloridi, hydrarg. sulph. cum sulph., āā ʒ ss. Leviga simul, dein adde olivæ olei, ʒ iv; adipis recentis, ʒ xvj; creasotonis ♏ xx. Misce.

It must not be expected that a cure will be effected in a week or two. If the disease have made much progress it will not be cured under several months. Epilation is recommended by M. Bazin. This is stated by Dr. Jenner to be impracticable; and I have found it to be so, except at the very onset, before the hairs are much attacked, and have become brittle. You may often fancy you have pulled out a hair by the root, when, on closer examination, you will find that you have only broken it off just as it emerged from the follicle. Shaving the head is generally desirable. The internal administration of cod-liver oil and steel is of course indicated in scrofulous and tuberculous children.

Tinea tonsurans is not a very common disease. Out of 7000 cases treated by me at the Hospital for Sick Children, from September 1858 to September

1861, there were only twenty-four cases of tinea tonsurans, of which three are mentioned as having been also affected with herpes circinatus; and five cases of the latter disease uncombined with tinea.

It is probable that there were more cases in which both the scalp and the body were affected with ringworm; because frequently, where two affections coexist, the major only is indicated in the hospital books, especially when they are so nearly allied as in the present case. The numbers just given do not fairly represent the relative frequency of herpes circinatus, because this is an affection which parents themselves frequently treat by the application of ink, or leave without treatment, as it frequently gets well spontaneously.

The best treatment for herpes circinatus alone is the local use of astringents, such as a strong solution of sulphate of iron, or a drachm of nitrate of silver to the ounce of water.

Conclusions.—Tinea tonsurans is a disease of the scalp, dependent on the growth of a peculiar fungus—trichophyton tonsurans.

Its growth is favoured by a weakly constitution and want of cleanliness; but these are not essential to its development. It is decidedly contagious; its growth is favoured by some atmospheric conditions more than others.

The fungus of tinea tonsurans is often found in the scales of herpes circinatus; but herpes circinatus may exist without it. Some cases of herpes circinatus appear to depend on the contagion of tinea tonsurans.

Pityriasis versicolor is another parasitic affection dependent on a fungus probably distinct from the trichophyton; but a sort of pityriasis may be caused by trichophyton, and other forms of it are non-parasitic.

In sycosis there is also a parasite; but whether it be the same as in tinea, is not established.

The fungi of tinea tonsurans and tinea favosa are quite distinct.

Two different fungi may be found on the same subject, one of them being accidental in its occurrence.

Many skin diseases not essentially parasitic may be occasionally the seats of a few sporules of fungi. Alopecia areata is probably one of this class of diseases.—*British Medical Journal, Nov.* 30, 1861, *p.* 577.

91.—ON THE TREATMENT OF SCALDS AND BURNS.

By Dr. John Young Myrtle.

[The plan followed by Dr. Myrtle for a long time has been the use of ointments, in such a form as most effectually to exclude the air, and at the same time to cool and soothe the parts.]

In employing the ointment plan of treatment, there is certainly nothing very novel; and the combination with sulphur may be regarded as of the same nature as the use of flour or starch made into a paste, or the Carron-oil mode of treatment. But as far as my experience has gone, sulphur seems to have almost a specific virtue of cooling and soothing the irritation of the injured part, and in removing the pain and inflammation which result from the application of heat.

When called to see a case of burn or scald, if nothing has been done, I immediately cover all the injured surface with a pretty stiff ointment, composed of simple hog's lard and flowers of sulphur, spread in the form of a good thick layer on linen cloth (which is greatly to be preferred to lint, as the latter is much more apt to adhere to surfaces deprived of their cuticle, if great care is not taken to remove the dressing), which ought always to be changed as soon as the part becomes hot and uneasy, when the sulphur is frequently found in the shape of a tolerably dry paste or crust. This crust ought to be carefully removed, and fresh dressing applied immediately, to be repeated as often as necessary. Under this simple treatment burns and scalds of a slighter character heal in two or three days; and even many cases which were wont to be tedious and troublesome under plans in common use, I find to heal kindly and speedily.

For some years after the adoption of the soothing plan in this form, combined with exclusion from the action of the air, I had the mortification to find that, when the scald has been very severe, or the cutis vera had been much injured by the burn, the cure was delayed by the occurrence of ulceration or sloughing, which I treated with the usual lotions, though too often with but tardy success. A good many years ago, in the course of a conversation with my friend Dr. Stark on the treatment of burns and scalds, I acquainted him with my ointment plan, and mentioned my occasional disappointments; when he told me that he had long used another ointment, which, when applied to the injured or ulcerated parts, generally induced a healing, reparative process far more speedily than could be obtained by the washes, &c., generally employed. The ointment in question is composed of common leeks and hog's lard, in the proportion of as much hog's lard as a slice of fresh butter for the table to each leek, which, being allowed to simmer by the fire in an earthen vessel until the leeks become quite soft and tender, and being strained through muslin, on cooling forms an ointment of a lightish green colour, with a slight garlic odour. This ointment I have had many opportunities of testing, and I can confidently testify to its great value in the case of scalds where ulceration has taken place, and of burns where the cutis vera has been materially injured.

In scalds and burns of a minor character I always trust in the first instance to the sulphur ointment; but when a burn is combined with a scald, or if an ulcerated spot appears, I have the leek ointment applied to those parts without delay, and am almost never disappointed in witnessing a rapid cure with comparatively little pain to the patient. In the case of severe scalds, as soon as ulceration threatens the vesicated parts, the cuticle ought to be carefully removed with a pair of forceps and very sharp scissors, so that the leek ointment may be adopted instead of the sulphur, when an hourly improvement will probably be observed.

I have also constantly used the sulphur ointment in cases of small-pox; and, when carefully and constantly applied, it has never failed to afford relief, and has almost entirely prevented pitting; indeed, so far as I have used it, I may say that I have not had a single case of disfigurement, even in cases of natural small-pox in adults. Its use also in the painful and troublesome excoriations of children is equally effectual and soothing. As to the leek ointment, it is as well to mention that I have frequently found it very serviceable in foul indolent ulcers, and also in gangrenous bed sores, and an instance of which may be mentioned in the case of a patient I attended for my friend Dr. Peddie, in which there was hopeless disease of the heart, kidneys, &c., accompanied with general dropsy. When I saw her on the 9th of September last, I found gangrenous inflammation of both lower limbs, extending from a little below the knee to the instep in the one leg, and to within two inches of the ankle on the other, each patch being fully two inches broad. A poultice was applied for a few days until the separation was fully established, the leek ointment was extensively applied, and though the lady died before the end of the month, the sloughs had not only come away, but the sores were rapidly contracting, and were in a wonderfully healthy condition. In a more favourable case, I am confident the best result might be expected. and troublesome granulations, such as generally accompany the continued use of poultices, might be in a great measure prevented.—*Edinburgh Med. Journal, Jan.* 1862, *p.* 641.

92.—ON THE TREATMENT OF VARICOSE ULCERS OF THE LEG WITHOUT REST.

By John K. Spender, Esq., Dudley.

[The "domette" flannel is composed of a mixture of flannel and cotton. It is superior to ordinary flannel as a material for bandages for the leg, being less heating and cumbrous to the limb. It is more elastic than the cotton bandage. Mr. Spender says:]

I am in the habit of using an ointment containing a very large quantity of alkaline earth (such as chalk), which, when spread thickly on lint, forms a sufficient protection to the sore, and neutralises the foul acrid secretion which often flows from it. The compound lead ointment of the *Pharmacopœia* answers tolerably well. A varicose ulcer thus dressed and bandaged generally heals with extraordinary rapidity; nearly all the pain ceases; and the patient may be advised to follow his usual duties or pleasures without the slightest hindrance. No medicine required, beyond what may be necessary to correct any constitutional disturbance, or to amend special dyscrasia.

I have found the domette flannel bandage very useful in other cases in which firm and persistent pressure is needed. The effusion which accompanies strumous synovitis of the knee-joint may be greatly controlled by this simple plan; and I may mention that it is of excellent service in promoting the cure of troublesome ulcers on the upper extremity.

Mr. Hilton has lately shown that "rest" is a most capital remedy; but there are instances in surgical practice in which "rest" is wholly unnecessary, and physiologically injurious. In the artizan and the tradesman, "rest" represents so much capital of bone and muscle lying fallow, and possibly becoming even the worse for it. Even the man of mere pleasure does not like to lie in bed for two or three months, if he can help it. We ought, therefore, to welcome a mechanical appliance of ready and easy use in the management of varicose ulcers of the leg, which simply aids the natural processes of cure, and accomplishes our work without that "rest" which is often a tedious and costly auxiliary.—*British Med. Journal, Feb.* 1, 1862, *p.* 129.

93.—ON THE TREATMENT OF INDOLENT ULCERS.

By J. Mitchell, Esq., Surgeon to the Lancaster Infirmary.

[The following is certainly a new method of treating old indolent, or callous ulcers generally, but more particularly of the leg. The healing process completes itself "in little more than half the time the ordinary treatment occupies."]

The method is very simple, and within the reach of every one. Supposing you have a case of old indolent ulcer, for example, all you have to do will be to wash the leg well; after so doing, fill up the excavated ulcer with fine powdered carbonate of iron, afterwards applying a large linen pad, without any moisture coming near; then envelope the whole limb in a starched bandage, allowing it to remain for three weeks or so, according to the extent of ulcerated surface. Another feature in this method is, that the patient need not be confined to bed, or be enjoined absolute rest, but can even walk a little every day. The limb must be kept perfectly dry.—*Lancet, Jan.* 4. 1862, *p.* 23.

94.—ON THE TREATMENT OF GOITRE WITH THE BINIODIDE OF MERCURY OINTMENT.

By H. M. Greenhow, Esq., Brevet-Surgeon H.M. Indian Army, Segowlee.

[Our readers will recollect an interesting paper about four years ago, by Dr. Mouat, of Bengal, (see "Retrospect," vol. xxxvii., p. 264.) in which the treatment of goître by biniodide of mercury was shown to be very successful in India. The present paper confirms what was then stated, and will refresh the minds of our readers on the subject.]

During the last six cold months more than 1500 people have been treated at Segowlee, and with the greatest success. Some cases required a second or even a third application of the ointment, but very few proved incurable. I may mention that I used a stronger ointment than was prescribed by Major Holmes or Captain Cunningham, for to 3lbs. of lard or fat, 12 drachms of biniodide of mercury were added. After this ointment was smeared over the

goitre, the patient sat in the sun with his neck well exposed. The effect was apparent in an hour or two, for a blister rose; but in the course of a week or ten days this healed, and left the tumours, if not completely cured, at least very greatly diminished. The people willingly paid their money, and, indeed, were eager to do so; but since the hot weather set in they have nearly ceased to attend.

My friend, Dr. Coates, Civil-Surgeon of Moteeharee, has kindly given me the result of his dispensary practice during the last six cold months. It seems he had a total of 13,067 cases, of which 7677 have been cured, 3318 relieved, 768 have not benefitted or have been found incurable, 937 have ceased to attend, and 367 are still under treatment. These results are very satisfactory, and prove the immense benefits to be derived from the use of the biniodide ointment. The remedy has, indeed, a wide-spread reputation in these districts, and is considered a certain cure. That being so, the question arises whether it might not be introduced into western countries; into Europe and America, into the valleys of Switzerland, the hills of Derbyshire, the valleys of the Ohio and the Mississippi. There is not, it is true, in all these countries, the same powerful sun which in India help us so in the action of this remedy; but where the sun is absent, it would be well to try whether a roasting fire would not supply its place, and raise the required blister. There can be little doubt that it would do so, and if those who make the experiment would communicate the results they met with to the Medical Times and Gazette, we might have a valuable record of the experience thus gained.

I have already (Indian Annals of Medical Science, No. 12,) entered so fully into the subject of goitre generally, and more especially into the disease as met with in India, that I do not propose now to discuss it; but there are one or two remarks which occur to me as worth making here:—

1. If there is no confirmation of my theory that drinking *lime-water* is the main cause of goitre, at least I have met with no facts tending substantially to shake that theory. All the well-water round this station contains much lime, and two wells in the lines of my regiment have acquired such a bad reputation for causing goitre, that the sowars prefer the river water for drinking purposes. About a dozen of them, who had never suffered from goitre before they came into this district,—men from Delhi and the Punjaub, who had not so much as heard of goitre—became affected with the disease in the last few months, but were easily cured by the biniodide treatment. The fact of these sowars, strangers in this part of the country, becoming goitrous, shows that the cause is a local one, and goes far to prove it to depend on the water of the district; for, except as regards the water, these men's food and drink were the same as those to which they had previously been accustomed. They all set their disease down to the water, and, I think, with reason; and as far as we know at present, the excess of lime in it is its greatest peculiarity.

I ought to say that Dr. Macnamara, the Civil-Surgeon of Tirhoot, has been conducting a series of experiments for some time past, which have for their object the elucidation of the cause of goitre; and I trust he may soon have some fresh light to throw on the matter.

2nd. As regards the connexion of goitre with cretinism, I have not seen any reason to change my opinion, that the two are by no means necessarily connected. Among all the goitrous patients treated by me, no cretins (or *baods*, as they are called here) were met with; yet there are many *baods* in the country round. These persons are, doubtless, often goitred, for they are exposed to the same causes of goitre as others; but if many hundreds of goitrous persons are brought promiscuously together, and no *baods* are found among them, that is strong *primâ facie* proof that the two diseases are not necessarily connected. Inquiries have been attempted to be instituted with a view to eliciting the causes of *baodism* here, the number of *baods*, male and female, goitred and ungoitred; whose parents had or had not goitres; who were born *baods*, &c., &c.; but no satisfactory results have been obtained. The difficulties of such an investigation are obviously very great.—*Medical Times and Gazette, Nov.* 30, 1861, *p.* 552.

DISEASES OF THE EYE AND EAR.

95.—ON THE USE OF ELECTRICITY IN CERTAIN FORMS OF PARALYSIS OF THE MUSCLES OF THE EYE.

By Dr. Soelberg Wells, Clinical Assistant to Mr. Bowman at the Royal London Ophthalmic Hospital.

[The patient had been suffering some weeks from paralysis of the external rectus muscle of the left eye, which was soon accompanied by an evident inward squint of the same eye. Dr. Wells says:]

I came to the conclusion that there was paralysis of the external rectus muscle of the left eye, with secondary contraction of the internal rectus of the same eye. My reasons for this opinion were as follows: The paralysis of the left abductor was proved by the want of mobility outwards of the left eye, by the disproportion between the movement outwards of this eye to fix an object, (the right eye being covered,) and the considerably greater inward associated movement of the right eye. This disproportion can only occur in cases of paralysis, for in common squint the secondary angle of squinting equals the primary—*i.e.*, the inward associated movement of the covered eye would correspond in extent with the outward movement of the squinting eye, whereas in our case it is not so, for the secondary angle of squinting is greater than the primary. Again, the paralysis of the abductor is proved by the phenomenon of the patient's inability to hit the object (the right eye being covered) when he strikes quickly at it with his hand. As some of my readers may not quite understand the reason of this, I will give, in explanation of it, an extract from my above-mentioned papers (*Ophthalmic Hospital Reports*, No. 9, p. 136):—

"If we close the right eye, and tell the patient to strike with his finger quickly (if he does it slowly he will have time to correct his mistake) at an object held somewhat in the left of his field of vision, he will miss hitting it, by going too much to the left; for the insufficiently innervated abducens demands, in order to bring the eye into the required position, as strong a contraction impulse as would, in a normal state of the muscles, effect a far greater amount of movement; therefore the patient at first, over-estimating the amount of contraction, believes the object to lie more to the side of the paralysed muscle than it really does, and consequently aims too much to the left of it. In time, however, if the paralytic affection be not too complicated, patients learn to correct these errors. The dizziness frequently complained of by them is not necessarily owing to any cerebral lesion, but is due generally to the confusion which arises from the difference between the real and imaginary position of the object."

The nature of the diplopia also pointed to a paralysis of the left abductor muscle. It was homonymous, showed only lateral differences (except in the diagonal position, outwards,) and the differences in the double images increased the further the object was removed to the left.

The second portion of our diagnosis—the secondary contraction of the left internal rectus muscle—was proved by the following facts: 1. By the convergent squint of the left eye, which existed not only when the patient looked at an object of eight feet distance, held in the middle line, but which extended even two and a half feet into the right half of the field of vision. Now, if it had only been a simple, uncomplicated paralysis of the left abductor, both optic axes should already have been fixed upon the object when it was held in the middle line. 2. The diplopia, which did not cease at the middle line, but corresponding to the squint, extended two and a half feet into the right half of the field of vision.

Let us now consider the cause of the affection. This appeared to be peripheral, *i. e.*, not dependent upon causes within the cranium, for there was no impairment of the mind, or of the sense; his memory was good, and there was no other impairment of mobility in any portion of the body. And in the absence of any symptoms of pressure, &c., upon the sixth nerve, within

the orbit, I presumed that the paralysis was most likely due to rheumatism; this being a very frequent cause of these affections, more especially during cold and wet weather, and I therefore gave a favourable prognosis, more especially as the paralysis was not of long standing. I, however, warned the patient that although the paralytic affection might be cured, some secondary contraction of the opponent and consequent convergent squint might remain, and demand an operation.

Treatment.—As the affection had gradually progressed even under the skilful treatment of his medical attendant, I naturally looked about me for some other plan of treatment, in the hope that this might prove more successful. As I had seen electricity very much employed in Von Graefe's clinique in paralysis of the muscles of the eye, dependent upon peripheral causes, and had seen many admirable results from its regular and prolonged employment, I determined to try its effect in this case, and am happy to say that it rapidly effected a cure. The iodide of potassium was continued till June 13, but I do not think that the favourable turn the disease assumed was due to the action of this remedy, which had before the application of electricity proved unavailing even to arrest the affection. In order to free the patient from the annoyance of the diplopia, I directed the left eye to be covered, the prisms required to unite the double images being too strong and clumsy to wear—(16° for reading, 32° for distance.)

June 30. I saw him again—he is greatly improved, the inward squint is reduced to about a-quarter of a line, and corresponding to this the diplopia extends only five inches (instead of two and a-half feet) into the right half of the field of vision.

August 2. Met him in the street, not the slightest inward squint, except on looking far to the left, is not at all annoyed by diplopia except in the same direction, but can follow his professional duties, read, write, &c., without any inconvenience. Left off electricity July 15.

September 3. The mobility outwards of left eye almost normal. No convergent squint until the object (held at eight feet distance) is moved four feet into the left half of the field of vision when diplopia arises. I wished him to resume the electricity in order that the affection might be completely cured, but he had no opportunity of doing so, and I therefore ordered him to practise the left abductor by means of a prism of 5° with its base turned inwards

November 12. The mobility outwards has increased a little more, the outer edge of the cornea can be almost brought to the outer canthus. The convergent squint and diplopia now only arise when the object (still at eight feet distance) is brought five and a-half feet into the left half of the field of vision. The patient considers himself perfectly cured.

The electricity was at first applied for five minutes daily, by Mr. Crosse, and this time was gradually extended to twenty minutes. One electrode, covered with moistened sponge, was applied to the skin of the closed eye exactly over the paralysed muscle, the other electrode was placed on the temple. One of the rotating magneto-electric induction machines was used.

Electricity certainly answered better in this case than I expected, for not only was the paralysis of the external rectus all but cured, but as this muscle increased in strength, the secondary contraction of the internal rectus was gradually overcome and the squint disappeared. Had the electricity been continued but a few weeks longer, I firmly believe that the paralysis would have been *completely* cured, the patient, however, felt no annoyance from the slightly remaining paralysis, and therefore really did not care much about resuming the treatment. I may be met by the opinion that the iodide of potassium, and not the electricity, produced the benefit, but how is it then that under its use the patient got no better? as soon, however, as electricity was applied, the improvement commenced and gradually and steadily continued.

Electricity is indicated in paralytic affections of the muscles of the eye dependent upon peripheral causes, its action is especially marked in cases produced by rheumatism.

In conclusion, I must warn the reader against ever using the *continuous* current in applying electricity to the eye, on account of its deleterious effect upon the retina; we should therefore always employ the extra current of an induction machine, using either a magneto-electric or volta-electric apparatus: each has its advantages, for a full consideration of which I must refer the reader to books on electricity, more particularly to Dr. Althau's excellent work on Medical Electricity.—*Medical Times and Gazette, Dec.* 28, 1861, *p.* 660.

96.—ON THE INSUFFLATION OF CALOMEL IN CERTAIN DISEASES OF THE EYE.

By Dr. Soelberg Wells.

[The insufflation of calomel in certain ophthalmiæ, particularly in specks of the cornea, was in frequent use many years ago, but has fallen into disuse. It has, however, been lately advocated by Von Graefe.]

Its use is particularly indicated in the following affections:—

1. *In the different forms of phlyctenular or pustular* (or what is often designated scrofulous) *ophthalmia.* It is not only of very marked advantage during the presence of the disease itself, but it is also of the greatest use in preventing the constant recurrence of this affection, which forms such a source of discomfort and annoyance to the patient. I think it, therefore, of consequence that the insufflation should be persisted in, at the interval of one or several days, according to the requirements of the case, even for some time after the disease itself appears cured; when its use is definitely abstained from, strict injunctions should be given to the patient to have the remedy at once re-applied, as soon as any symptoms of recurrence shew themselves Although it should generally only be used when there is hardly any inflammatory irritation, I have often employed it with the greatest advantage in these cases of pustular ophthalmia, when there was considerable vascularity and some lacrymation. In such a case, however, we should see the patient every day, for fear that too much reaction might be set up. If this cannot be done it is better to abstain from its use until the amount of irritation has decreased. The insufflation of calomel is contra-indicated when there is great irritation, vascularity, photophobia, with very copious lacrymation: or if there are pustules or abscesses of the cornea.

2. *In the pannus due to phlyctenular corneitis.*—But if this form of pannus is accompanied by corneitis fasciculosa (frequently called scrofulous corneitis; the distinctive marks of the disease being an elevated infiltration, which is pushed forward towards the centre of the cornea at the extremity of a bundle or leash of vessels,) calomel should not be used as there is too much irritation, and red precipitate ointment should be employed. The indication for the use of the latter is, the presence of vascularity of the cornea, with no tendency to a general detritus (necrosis) of the corneal tissue.

3. *In the regressive stage of this fascicular or scrofulous corneitis* (when the infiltrations have disappeared) calomel is indicated, in order to prevent a relapse.

4. *In phlyctenular corneitis* (sometimes called herpes cornea, also described as ophthalmia scarlatinosa, morbillosa, &c., as it is frequently met with during the period of desiccation in scarlatina, measles, &c). At a later stage of this affection, when the severe inflammatory irritation, with its increase of temperature, arterial hyperæmia, lacrymation, &c., has passed away, calomel often acts as a specific, almost as a charm.

5. *In various forms of opacity of the cornea,* more particularly if they are not too dense, or of too long standing, and if there is still some slight irritability of the eye, and where we fear a recurrence of the original affection. The great advantage of calomel in such cases is, that we can thoroughly regulate and control the degree of its action, which is also not too irritant.

The calomel to be used for insufflation should be very finely powdered and perfectly dry, so that it does not form clots on the conjunctiva or cornea,

which are apt to act as mechanical irritants. It should be applied with a fine camel's hair paint brush, held between the thumb and forefinger, a slight fillip with the middle finger against the handle of the brush will jerk some of the powder into the eye. It should be applied every day, or every other day, according to the requirements of the case; if after its application the eyelids are much gummed together in the evening it should be employed less frequently. In children it is exceedingly difficult to get anything into the eye, on account of their restlessness, or the intense spasm of the eyelids. In such cases it is best to place the head of the child between the knees of the surgeon, so as to fix it thoroughly, its legs and hands being held by an assistant, who sits on another chair opposite, the eyelids are then easily opened by the elevators, and the eye may be thoroughly examined, and the application, whatever it be, efficiently applied. Simple as this hint is, it is yet of importance, as all will admit, who have witnessed the, but too often, vain endeavour to gain a glimpse at the cornea or to apply some lotion, &c., in some of these cases.—*Ophthalmic Hospital Reports, Jan.*1862, *p.* 314.

97.—ON THE USE OF FORCEPS IN EXTRACTION OF CATARACT.

By John F. France, Esq., Surgeon to the Eye Infirmary, Guy's Hospital.

[The importance of fixing the eye effectually can hardly be exaggerated; for from the spasmodic movements of the organ mainly arise the accidents to which this operation is obnoxious. For the purpose of holding the eye fixed nothing is better than a pair of ordinary artery forceps.]

A pair of toothed artery forceps should be selected for this purpose, the dentations of which close at the extreme point of the nibs; as, if the latter are rounded off, so that their teeth do not project and bite at the very point, the forceps is likely to take hold only of the loose conjunctiva. The patient (being recumbent), is first desired to look upwards, while the operator at his head depresses the lower lid. The open nibs of the forceps are then applied upon the exposed globe beneath the cornea, and made by slight pressure to scrape along the surface as they close; in order to seize, together with a fold of conjunctiva, the tissues beneath (including, if possible, a few fibres of the inferior rectus tendon), and convert the forceps into a firm handle to the eye. The instrument is now delivered to an assistant, resting his hand upon the cheek; while the operator proceeds to raise the upper lid, and apply his fore and middle fingers above and on the nasal side of the globe in the usual manner, thus consummating his command of it.

Should any inconvenient reflection from the surface of the eye, the natural conformation of the parts, or other circumstance, render an alteration in the exact position of the cornea desirable, this can now be effected at pleasure by a word to the assistant, who can gently draw it into, and (the surgeon's fingers co-operating) retain it steadily in, the precise position that is required. Meanwhile the attachment of the forceps serves simultaneously to keep the lower lid depressed, and enables the contact of additional fingers to be dispensed with. All things being thus prepared, the knife can be deliberately inserted, carried in a uniform, undeviating course across the anterior chamber, and be brought out accurately at the nasal margin of the cornea. No irregular movements of the globe delay commencement of the section; no spasmodic inversion, without previous warning, obscures its progress, and invests its completion with sudden difficulty and danger. But the cornea remains stationary and central, alike while the cutting instrument pierces its temporal margin, while it divides the texture continuously, and when again it emerges at the nasal edge.

As soon as the cornea is fairly transfixed by counter-punctuation, and a narrow isthmus alone remains for division, the knife itself holds the eye still, the iris lies safely behind the instrument, and the forceps must be detached at the same moment that the pressure of the fingers is withdrawn. The sec-

tion is then completed, and the operation from this stage (which is, indeed, the turning point of the whole) proceeds in the ordinary manner.—*Guy's Hospital Reports, Vol.* vi., 1860, *p.* 78.

98.—ON PARACENTESIS OF THE CORNEA.

By GEORGE LAWSON, Esq., Surgeon to the Great Northern Hospital; Clinical Assistant to Mr. Bowman.

[There is often considerable advantage if not an absolute necessity in tapping the anterior chamber of the eye, in cases of sloughing ulcers of the cornea, or of ulcers which refuse to heal under other modes of treatment.]

The operation is a simple one, and best performed by the manner usually adopted by the surgeons at Moorfields, viz., by passing a broad needle through the cornea at its lower margin, keeping the point well forward towards the cornea, to avoid wounding the lens, and then suddenly turning it on its edge so as to allow the aqueous to run off, and rapidly withdrawing it, as soon as the iris approaches the cornea.

The indications which call for this line of treatment may be briefly stated.

1st. Increased tension of the globe. The eye, in any of the above-mentioned cases, may have its tension slightly increased, and this seems, in a great measure, to depend on an increased secretion of the aqueous, for the anterior chamber becomes deepened; and this is specially observable when contrasted with that of the other eye, and the iris, instead of presenting a plane surface, slopes backwards.

2nd. Deep ulcers which threaten to perforate the cornea will often rapidly assume a healthy action after the tension of the cornea has been diminished by letting off the aqueous humour; and sloughing ulcers will, under the same treatment, derive similar benefit.

That this benefit is often only transitory is true, but the operation is so simple, that it may be repeated an indefinite number of times, if the patient after each derives relief.

3rd. In cases of onyx or pus between the laminæ of the cornea, the relief of the tense state of the cornea promotes absorption, and relieves pain, and so places the eye in a favourable condition for complete recovery. An onyx which threatens to burst backwards, can be most safely combated by tapping the anterior chamber of the eye in addition to the use of other remedies.

4th. This operation relieves pain, and, if carefully and properly performed, can do no hurm. The relief of pain is so remarkable, that patients, on their next visit to the hospital, will, unasked for, relate the great benefit they derived from what had been done to the eye; but at the same time they will often state that after about twenty-four hours the pain was nearly as bad as ever. The truth being that the aqueous had become completely restored, and that the tension which called for the first tapping was as great as ever.—*Ophthalmic Hospital Reports, Jan.* 1862, *p.* 317.

99.—ON THE ADVANTAGES OF HAVING SOME EYE-INSTRUMENTS GILDED, WITH A NOTE ON NON-METALLIC INSTRUMENTS.

By J. F. STREATFEILD, Esq., Surgeon to the Royal London Ophthalmic Hospital.

Occasionally we may see an instrument used in ophthalmic surgery that has been gilt, but this distinction seems to have been without any other purpose than that of ornament. No one eye instrument that I know of is *always* gilt, nor is any one *class* of instruments commonly thus distinguished; but there are practical advantages in having a gold surface, to some of them at least, that I have learnt not to neglect. The first instrument I have had gilt was my spatula-hook; for in the operation of corelysis, when the instrument, passed behind the pupil, is in apposition with the lens. it is not easy to tell,

and it is so much the more important to know the exact position of the instrument, and the direction it is made to take. I found, as I expected, that in using this instrument it enabled me to see better what I did in all that I had to perform. Then I had an iris-hook gilt, and, for the operation in which it was employed, I again found the advantage of the yellow metal; and now I intend to have the canula-forceps, and scissors, and other iris instruments coated in the same manner. I am inclined to think that at least all instruments used for operating on parts within the eye should be gilt—the cutting instruments may be an exception—but of those that can be gilt the lighter colour will make them much to be preferred to the dark polish surface of steel. In an operation, there are two reasons why a steel instrument is not easily seen in the eye—it is so dark that, in the anterior chamber, it probably does not contrast well with the colour of the iris, and it has so smooth a polish that, in the deeper parts of the eye, it does not generally reflect enough light. In the aqueous chambers its highly polished surface reflects the light very strongly from some one part or another of the instrument (it is not easy always to tell whence it comes), or the instrument cannot be well seen because the light is hardly at all reflected from it. A surface of gold is perfect with a much less degree of polish, for it will not rust, which is another especial advantage for eye instruments, because of the tears and other watery fluids with which they are brought into contact; for this reason, eye instruments generally, being so small, would be kept in better order if they were gilt. Steel, even with care, will rust if there is any want of smoothness on its surface. Dull gold, on an instrument of polished steel, will show light whichever way the instrument is held.

I do not know why any small blunt instruments which we employ should be always of steel or any other metal, and therefore, wishing to have a spatula-hook to be as conspicuous as possible in contrast with the iris and its brown adhesions, I have had one made of ivory, and, though it is so very small, it is tough, and very little strength is required in it, as, for other reasons, no force may be used in the detachment of iris adhesions. In corelysis, lately, I have employed this instrument, and have found it practically answer my purpose better than any other that I have of metal.—*Ophthalmic Hospital Reports, Jan.* 1862, *p.* 342.

100.—ON THE USE OF ATROPINE-PAPER.

By J. F. Streatfeild, Esq., Surgeon to the Royal London Ophthalmic Hospital.

At the present time, when atropine is so much used for ophthalmoscopic purposes, as well as for the treatment of iritic and other cases, I hope the mode of employing it, which I beg to introduce to the notice of oculists, for their convenience, is very opportune. It is simply a coloured tissue paper imbued with a solution of the sulphate of atropine, of such strength that a small square piece of it, of a certain size, is equal to or contains as much of the salt as a drop or minim of the solution of a strength in ordinary use. The paper, soaked in the strong solution, is hung up to dry, and is turned about while drying, that the atropine may be equally distributed. The little piece of paper to be used, one fifth of an inch square, is taken up on the tip of the fore-finger, previously dampened, and the patient's lower lid being drawn down, he is told to look upwards, and the scrap of paper is put on the sclerotic conjunctiva, below the cornea, almost without the knowledge of the patient; the lid is then let go, and the piece of paper is left between the ocular and palpebral conjunctivæ; a handkerchief is then tied over the eye, that the lids may be kept closed for a while.

The atropine thus used has, I find, acted at least as well as the drops of the solutions that are commonly employed. I think, as is likely, it has acted more powerfully than the drop of any equivalent solution, of which so small a quantity really remains under the eyelids when they are closed after its instillation. The tissue-paper becomes at once wet and soft with tears, lies

flat on the eye-ball and creates no more conjuntival redness or lacrymation than the drop of solution.

At first, when the present plan suggested itself to me, I used white paper, but this, soaked in tears, becomes transparent, and is hardly distinguished from the conjunctiva; since then I have adopted green paper, for red might not be plainly seen in any case of external redness of the eye, and strips of blue or yellow paper might be mistaken for litmus or tumeric paper. The green paper is always easily recognised: it contains neither copper nor arsenic. In a quarter of an hour, or whenver the mydriatic effect is produced, the scrap of paper is easily removed with the tip of the forefinger from under the lower lid, which is depressed for the purpose; if the little piece has got under the upper lid, it will be floated down by shutting and opening the eye a few times, and may be wiped out, as before, without any difficulty.

The sole object I had in view, when this paper was first prepared for me was, as I have said, the convenience of those who, like myself, have constantly to be provided with atropine for use. I have carried about a small bottle of the solution in my pocket, and sometimes I had left it at home when I wanted it, but now I have always in my pocket-book some of the paper ready for use, almost without weight, occupying hardly any space, and making no separate item to be remembered. A glass bottle may be protected from breaking or escape of the fluid, but all bottles are cumbersome, and the many different kinds of atropine drop-bottles, almost every oculist having his own pattern, prove that there was no very convenient method of carrying the agent, which in practice we most require; the paper will, I believe, be found not only convenient in this way but very efficacious. Its portability is perhaps the great advantage of the atropine paper, but, recollecting the small quantity of any solution dropped into the eye that is probably retained when the lids are closed, and its further dilution by the flow of tears generally produced, I think the atropine paper may be proved to be very powerful in those cases in which the *greatest* mydriatic effect is desirable; for the whole of the dose used is first placed within the lids, and lacrymation at all does not usually follow the use of the atropine paper, therefore I have been using it in cases of old synechiæ before operating for their detachment. The salt in the meshes of the paper is dissolved out gradually, and with very little, if any, increase of tears, is mixed with them to spread itself over the surface of the conjunctiva. Having thus explained the mode of its operation, I intend to try if the same paper vehicle will not be advantageous in the use of other drops and lotions for the eye. Perhaps a strip of paper, soaked in a strong solution of sulphate of copper, cut of the size of the cartilage of the upper lid and laid upon its under surface when the conjunctiva is "granular," will not improbably act more efficiently than the drop of solution of the same salt, and less painfully than when it is applied in the solid form, the greater effect being attainable by the *real* use of as much of the salt as is contained in an ordinary drop of the solution used, and the pain being, I suppose, mitigated by a *gradual* application of the sulphate of copper instead of its being used at once in the solid form; but this remains to be proved.

The small space the atropine paper will occupy in the pocket is not the only convenience to be found in its use. It does not alarm timid patients as a bottle of drops (believed to be "caustic") and a quill, brush, or glass tube brought up to the eye does: children are often then quite intractable, and, if they cry, the atropine solution is often too much diluted to be of any use. The atropine paper, it will be observed, can never become a common mode of employing the agent by the patients themselves, for very few of them or their friends could be trusted to put the paper properly into the eye, or safely to wipe it out again. The atropine paper is, again I state, only brought forward by me for the use of the surgeon himself, chiefly for convenience, and also for greater efficiency in some cases in which the greatest effect is desirable.

The atropine paper, as I have described it, is prepared by Mr. Squire, chemist to Her Majesty, 277 Oxford street, London—he will, as I have re-

quested him, send any one who will try the new vehicle, a specimen, by the post. The paper has been dipped in a solution so strong that a piece of it, one-fifth of an inch square, contains as much of the salt as a drop of the solution of two grains to an ounce of water. It is made up in little books with stiff covers, each page is an inch square, with black lines dividing it into fifths both ways; thus each page contains twenty-five doses. All the pages are cut through, in the horizontal lines, from the front edges almost to the back, so that the quantity required each time can be easily out (or torn) off, in the vertical lines, from either of the horizontal strips into which the paper are divided.—*Ophthalmic Hospital Reports, Jan.* 1862, *p.* 310.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

101.—ON DYSMENORRHŒA AND STERILITY.

By Dr. JOHN COGHLAN, Wexford.

[Some time since Dr. Coghlan, in an article published in one of the journals, expressed his belief in the mechanical theory of dysmenorrhœa. A further experience and careful attention to the subject have convinced him of the correctness of this opinion. In the more severe forms of the disease we find a permanent narrowing of the canal, such as to prevent our passing a bougie, even of the smallest size, into the cavity of the uterus without considerable difficulty.]

I had a severe case of dysmenorrhœa about three weeks ago, for the relief of which I had intended incising the canal, but on examination, finding it easily dilatable, I rolled up one of my thin leaden tubes very small, passed it well into the canal by means of the instrument hereafter described, and dilated it to some extent. In two days after, though a week before the proper time, menstruation came on, and the period passed over with very little pain. I did not see her for some days after she was well, and found, on examination, the tube had, without her knowledge, fallen out, but her comparative freedom from pain is, I think, presumptive proof that it had remained in during the period.

In a recent number of the "Medical Times and Gazette" I read an admirable lecture bearing on this subject, delivered by Mr. T. Spencer Wells at the Samaritan Free Hospital, and am happy to think that obstruction at the os and cervix uteri is being properly appreciated as a cause of dysmenorrhœa and sterility. Now, with regard to the relieving of these cases by operation, I think that when we find a tight, unyielding constriction, the proper proceeding is to incise the os and cervix uteri by means of the probe-pointed knife described in my paper June last, and the reason I particularize this knife is that I believe that by no other means can we in those cases requiring it, perform the operation so perfectly, safely, and expeditiously, and with so little pain to our patient. In the first place, with ordinary care, we are sure to divide the os internum; secondly, we must, with or without the speculum, go in the right direction, and the incision is made at once equally on both sides, and with but little pain. Now, suppose in lieu of it that we decide on using the sheathed bistoury. In most of these cases we can, with difficulty, introduce a No. 1 Bougie, and consequently we cannot introduce the sheath of even a single blade strong enough to cut the dense structure with which we have to deal; but, supposing that we could introduce it, in withdrawing it we cut but at one side; we then, should it have a regulating screw, double the projecting power of the blade, reintroduce it, and use the wound just made as a support for the sheath whilst we incise the opposite side. The unfixed state of the uterus and the density of the parts would necessitate all these proceedings if the surgeon wished to have both sides incised equally. Suppose we use a probe-pointed bistoury, it would require the exercise of both skill and care to use it safely for our patient, and would necessitate the use of the speculum. These are the reasons why I prefer the probe-pointed knife to any other instrument. The knife I at present use is not so full

where it springs from the probe-point as that which I described in my paper, and has a rather deep circular groove at one inch and three-quarters from the point, for the purpose of serving as a guide to the finger, or to the eye when the speculum is used, as to the depth to which the knife has penetrated. By having it marked so far from the point, we can easily judge of the proper distance to introduce it, both in the healthy state of the organ and in those cases in which we find it hypertrophied.

In manufacturing the instrument, the stem was left soft, so that it can be bent to suit the varying obliquity of the uterus, at the same time that it is sufficiently stout for the purpose intended. Before using the knife, I introduce a small bougie in order to be certain that the canal is permeable by its probe-point. Now, with regard to the result of our incision, judging from analogy, I would certainly say, that unless cicatrisation was completed over a metallic plug, that the contraction would become as great as ever. In the cervix uteri we have the cut surfaces kept in contact both by the natural shape of the part and by the muscular fibres which surround it; and interrupt the process as we may by the occasional introduction of the finger, I am inclined to think that some amount of adhesion would take place, which, assisted afterwards by a contracting cicatrix, would, in the course of time, leave our patient in about the same state as before the operation I do not for one moment doubt that pregnancy and other advantages have followed the operation at the hands of the distinguished and talented surgeons who practice it; but I am much inclined to think that these advantages have occurred before cicatrisation and the succeeding contraction were complete. With the object of making the operation of incising the os and cervix uteri permanently valuable, I recommended, in my former paper, that a coiled-up tube of thin sheet-lead should be introduced immediately after the incision, and dilated by means of a long, slight dressing forceps; but, having sometimes found this process not easy of execution, I devised a rather simple instrument, by means of which I never fail in easily introducing, dilating, and leaving in the canal, the leaden tube, which I believe to be absolutely necessary to the ultimate success of our operation. This instrument consists of two blades, forming when closed a slender conical beak one inch and a-half long, terminating in two long handles with a spring and regulating screw. The following wood-cut is one-third the size of the instrument:

Dr. Coghlan's Dilator.

When about to use the dilator, we must fully separate the handles and secure them so by means of the screw; then take a piece of sheet-lead rolled out to about the substance of a thin visiting card, from an inch and a quarter to an inch and a half long. Wrap it tightly round the beak of the instrument and cut off the superfluous parts, leaving it of sufficient width to form a tube of the size we require. Having perfected our incision of the os and cervix, we introduce the beak of the dilator up to the shoulder which supports the leaden tube, we then unscrew the nut to the extent we wish to dilate the tube and press the handles together, by that means the blades of the beak are divaricated and the tube dilated. We then take off the pressure from the handles, the spring closes the beak, which should then be gently disengaged, leaving the tube in the canal. The shoulder of the instrument, in addition to being wide enough to allow of the tube being spread out a little at the lower part, should be rather near the joint, in order by increasing the divergence of the points to keep the leaden plug more securely in its place. As, if too distant, the blades being conical would leave the sides of the tube nearly parallel.

In those cases of dysmenorrhea and sterility in which we have reason to

suppose that obstruction exists, but where we find the os and cervix dilatable, I think that our proper proceeding is to introduce the leaden tube at once by means of the dilator without incising the parts; I have yet had little experience of this proceeding, but think that it will be found valuable. In the dysmenorrhœal case which I have before mentioned, whether it was that the patency of the canal was preserved for the time, or that the nervous irritability of the organ was diminished by the presence of the foreign body, the comparative freedom from pain was quite remarkable.—*Medical Times and Gazette, Feb.* 22, 1862, *p.* 185.

102.—*Veratrine in Painful Menstruation.*—The severe pains sometimes attendant upon the expulsion of the menstrual coagula may often be relieved by large doses of castor: and M. Vannaire strongly recommends in this mechanical form of dysmenorrhœa, an ointment containing one-hundredth part of its weight of veratrine, from fifteen to thirty grains of which are lightly rubbed into the hypogastric region twice a-day. Four or five applications have always sufficed.—*Revue Méd., and Med. Times and Gazette, Dec.* 7, 1861, *p.* 585.

103.—RETAINED MENSES OF TWO YEARS' DURATION, CAUSED BY ATRESIA VAGINÆ, AND TREATED BY PUNCTURE OF THE UTERUS PER RECTUM.

By I. Baker Brown, Esq., London.

[The patient was 15 years of age, single. The symptoms were great pain at every monthly period, without any show of menses. Latterly the pain had been constant and excruciating. The uterus externally felt the size of a four months' gravid organ, and it could be felt bulging on the rectum.]

Nov. 21st, Mr. Brown introduced a curved trocar into the uterus per rectum, and fastened the canula in by tapes. At the time of the operation a good deal of treacly-looking matter escaped through the canula, and continued to do so for two or three days after. On the 24th she had some pain, followed by a discharge of healthy and fresh menstrual fluid through the canula. On the 27th, the bowels were opened, the canula being still in. The canula was kept in for a fortnight, the bowels acting regularly every day. On the 2d of January, after two or three days' suffering of uterine pain, she again menstruated per rectum, and shortly afterwards returned home quite well.

Dr. Routh, after thanking Mr. Brown for his contribution, remarked that the case was satisfactory as one of recovery after liberating a menstrual accumulation per rectum. It was important because usually, when an opening for the same purpose was made per vaginam, death was the result. How far, therefore, did recovery depend upon the rectal opening? It was well known we occasionally met with large ovarian cysts within the pelvis, which gave rise to so much distress and pain that the liberation of the contained fluid became imperative. Now he believed five such cases had occurred in the Samaritan Hospital, all of which, except one, had died after the operation. The continual drain kept up of purulent matter, which usually became very offensive, exhausted the patient in the end, although the immediate effect was relief. Did this depend on the passage of air within the cavity? It was known as a practical point, already insisted upon by Dr. Oldham, for reducing retroverted wombs, that if the vagina was opened and pulled backwards a rush of air took place, and the vagina was pulled out, and so the reduction effected. Did not something like this occur in those cases where the opening was made by the vagina, which was prevented by the sphincter ani from taking place when the opening was made per rectum? *A priori*, we should expect the reverse, from the probable passage of flatus through the opening made per rectum into the cyst or uterus. It was known how intensely pu-

trid were the contents of abscesses near the rectum, from the passage of fæcal matters or flatus within them; yet recovery was not uncommon when the opening was made per rectum. Did anything depend on the vaginal opening, when made, remaining unclosed? Altogether he thought it an important surgical point to determine, which he hoped on some future day to take up and embody in a paper for the Society.

Mr. SPENCER WELLS agreed with Dr. Routh, as to the necessity for inquiry into the relative safety of tapping by the vagina or by the rectum in cases where ovarian cysts were adherent within the pelvis. He had tapped by the vagina in three patients in the Samaritan Hospital. In the first case the operation was performed more than two years ago; and although the woman could not be said to be entirely cured, as some discharge still went on, yet she was in so good a state of health as to be able to walk two or three miles. In a second case, the woman died a few months after the tapping, exhausted by continuous discharge. In the third instance the patient gained great temporary relief, but the sac was filling again. In cases where he had tapped by the rectum, he had not seen the same suppurative inflammation of the lining membrane of the cyst as after tapping by the vagina; but he had seen troublesome dysenteric tenesmus from the constant discharge of ovarian fluid into the rectum. He could not believe that it was better to have the fetid gases formed in the intestines entering an ovarian cyst than the pure atmospheric air; and he had seen cases where ovarian cysts had opened spontaneously into the rectum, or into some other part of the intestinal canal, prove rapidly fatal after putrefactive fermentation of the ovarian fluid. It was a very important practical question, which required further observation.—*Lancet, Feb.* 18, 1862, *p.* 175.

104.—*Living Spermatozoa in the Vagina Eight Days and a Half after Intercourse.*—In the American Medical Times, for March 9th, Dr. S. R. Percy reports a case in which, eight days and a half after the last sexual connection he found semen issuing from the os uteri. A microscopical examination revealed "living spermatozoa, and many dead ones." During this time the husband of the patient had been from home. Of the honour and veracity of the patient, Dr. Percy says: "I would stake my reputation on her honour." Dr. Percy thinks this fact may have a medico-legal bearing, and will necessarily widen the margin to the ordinary term of pregnancy, granted to exceptional cases.—*British Med. Journal, Dec.* 7, 1861, *p.* 605.

105.—*Use of Oxalate of Cerium in the Vomiting of Pregnancy.*—By ALBERT LLOYD, Esq., Bermondsey.—[The following case related by Mr. Lloyd, is of some interest.]

Mrs. J., aged 26, the mother of one child, of very anæmic appearance, has been out of health for the last fourteen months, during which time she has miscarried twice; on the last occasion had much hemorrhage. On October 22d she applied for medical treatment. When I visited her I found her suffering from pain in and about the right ovary; but her pulse did not seem to indicate that it was of an inflammatory character, neither was the pain much increased on pressure over the affected part. The next day, however, a most marked change had taken place, as could be seen at once in her face. Pulse 160, thready; great pain over the right iliac and hypogastric regions, which became intense on pressure, with other symptoms of peritonitis. Her case was rendered the more distressing by her continuous retching. Everything taken into the stomach, however small in quantity, was instantly rejected; the effervescing mixture with prussic acid, ice with the fluid given, increased rather than checked it. I thought I would try the effect of oxalate of cerium, so highly esteemed by Dr. Simpson, of Edinburgh, for the sickness of pregnancy. I ordered her three-grain doses every two hours,

to be placed on the tongue dry, with a small piece of ice to assist in swallowing each powder. All vomiting ceased after the second or third dose, and she was now able to retain the brandy and eggs, which were before rejected. All irritability of stomach having subsided, I ordered her twenty minims of Battley's sedative solution every three hours, and after being under its influence thirty-six hours, she rapidly recovered, and was up in a fortnight.

I send this case more in reference to the oxalate of cerium here exhibited. I read in your last impression an inquiry as to the success of this remedy in the sickness of pregnancy. I know it to have been very useful in several cases; dose, three grains every three hours. In two cases, however, it failed. —*Lancet, Nov.* 30, 1861, *p.* 538.

106.—*Use of Pepsine in the Vomiting of Pregnancy.* By GEO. SELWYN MORRIS, Esq., Guisbro'—[Speaking of the oxalate of cerium this writer says:]

I have used it (oxalate of cerium) in six cases lately, where sickness and retching were most urgent in pregnancy. I cannot say that I am at all satisfied with the results. It is true three of the cases were relieved by it; but in order effectually to stay the urgent vomiting, I was obliged to have recourse to my favourite drug (pepsine), in ten-grain doses. I gave the oxalate of cerium in tragacanth powder, two to three grains for a dose, three times a-day.—*Lancet, Nov.* 30, 1861, *p.* 538.

107.—RETROVERSION OF THE GRAVID UTERUS: THE BEST POSITION DURING ITS REDUCTION.

By Dr. THOMAS SKINNER, Liverpool.

[During the year 1860, Dr. Skinner published some papers on this subject. (See "Retrospect," vol. xliii., p. 278.) Objections have been made to the position of the patient recommended by Dr. Skinner, and also to the introduction of the hand into the rectum instead of into the vagina. The present paper is an answer to these objections. He says:]

The position which I have put forth, and which I still maintain is the best, is "the patient upon her left side, the pelvis well raised, the shoulders low, and the thighs as much flexed upon the abdomen as possible." It is objected to this position that it ignores the aid of gravitation; but I cannot see how it does so. I think that the position sufficiently favours gravitation; and that in the great majority of cases it will be found quite enough so for all practical purposes.

It has been recommended in preference, that the patient should be placed with the lower extremities in bed, the pelvis over the side of the bed, and the elbows and hands on the floor; and the observation has been made that this position can be maintained for a considerable period. I cannot agree in this position, as in Mr. Wall's and many other cases where this position was tried, the taxis had to be stopped and renewed at intervals, the position being found to be both irksome and tiresome. This position is further open to the following objections, namely, that of raising unnecessary fears on the part of the patient; of being most repulsive to the natural modesty of even the most callous of the sex, and the fact, that the reduction has been and can be easily accomplished without it; besides to invert a female in the puerperal condition should never be done if it can be avoided, as it determines the blood to the head and chest, harasses the breathing, and entirely prevents the aid of anæsthetics.

With regard to the statistics of the point at issue, I may state that I have collected all the facts on record connected with seventy cases of retroversion of the gravid uterus; of these, nine were placed with the lower extremities in bed, and upper extremities on the floor. In five of them, the taxis was successful, while in four, it entirely failed; of the four failures, three died. In the re-

maining sixty-one cases, although the position assumed is not positively stated in all of them, yet there are sufficient facts to lead me to infer that they were not inverted. They were placed chiefly upon the left side, upon the back, and a few of them on "all-fours." In the great majority of these sixty-one cases, the taxis (chiefly on the left side or back) was successful; and in a few of them, catheterism alone, without the taxis, was sufficient to reduce the displacement. In fourteen cases, the patients died. Add to these statistical facts, that I have myself repeatedly reduced the retroverted gravid uterus with ease; and that I have never employed any other position but the one I have recommended (except once in a difficult case, where I placed the patient on her back); and I think that we have sufficience of guarantee, not only of the success attending the practice, but of evidence that the profession hitherto have not generally adopted the practice of inverting the patient; nor do I think that they are likely to do so even now, except under the pressure of circumstances, or as a *dernier ressort*.

It has been argued, that inverting the patient is best; because the uterus is pear-shaped. Let me remind those who think so, that the uterus begins to lose this form about the third month; that between this term and the fifth month (the most prevalent period for the occurrence or the detection of retroversion), it gradually becomes spherical; after which, and up to the full term of gestation it again becomes pyriform.

Lastly, objections have been made to the hand being slowly introduced into the rectum (as a last resource, and when all other means have failed, puncture of the bladder or uterus being excepted), and there seems to be some doubt entertained as to the possibility of doing so. There need be no doubt, as the operation has already been done by several of the most distinguished members of the profession, and with comparative success; as for instance, in Mr. Weir's case, when all the most approved methods had been attempted in vain. The operation, or a somewhat similar one, is performed very frequently for *fissura ani;* I have seen Professor Simpson do so; and I have repeated the operation myself with the greatest ease. It is not nearly so difficult an operation as *à priori* we might be led to suppose, especially when we resort to the use of certain adjuvants I have alluded to in my essay, namely, steaming, the warm bath, inunction, venesection, tartar emetic, opium, and more particularly the use of chloroform.

As the rule in ordinary cases, I have recommended the first *two fingers* of the right hand *per rectum.—British Med. Journal, Jan.* 4, 1862, *p.* 7.

108.—ON A NEW MODE OF ADMINISTERING CHLOROFORM.

By Dr. J. Y. Simpson, Edinburgh.

[At a meeting of the Edinburgh Obstetrical Society, held recently,]

Prof. Simpson stated, that for some time past he had used chloroform in a manner somewhat different from that in which he was formerly in the habit of administering, and recommending it to be administered; and he believed that by the new method the patient was more rapidly anæsthetized, and at the same time a great saving was effected in the quantity of the drug employed. The difference in the two modes of administration consisted in this, that according to the old plan the fluid was poured upon a cloth folded into several layers, and the hand of the administrator had to be kept between the cloth and the patient's face, in order to ensure the due access and admixture of air; while, in following out the new method, *one* single layer of a towel or handkerchief was laid over the patient's nose and mouth, taking care not to cover the eyes, and on this single fold the chloroform was poured, drop by drop, until complete anæsthesia was induced. There was little or none of the drug lost by evaporation when it was administered in this manner, for the patient inhaled it at the moment when it was poured on the cloth, and inhaled it mixed with a sufficient quantity of air, which was easily inspired through a single layer of any ordinary napkin. Dr. Moir had been long in

the habit of administering chloroform in this way upon a handkerchief thrown over the patient's face. He (Prof. Simpson) believed it would add to the safety with which the drug might be employed, to administer it in the manner he had described. He had often feared lest the lives of patients should be sacrificed by the careless manner in which, in particular, students and young practitioners sometimes applied the damp folded cloth over the patient's face, without admitting a sufficient supply of air; and he had no doubt that many of the deaths attributed to chloroform were due only to the improper administration of it, and were, consequently, no more chargeable on the drug itself, than were the many deaths resulting from accidental overdoses of opium, &c. But the dangers from careless and improper administration would be diminished if there were never placed over the patient's nose and mouth more than one single layer of cloth, moistened with a few drops of the fluid. The first patient to whom he had administered it in this manner had been chloroformed several times previously, and had never gone to sleep till an ounce and a half or two ounces of the fluid had been employed; but when administered drop by drop on a single layer of a thin towel, one drachm had sufficed to induce the most profound sleep. It had thus all the advantages that had ever been claimed for the complicated apparatus which some medical men were still in the habit of using. There was only one precaution to be attended to in employing chloroform in this manner, viz., care should be taken to anoint the lips and nose of the patient beforehand with oil or ointment, to prevent the skin from being injured by the contact of the fluid that resulted from the close application of the wetted towel to the patient's face.

Dr. PETER YOUNG stated that he had recently administered chloroform in the manner described by Prof. Simpson, to a patient in labour, and that he had kept her asleep for ten hours with only two and a half ounces of the drug.

Dr. KEILLER remarked upon the difficulty that was occasionally experienced in putting some patients to sleep, and stated that a hysterical patient then in his ward had used an enormous quantity of chloroform before she could be brought under its influence. But he had no doubt that had it been administered in the manner Dr. Simpson had just proposed to do, a much smaller quantity would have sufficed; and he might state that when he himself suffered from severe ear-ache on one occasion, he had put one or two drops of chloroform on cotton wadding, and held it into his mouth, and a very few deep inspirations served to relieve the pain. He (Dr. Keiller) had since then frequently applied chloroform very advantageously in this way in cases of tic and other neuralgic affections.

Dr. ALEX. R. SIMPSON had administered chloroform in the manner under discussion in a considerable number of cases, and always with the result that a great saving was effected in the quantity employed. He had the day previously kept a boy, on whom Mr. Edwards was operating, asleep for more than half an hour with between two or three drachms of chloroform, and some of the fluid first poured out was lost in consequence of the resistance made by the patient to its administration.—*Edinburgh Medical Journal, Dec.* 1861, *p.* 593.

109.—INDUCTION OF PREMATURE LABOUR IN A CASE OF ALBUMINURIA WITH PUERPERAL CONVULSIONS.

By Dr. ROBERT BARNES, Physician to the Royal Maternity Charity.

The following case exhibits, in a striking manner, the remarkable power of bringing gestation to a close which we possess in the operation of dilating the cervix uteri by means of fluid pressure:—

6th November, 1861. My assistance was requested by Dr. Tidy, of Hackney, in the treatment of a case of convulsions. The patient was a young woman, the wife of a butcher, and had, till her present illness, enjoyed good health. She married in March, and was believed by her friends to be in the

seventh month of her first pregnancy. Some weeks ago, slight swelling, impeding the free use of her hands, was observed; but there was nothing distressing until the 3d instant. Since this date anasarca has become rapidly general and considerable. On the 5th, having complained of blindness, she was seized with eclampsia, and remained more or less comatose. Dr. Tidy bled her to ten ounces. The effect of this bleeding was very satisfactory. There were no fits after eight A.M. of the 6th, but she lay comatose, though not always profoundly so. The bowels were freely relieved by a purge. I saw her at half-past five P.M. on the 6th. She was semi-comatose; did not answer questions; the pupils could not be observed, owing to the strength with which she clenched the eyelids when touched; pulse 100; tosses about; face, neck, and hands much swollen; face cyanosed. Urine had been freely passed in bed. Blood drawn yesterday shows a large, loose, red clot, not buffed. The os uteri was not at all open, but admitted tip of finger on pressure; it was reached with difficulty. The persistent coma, which had already lasted more than twenty-four hours without signs of remitting, determined us to deliver. The membranes were punctured with a knitting-needle; a little liquor amnii escaped; the head presented. The patient was left for two hours, to allow the uterus to contract, and prepare for expansion of the cervix. At eight P.M. we met again. A pint of urine was drawn off. The os was but slightly open; there was no indication of labour, and the patient's general condition was not more promising. I applied a caoutchouc dilator, distending it with water, in the first place, to the vagina, so as to remove all obstructive rigidity in this part of the parturient canal. The vagina was quickly expanded, giving more freedom for further manipulation, and slightly opening the os externum uteri. I then applied a dilator mounted on a flexible metallic stem inside the cavity of the cervix; by gradual distension during thirty minutes, a considerable expansion had been gained; three fingers could be introduced, and feeble labour-action was excited. She was given thirty minutes more to allow this action to work. At nine P.M. the os uteri was a little more dilated; but the pains were so feeble that there seemed no certainty of delivery being effected spontaneously for many hours. I therefore sought to deliver by bimanual version; but owing to the great length of the cervix and of the uterus proper, which was a rigid cylinder, the absence of liquor amnii, and the tonic contraction of the uterus, I did not succeed readily. A loop of cord came down before the head; it was pulseless. The head was perforated at once, and forty minutes were spent in careful extraction by the crotchet, the bones affording but slender hold. The uterus contracted well, and cast the placenta. Chloroform was administered during the operation for the purpose of securing the quiesence of the patient.

On the 13th November I learned that consciousness quickly returned after the delivery; the anasarca soon disappeared, and the patient did well.

The urine drawn by catheter was acid, turbid, loaded with albumen—specific gravity, 1024; it contained 1·66 grains of urea in 100 grains. After standing forty-eight hours it yielded abundant crystals of uric acid, and numerous sporules and strings of sugar fungus.

Commentary.—The interest of this case consists in the rapidity, certainty, and safety with which labour was induced and completed. Under the ordinary methods, delivery could not have been counted upon in less than twelve hours, if even so soon. Coma would have persisted, eclampsia would almost certainly have returned, and death would probably have supervened whilst delivery was being expected. By the method adopted, in two hours security was attained for the mother, relief from anxiety by the friends, and for the medical attendants there was the pleasing consciousness of having triumphed over a formidable disease. I know of no obstetric operation more certain or more successful. It is a saving operation in some of the most appalling and imminent emergencies in midwifery. It obviates the loss of time, where the loss of minutes is the loss of life.—*Lancet, Jan.* 24, 1862, *p.* 4.

110.—CASES OF CONCEALED ACCIDENTAL HEMORRHAGE AT THE FULL TERM OF UTERO-GESTATION.

Dr. J. G. Wilson, Physician to the Glasgow Lying-in-Hospital

[A case of concealed accidental hemorrhage, especially where it occurs before delivery, is a complication of an alarming and very deceptive kind. It is very embarrassing to the inexperienced practitioner, since the cause of the syncope and other symptoms is not immediately apparent.]

Case 1.—On the 9th of May, 1855, I was called to see Mrs.——, aged 34, a pluripara, of a lax, leuco-phlegmatic habit of body, and who was on the eve of her confinement. This forenoon, when actively employed in the discharge of some household work, she was suddenly seized with great faintness and exhaustion. On the occurrence of these symptoms I was requested to visit her. On my arrival I found her in a state bordering upon complete syncope, with a small thready, tremulous pulse, and general pallor and coldness of the surface of the body. She complained much of nausea and retching, restlessness, vertigo, dimness of vision, ringing sound in the ears, &c. I immediately prescribed brandy and ammonia, which had the effect of reviving her a little. There was no appearance whatever of any external hemorrhage. The abdominal enlargement was such as is usually met with at the full period of pregnancy. Towards the right side of the fundus uteri there was a distinct, prominent, and circumscribed swelling, which was not previously observed by the patient. This swelling had a peculiar doughy feel, and was the seat of a peculiar "bursting" sensation. The patient had no labour-pains, and the os uteri was only so far dilated as to admit the tip of the examining finger. Suspecting internal or occult accidental hemorrhage, I punctured the membranes and administered ergot with the view of inducing labour. In the course of three hours thereafter uterine action supervened, and in four hours subsequently I applied the forceps and delivered the patient of a full-grown but still-born child. The infant presented a very anæmic appearance. During labour very firm external pressure, by means of a compress and bandage, was applied over the abdomen, and particularly towards the fundus uteri. Notwithstanding the free administration of brandy and other stimulants, the patient all this time suffered much from faintness and depression. There was, however, no perceptible increase of the swelling or fulness above mentioned. Immediately after the removal of the placenta two or three very large coagula were passed *per vaginam*. There was no hemorrhage of any consequence after the discharge of these clots. The placenta, on examination, presented a peculiar bell-shape appearance. It had a large cup-like excavation or concavity on its uterine surface, which was fully occupied by coagulated blood. This hollowed out appearance was evidently occasioned by the pressure of the effused blood. The placental substance, towards the centre especially, appeared much denser than usual—this was also ascribed to the compression exerted by the extravasated blood. The placenta seemed free from all disease. The patient made a slow recovery, but eventually did well.

Remarks.—In this case I think that the sudden and extreme exhaustion was, beyond all doubt, due to occult intra-uterine hemorrhage. The appearance of the placenta was such as to warrant the supposition that the hemorrhage was in a great measure, if not altogether, confined within the limits of the placental site. The fulness observed, and the "bursting" sensation experienced on the right side of the uterine fundus, was evidently caused by the circumscribed effusion of blood in the utero-placental region. From the pale and exsanguine aspect of the infant, I am satisfied that its death was occasioned by the hemorrhage. To no other cause could I attribute its death. The same plan of treatment should, in my opinion, be had recourse to in cases of this kind as would be adopted were the blood making its appearance outwardly.

The following is a rare instance of fatal hemorrhage, altogether confined between the placenta and uterus:

Case 2.—In September, 1853, along with the late Dr. Alexander Stewart, I inspected the body of a woman who died suddenly undelivered, apparently at the full period of pregnancy. On inquiry we ascertained that she was engaged with her ordinary domestic duties, in her usual state of health, till within two hours of her decease. Sudden and alarming faintness was the first symptom that attracted the notice of her friends, and she is reported to have been "just out of one faint and in to another" till she died. No blood had escaped externally. The abdomen generally presented the usual appearance observed at the termination of the ninth month of gestation. On the left side of the fundus uteri, however, a prominent but irregular fulness or swelling was observed, which had a soft doughy feel, and emitted a dull sound on percussion. On opening the uterus a full-grown female infant was found with the dead presenting. The child had a blanched exsanguine appearance. The membranes were unruptured. The liquor amnii was quite untinged by blood. The swelling above referred to was found to correspond exactly with the situation of the placenta. Except at its extreme circumference, the placenta was wholly detached from the uterus, and the intervening cavity was distended by a very large quantity of blood in a partially coagulated state. The placenta was of the average size, and exhibited no appearance of disease. Careful examination of the other abdominal and thoracic viscera revealed nothing abnormal.

Remarks.—This case is interesting not only on account of the concealed character of the hemorrhage, but also in consequence of its confined situation, and the unfortunate result to which it led. It clearly shows and fully proves what I know is disbelieved by some, and very much doubted by many, viz., that a quantity of blood, sufficiently copious to destroy maternal life, may be effused between the uterine surface of the placenta and the corresponding uterine wall. The placenta in this case, as in the last, presented a peculiar bell-shape, or basin-like appearance; with the exception of its margin, it was entirely separated from its connection with the uterus, and the interspace between the two contained an immense quantity of coagulated blood. This detachment of the placenta was apparently of spontaneous origin, there being no evidence of external violence. It is certainly remarkable that the edge or circumference of the placenta should have maintained so perfect and complete adhesion to the uterine parietes, while the central portion was subjected to so much stretching and distension. Two cases somewhat similar to the one now related are mentioned by Drs. Hardy and M'Lintock, in their "Report on Midwifery and Puerperal Diseases," (p. 194). An instance of the same kind is recorded in the "New Medical and Physical Journal," (1813, No. 38, 535). The post-mortem examination in this case showed—"that a separation of the centre of the placenta from the parietes of the uterus had taken place, whilst its edges were completely adherent, forming a kind of cul-de-sac, into which blood had been poured to the amount of a pint and a half, which had become coagulated within the cavity thus formed."

The subject of concealed uterine hemorrhage appears to me not to have received from writers on systematic midwifery that amount of attention and consideration which its importance demands. In many obstetric treatises no allusion or reference whatever is made to such an occurrence. The "Glasgow Medical Journal" for April, 1854, contains a very excellent paper on this subject, by Dr. Joseph Bell of this city. Dr. Bell illustrates his paper by the details of three most interesting cases. In the Transactions of the London Obstetrical Society, for 1860, will be found a valuable communication on the same important subject by Dr. Hicks, of Guy's Hospital, in which he relates twenty-three instructive cases, collected from various sources. Both of these papers will amply repay careful and attentive perusal.—*Glasgow Med. Journal, Jan.* 1862, *p.* 439.

111.—ON THE MANAGEMENT OF THE PLACENTA.

By Dr. P. McLoskY, Rothwell, Northamptonshire.

[It is a singular fact that at the present time there should be so much difference of opinion as to the proper mode of conducting the third stage of labour. All agree that where the placenta remains entirely, or in part, in utero, and flooding occurs, the practice is, to remove it; but a wide difference exists as to the manner of doing this.]

I am of opinion that a man always carries about with him the requisite machinery for successfully overcoming all the difficulties of the third stage of labour. This machinery consists of his head and hands; and I would reckon it bungling practice where the woman would be left with the placenta, or part of it, *in utero*, or permitted to die from uterine hemorrhage. Without offering any opinion with regard to what I consider the best practice in managing the various difficulties liable to arise in the supplemental stage of labour, I shall briefly state what my practice is to prevent their occurrence; for I know that, however fond of abstract reasoning men may be, they can seldom be reasoned out of their habitual practice: a little compliment to their own self-conceit.

I may state that, in every labour which I conduct, whether hemorrhage be suspected or not, my practice is invariably the same, and in no single instance do I avoid adopting precautions. When the head and shoulders have been expelled, the perineum no longer needs support; the left hand takes charge of the child, to protect the air-passages and prevent sudden expulsion; the right hand, either passed over the right side, or slid betwixt the thighs (the woman, of course, being on her left side), is placed on the fundus and body of the uterus. The pain which delivers the nates is the signal for the right hand to grasp the fundus with a firm, steady, downward, moderate pressure. On the contracted ball of the uterus, the hand of an assistant is placed in a similar grasping manner, the child being separated and given to the nurse. The right hand, after being rapidly run along the cord to ascertain the placental position, is again immediately placed on the uterus, the cord being run around the fingers of the left hand close to the vulva. Gentle tension shews the strength of the cord; then if the uterus continue hard and equally contracted, alternated pressure on the fundus, body, and cervix, with moderate traction of the cord, is kept up till the placenta is either expelled or comes into the vagina, when an expiratory effort with steady pressure on the uterus, aided by the left hand's traction on the funis, or on the margin or fold of the placenta, accomplishes the delivery. When relaxation occurs, smart friction with points of the fingers pressed against the walls of the uterus; pinching, kneading, and otherwise manipulating in a lively manner over its whole surface, soon produces partial or entire contraction, when the organ can again be grasped, and pressed gently downward, all the while keeping a strain on the cord. When the placenta is high towards the perineum—when low and in the vagina towards the pubis—for several minutes after the expulsion of the placenta, the right hand every few seconds is passed over the uterus to feel if it remain hard; if not, a more gentle manipulation is practised till it become so, and then the bandage applied. In the great majority of cases the placenta is expelled within ten minutes of the birth of the child. I care not to wait for pain, I create it. The peculiarity of this is, that the uterus has constant support from the moment of the fœtal, up to that of the placental expulsion by the practitioner's own hand, a living sentient compress.

The *rationale* of this is: We know that the uterus has motor power from three nervous sources—the cerebral, spinal, and ganglionic. It has an independent power of its own also. We know that the abdominal muscles have a powerful influence on the expulsive power of the uterus, by the immense pressure rousing its reflex actions, and we know that the sudden withdrawal of their support from more than one viscus produces syncope, sometimes death; and in case of the uterus, inertia, or spasmodic contraction: therefore the continuous support of the hand, not simply on the abdomen, but actually

on the uterus, supplies that of the muscles, while the frictions, pinchings, and various manipulations excite its reflex and peristaltic actions, partly by the irritation of pressure, and partly by exciting its ganglionic reflex action through volition acting on the muscles of respiration. In certain cases, say dropsy, fatty degeneration, phthisis, or hemorrhagic diothesis, to these means might be added the centre spinal action, induced by ergot administered a few minutes before delivery; but my main reliance would be on *my hands*, as it would be in every hemorrhage of this stage of labour.

For the last eight years, in a midwifery practice not inconsiderable, I have invariably adopted this mode; and during that time I have not met one single case of uterine hemorrhage, neither have I ever known the least ill result from the amount of pressure used. By this mode every uterus can be made to contract except a moribund one—and I have such confidence in its safety, usefulnesss, and superiority, that I shall never cease to adopt it. It has now been tried by me in over two thousand cases without a single accident.—*British Med. Journal, Feb.* 1, 1862, *p.* 116.

112.—PARTIAL PLACENTAL-PRESENTATION AT EIGHT MONTHS.

By Dr. Francis H. Ramsbotham, Obstetric Physician to the London Hospital.

At 4 P.M. on January 23, 1840, I was sent for by a medical friend to Mrs. P., Bethnal-green road, in labour of her fifth child, near the end of the eighth month of gestation. She had suffered occasional attacks of hemorrhage for between two and three months. On that morning uterine action commenced, and a large and sudden gush of blood occurred, producing faintness. The discharge continued in a trifling degree throughout the day, with slight pains. I found her very much depressed; os uteri dilated to a size larger than a crown piece; the membranes still whole; fully a third of the placenta hanging in the vagina; head presenting. I ruptured the membranes, hoping that the pains would come on more actively, and that the head, being pressed strongly against the os uteri, would plug the bleeding vessels, and thus put a stop to, or at least materially diminish, the draining that was going on. Ergot was also given, with brandy, to excite increased action. These means not producing the desired effect, I determined to deliver by turning, and did not expect to experience much difficulty. In this again I was disappointed. Though the os uteri was so widely open, the cervix resisted the introduction of the hand, and I never fully passed it into the cavity. I managed, however, to get down a foot, and was thus enabled to "turn." The delivery occupied half an hour; the chief difficulty consisted in bringing the head through the os uteri, which part of the operation took up twenty minutes. The child was dead. The placenta was expelled immediately, and the woman recovered without a bad symptom.

N.B.—I have often experienced much difficulty in endeavouring to introduce the hand into the uterus, for the purpose of turning the child, when the case has been premature by one or two months, from a constricted ring within the os uteri, although that organ itself has appeared sufficiently flaccid to admit it easily. And this I have attributed to the undeveloped state of the cervix, especially since I have found that the farther from the end of gestation the labour has come on, the greater has been the resistance opposed.—*Medical Times and Gazette, Jan.* 4, 1862, *p.* 4.

113.—ON PREPARING FOR TURNING IN DRY LABOURS.

By Dr. Langer, Davenport, Iowa.

Dr. Langer speaks in the highest terms of the following plan, which he adopts for facilitating turning, when this is required in "dry" labour:—

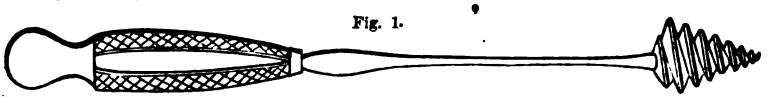
"Previous to turning, I place the patient on her back, side, or on her elbows and knees, as the case suggests, the better to enable me to introduce into the os tincæ two or three fingers to reach the child. With these I endeavour to carry between the head and shoulder, if it is a shoulder presentation (or near any convenient part according to the malposition), a large elastic catheter, whose orifice and pointed end are filled, to the length of one inch, with clean lard, which has been kept at a low temperature, the mouth-piece of the catheter being attached to an elastic tube connected with a stopcock and an elastic Davidson or other forcing pump. Before connecting the catheter with the pump, I fill the catheter with sweet oil, at blood temperature, and lock the cock to keep the air out. I now endeavour to introduce the catheter as high up as practicable into the cavity of the uterus; better, if possible, between the ovum and inner walls of the uterus, but always opposite to the attachment of the placenta. Having reached, with the point of the catheter, the required height, I connect the catheter with the pump, filled with tepid oil, the free end of the pump being immersed in a vessel containing oil kept at the same temperature. I inject, with a small degree of force, in the interval of pains, from a pint to a quart or more of oil, directing the patient, between the injections, to change her position from back to side, or elbows and knees, and *vice versa*, even to sitting or walking. On one occasion, when I could not get oil, I used the white of an egg. From this simple operation I have noticed the most pleasant results—namely, I have seen patients who had been for twenty-four hours and more in intense suffering, in a comparative short time calming down, with contractions of the uterus less annoying, the uterus, too, becoming more pliable to the hand for the operation of turning. No great difficulty was then experienced, and the employment of force was not required. Nay, I have succeeded even, after such preparation, in changing a malposition into a normal one, by the combined method of internal and external manipulation, without introducing the hand into the cavity of the uterus. The patients so treated have had less symptoms of nervous shock, and have exhibited far less the consequences of the dreaded operation, and consequently their recovery has been more speedy."—*American Medical Times, and Medical Times and Gazette, Nov.* 16, 1861, *p.* 511.

114.—A NEW INSTRUMENT FOR CRANIOTOMY.

By Dr. Lewis D. Harlow.

This instrument, Figs. 1 and 2, which I have named the craniodiaclast (signifying an instrument for *breaking through* the skull), or more briefly, the *diaclast*, is unlike any other heretofore in use, as will be seen by the following description:

Fig. 1.



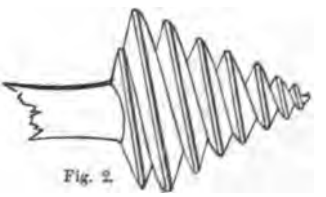


Fig. 2.

(Cranio-) Diaclast.—Fig. 1. View of instrument, reduced size. Fig. 2. Screw, natural size.

The handle and shank are similar to those of the common trocar, differing only in respect to size, and are, when measured together, eleven inches in length. But the chief peculiarity of the diaclast is that portion used for perforating the head.

This consists of a large cone-shaped screw, resembling in form a boy's top, with which every one is familiar. Its length, and the diameter of its base are *equal*, each being one inch or a little less.

The threads of this spiral cone, or conoid screw, are small and near together just at the point, but increase regularly and rapidly, in size and distance, and becomes rounded and smooth as they wind from the apex to the base. They present, therefore, no sharp, cutting edges, by which the soft parts of the mother can possibly be wounded; the point only requiring to be guarded until brought in contact with the child's head.

The small end is like that of a common pointed screw, and is made of the best tempered steel. It readily penetrates the cranium by a semi-rotary, drilling motion, and enters by a few facile turns, wholly within the cavity. This entrance is effected (after the penetration of the point) by the mechanical power of the screw alone; as the spiral beyond the point has no *cutting* edge; but that power has been proved experimentally to be amply sufficient.

As it makes its way through the skull, the bone is broken in pieces, and the fragments are forced asunder, as if the head were being riven by a wedge. Its withdrawal in a direct line requires considerable force, but has the advantage of still further breaking up the bones and leaving in the carnial vault a large open rent, through which the craniotomy forceps can, without difficulty, be passed.

On making trial of this instrument upon the well ossified heads of still-born children, it was found to penetrate, with the greatest ease, the base of the skull or the face, at any point, and separate the fractured pieces, at the will of the operator; thus showing its perfect adaptation to cases of breech-presentation (after the body has been delivered), and of face presentation, when these are complicated with so great pelvic deformity as to render embryulcia unavoidable.

The advantages of the diaclast may be summed up as follows:—

1st. It is simple in construction.

2d. It is perfectly safe.

3d. It may be used with the greatest facility.

4th. It is efficient and adapted to all cases.—*American Journal of Medical Sciences, Jan.* 1862, *p.* 281.

115.—ON THE UTERINE DOUCHE AS A THERAPEUTIC AGENT: NEW INSTRUMENT.

By Dr. Graily Hewitt, Physician to the British Lying-in-Hospital.

The employment of the uterine douche in the treatment of certain diseases of the uterus was, the author remarked, no novelty. The beneficial action of cold water applied by means of the douche to the portion of the uterus accessible to its action from the vagina, in the treatment of chronic leucorrhœa and allied affections, had been recognised by many authorities. But although the principle in question was recognised, it could scarcely be said that the use of the uterine douche was by any means widely practised; in point of fact, it was very rarely employed by the profession at large. One reason why the douche was so little employed was, that there was no ready and efficacious mode of administering it. The various forms of injection apparatus procurable were all more or less open to one of these objections: the quantity of fluid capable of being used was too limited, or manual effort was required, or possession of ingenuity on the part of the patient was essential. In order to administer the douche effectively an instrument is required at once portable, necessitating no mechanical or other effort on the part of the patient, and admitting the use of a large quantity of water. The fatigue attending the use of ordinary forms of injection apparatus interfered most seriously with their usual employment. The high opinion which the author entertained of the therapeutic power of the uterine douche induced him to devise an instrument which should fulfil the necessary indications.

The apparatus now exhibited to the Society (ingeniously constructed under the author's directions by Mr. Thos. H. Savory, of the firm of Savory & Moore, of New-Bond street) is extremely portable; the whole is contained in a box seven inches across and four inches deep. It consists of an india-rubber vessel, folding up like a "Gibus" opera-hat, and from the bottom of which a long flexible tube conveys the fluid. When opened out, the rigidity of the reservoir is maintained by a brass rod screwed in its centre. The reservoir holds nearly a gallon of water, the whole of which can be used at one operation. The action of the douche, which is not that of the syphon, is dependent entirely on gravitation; all that is necessary to set it in action is that the reservoir be placed on some object a foot or two above the seat or couch on which the patient is reclining. On turning the stop-cock the water flows continuously, fast or slow as may be desired, through the vaginal tubes until the reservoir is empty. The instrument possesses all the requisite qualities: it is portable, self-acting, and not likely to get out of order.

In cases belonging to any of the following categories, irrigation of the os and cervix uteri, which could be most perfectly and easily carried out with the aid of this instrument, would be found of very great service:—Cases of profuse menstruation, dependent on the presence of a lax condition of the vessels of the uterus, giving rise in many cases to prolapsus uteri; chronic leuchorrhœa, proceeding from the same cause; enlargement of the uterus, proceeding from defective involution after pregnancy or after abortion; cases of engorgement of the lips of the os uteri, associated with hypertrophied condition of the mucous membrane and excessive secretion of the mucous follicles of the cervix; in all cases, indeed, in which there is undue fulness of the uterine vessels, or defective tonicity of the muscular fibre generally. —*Lancet, Feb.* 22, 1862, *p.* 202.

116.—*Arrest of Secretion of Milk.*—Dr. Harriss, of Savannah, employs a saturated solution of camphor in glycerine in preference to belladonna. It is gently applied over the surface by means of flannel, several times a day. The same solution, with the addition of four grains of tannin to the ounce is of utility in sore nipples.—*American Med. Times, No.* 15, *and Med. Times and Gazette, p.* 511.

117.—ON PUERPERAL FEVER, REGARDED AS AN EPIDEMIC.

By Dr. R. Uvedale West, Vice-President of the Obstetrical Society of London.

[Dr. West gives a numerous series of cases of puerperal fever. No other hypothesis than that the infection was conveyed by the accoucheur and communicated by manipulation will explain the facts, unless, indeed, we may look on the whole series as an epidemic, a view which would seem to be corroborated by the occurrence of well-marked diphtheria as a symptom in two of the cases. He says:]

On the 4th of March, 1859, I was hastily summoned to visit a woman in the country who had three days previously been delivered by a midwife after an average easy labour—fifth pregnancy. I found her suffering from acute peritonitis, with a peculiar swollen condition of the abdomen, which I judged, from what I could learn of the patient's previous history, was caused by the presence of an ovarian tumour. The woman was very bad, and died three days after, March 7th. I found, on a post-mortem examination which I made the following morning, a large unilocular cyst of the left ovary filled with the usual fluid and occupying the abdomen, so as to push up the small intestines into the left hypochondriac region. The peritoneal covering of the cyst was black and gangrenous, as was that of the small intestines. The colon was much distended with flatus. It was, therefore, a case of acute peritonitis, involving chiefly the peritoneal covering of an old standing ovarian cyst, and terminating rapidly in gangrene. My attendance on this case,

and especially perhaps the post-mortem examination of the subject of it, were followed in my midwifery practice by an unusual proportion of febrile diseases of the puerperal state, caused either by direct contagion or by some epidemic influence, that I think it may be instructive if I introduce in this place some account of the whole of the cases of midwifery attended by me during the months of March, April, May, and June; with which last month the epidemic, if epidemic it was, ceased. I may premise that having felt a wholesome dread of the possible consequences to my lying-in patients of my having conducted this unlucky autopsy, I took every possible precaution, by way of disinfection, such as many ablutions, entire change of wearing apparel, &c., to obviate what I was afraid would take place. On a gross loss of forty-eight midwifery cases attended between the 8th of March, the day of the post-mortem examination, and the end of June, there were ten cases of puerperal disease sufficiently grave to excite considerable apprehension for the result, although they all recovered. This was surely a very undue proportion. Although some of them, as did the case from which they all appeared to originate, exhibited inflammatory symptoms, they were all, equally like that case, more or less asthenic in type, and were benefited by stimulant or tonic treatment.—*Glasgow Med. Journal, Jan.* 1862, *p.* 404.

118.—ON PUERPERAL FEVER.

By a Correspondent of the "Medical Times and Gazette."

The building of a new Lying-in Hospital in Munich, was commenced in 1855, and patients were first admitted into it in 1859. Unfortunately, puerperal fever soon became endemic in the Hospital, and no year has yet gone by without us having been visited by this terrible scourge. The most intense epidemic was that of last year, and I will to-day communicate to you some of the most important observations which were made at that time by Dr. Hecker, who is Professor of Clinical Midwifery, and Dr. Buhl, the Professor of Pathological Anatomy.

During summer and autumn of 1859, the sanitary condition of the women in the Lying-in Institution was, on the whole, satisfactory. Certain premonitory symptoms of an epidemic of puerperal fever having been observed towards the close of the year, the first bad cases of that disease occurred in the middle of January, 1860. The epidemic lasted from that time till July. There were 663 births in the six months, and 80 women were affected with the fever, that is twelve per cent. Of these 80 there died 33, that is 41 per cent. of those affected, and 5 per cent. of the whole number. Simultaneously with the puerperal fever a very dangerous affection of newly-born infants was observed, which was quite analogous to the disease from which the mothers suffered. Amongst the 33 children of the 33 women who died, 20 also died within the first week, and evidently under the influence of the epidemic. Of 49 children borne by 49 women who recovered, 12 died from the same cause. The disease of infants showed the following forms:—

1. Acute dissolution of the blood; of which 13 cases were observed. The children collapsed soon after birth in an extraordinary manner; the skin assumed a dirty yellow colour; food was refused, the respiration became accelerated, and screams were heard as in acute hydrocephalus. Death generally ensued within 24 hours, and the post-mortem examinations which were made in every case with the utmost care, did not show any local disease; only the blood appeared smeary, and there was a striking putrefaction of all organs, which are prone to this process, and which had evidently commenced during life; this was observed even if the cavities of the body were opened soon after death. Cadaveric spots were spread over the whole surface of the skin. This septic condition generally began immediately after birth; but in one case Professor Hecker satisfied himself that it had already commenced in utero. This was the case of a woman in her second pregnancy, and who fell ill during parturition with violent vomiting of green matter; she then gave

birth to a dead child, and died herself on the third day after delivery. The child was a well-developed girl, of 6 5-8ths pounds, and had died during parturition without any visible cause, as the umbilical cord had not been constricted, and the second period of parturition only lasted an hour and a-half; from which it was fairly concluded, that the illness of the mother, which had come on during parturition, had caused the sudden destruction of the life of the infant. If in such cases of dissolution of the blood, a local disease occurs, this affects the respiratory organs; the autopsy showing signs of lobular pneumonia having commenced. In two cases there was extensive hepatisation of both lungs, and putrid exudation.

The second class of cases consisted of septic inflammation of the umbilical vessels, with its sequelæ. Three cases of arteritis umbilicalis were observed, one of phlebitis, and four of more or less diffuse perotonitis, with discoloured exudation matter. In these cases there were also morbid appearances in the skin—viz., erythema, which in one case extended over the whole surface of the body with a rapidly fatal result, and erysipelas migrans, with strong induration of the affected parts, and which appeared several times in the face.

A third class of cases was characterised by atrophy. This form of the disease was observed in four cases, the subjects being originally strong children, and no cause being apparent but the miasma.

Regarding etiology, it appeared that the stay of the pregnant women in the Lying-in Hospital disposed them to puerperal fever. The majority of patients fell ill within the first week of their stay in the institution. Of those who entered it during parturition, 13·8 per cent. fell ill, while of those who had been in it from one to four days before delivery, 29 per cent. became affected. If, however, the pregnant women remained longer in the hospital, they appeared to become callous to the contagion. In the wards for poor patients the disease was much more frequent than in those for private cases. It would seem possible that the clinical instruction of the students in the examination of pregnant and parturient women might be the cause of the affection, and M. Semmelweiss even now holds the opinion that the origin of the disease is entirely due to infection with cadaveric poison. But several objections seem to be fatal to this supposition. During the epidemic I am speaking of there was no occasion for such an infection of the pregnant women taking place. The autopsies were never made in the Lying-in Institution itself, but in the anatomical theatre; and neither the students, nor those who had the women under their care, ever touched the dead bodies. Besides, the hands were always well washed before an examination was made. Moreover, it happened that infants fell ill and died of puerperal sepsis, whose mothers made a good recovery. This fact is not consonant with the assumption of an infection by cadaveric poison. It would be sophistry to say that, although the mother seemed to be well, she was nevertheless infected; for where there is no morbid symptom, we are not justified in assuming the existence of a complaint. Another objection against the theory of M. Semmelweiss would be, that the women fell ill in sets, and that sometimes not a single case of fever occurred during a fortnight. It has often been stated that there was much more disposition to contract the fever in women who had had a protracted labour; and that primiparæ were, therefore, more exposed to danger than such as had already borne children before. The observations made here on this point during the last epidemic prove many fell ill whose labour had been very quick, and that the duration of the labour had no relation to subsequent illness. I will also mention that, of 80 cases in which fever occurred, birth was accomplished by nature in 75 cases, while only in five instances operation was necessary.

The researches of Professor Buhl concerning the pathological anatomy of this disease are of importance. Proceeding from the fact that the inner surface of the uterus is always affected, he looks upon this as the starting-point of all other morbid changes. The disease spreads from the place mentioned along three different roads—viz., the mucous membrane of the tubes, the bloodvessels, and the lymphatic vessels. In conformity with this circumstance, there are three principal forms of puerperal fever. The first is puer-

peral peritonitis without pyæmia; the irritant matter produced in the uterus spreads through the canal of one or both tubes, and through their orifice into the peritoneal cavity; their tissue becomes swollen, loosened, and injected, and the canal is filled with thick pus. There is either unilateral inflammation of the serous covering of the ovary and the womb, or general peritonitis. This form is most frequent if there be no epidemic; but during such an one it is very rare, and only occurs in connexion with one of the other forms. Not a single case of this kind occurred during the epidemic I have described. This form is not nearly so dangerous as the two others, recovery being not uncommon; and if death results, this is generally the consequence of general peritonitis.

In the second form, which is puerperal pyæmia without peritonitis, the inner surface of the womb constitutes, as it were, one large wound, and the affection is traumatic. There are symptoms of absorption of putrid matter by the veins, and we find not only normal coagula, but also thrombi containing pus, and prone to putrefaction, especially in the veins at the insertion of the placenta; also in the veins of the plexus pampiniformis, the spermatic veins, the vena cava inferior, the renal or crural veins and the cavities of the heart. There is often embolic inflammation, followed by suppuration and gangrene of the lungs, the spleen (which is always enlarged), the kidneys, the eyes, the brain, and other organs. The characteristic feature of this form is that, unless a complication should be present, which is rarely the case, there is no peritonitis. It is more dangerous than simple peritonitis, and may occur sporadically; but it is more frequently epidemic, although neither so common nor so intense as the third form. It is not unfrequently an indication that the violence of the epidemic is on the decrease. For while pyæmia without peritonitis scarcely ever ends in death before the ninth day, but is generally protracted over two, three, and more weeks, the third form generally proves fatal within the first week after parturition, and in some cases even the very day after delivery, or a few hours after it. This second form was, in the epidemic I have described above, only observed four times.

The third form is entirely different from the second and the first. There is always peritonitis, but no thrombosis of the veins, which are merely filled with thin, discoloured blood. The disease spreads by absorption through the lymphatic vessels and the connective tissue which envelopes them and the bloodvessels, and where yellow coagula, pus and putrid matter, are accumulated. Besides this local reeping of the disease, there is also general infection by absorption of poisonous matter in the blood, either immediately from the womb or from the infected lymph. This form may, therefore, be called puerperal pyæmia with peritonitis, or pyæmia with lymphangitis. It is the most malignant form regarding intensity, rapidity of course, and fatality, and it is also the most frequent in epidemics of puerperal fever. Of thirty-nine fatal cases in the last epidemic, thirty-five belonged to this form. If death ensued very rapidly, there were only general septic appearances in the body; and local changes only became apparent if the disease was of a somewhat longer duration.—*Medical Times and Gazette, Feb.* 8, 1862, *p.* 142.

119.—ON PUERPERAL FEVER.

By Dr. Fox and Dr. Tyler Smith.

[The following remarks were elicited at a meeting of the Obstetrical Society of London, by a paper read by Dr. Tilbury Fox. Speaking of the clinical history of the General Lying-in Hospital, Dr. Fox observed:]

In tracing the connexion between erysipelas and puerperal fever, the different epidemics formed links in the chain of gradation and identity; that all the symptoms of intense puerperal fever were produced in cases in which the most decided evidence of erysipelas alone existed, and therefore the assumption of a special peculiar disease *sui generis* (puerperal fever) was unnecessary; that in the case of primiparæ, lacerations to an appreciable ex-

tent being the rule, great facility of ingress and onset, so far as the poison of erysipelas is concerned, is their chief source of liability to attack, which latter appears statistically to be true.

Dr. TYLER SMITH said the subject of puerperal fever was the most important which could occupy the attention of the society. The whole obstetric mortality of England and Wales exceeded 3,000 annually. Of this number of deaths more than 1,000 women, or nearly three daily, fell victims to puerperal fever; and it was the healthy and vigorous primipara whom it was most prone to attack. The obstetrist could put before him no nobler object than the diminution of this mortality. Unhappily, we could not look to treatment to accomplish this. Under various circumstances, and in different countries, every variety of treatment had been tried and found wanting. If not curable, it was however preventable. It was not, therefore, to treatment, but to prevention, that we must look for the means of dealing with it successfully. If epidemics of puerperal fever were less rife now than in former times,—and at present they rarely occurred, except from the crowding of women in lying-in hospitals,—it was because we lived under better sanitary conditions, and paid more special attention to preventive measures. We should surround every lying-in woman, as far as possible, with antisepetic precautions. Nothing, he believed, would tend more to diminish the frequency of puerperal fever than the full recognition of its infectious and contagious nature, in whatever way it first occurred. It would not so often happen if accoucheurs recognised the fact that erysipelas, typhus, scarlatina, small-pox, hospital gangrene, putrid sore-throat, diphtheria, the post-mortem and other poisons were excessively prone, if brought near the lying-in woman, to originate puerperal disease. He did not question but that any of the agents which produced zymotic maladies might cause puerperal fever, or that it might arise in individual cases from the retention and putrefaction of portions of placenta or membrane or coagula, or the decomposition of fibrinous clots in the uterine vessels, especially in women who were predisposed by hemorrhage, albuminuria, or other causes of debility; but contagion and infection, which might to a great extent be recognised and avoided, were its chief and most preventable sources. If all our means, in the way of prevention, were habitually brought into operation, he did not doubt that puerperal fever, instead of being the highest, might become a very moderate cause of obstetric mortality.—*Lancet, Nov.* 23, 1861, p. 501.

120.—THE TREATMENT OF CHILD-BED FEVER.

In an epidemic of child-bed fever, which occurred some time ago in the obstetrical clinique of Professor Von Ritgen, the following plan of treatment was adopted, with exceedingly beneficial results, as even cases of the utmost severity were cured under its influence. At first one-eighth of a grain of morphia was given, and this dose repeated two, three, or even four times a day, according to the violence of the abdominal pain. An hour after the dose of morphia, a mixture of camphor was administered (℞. Camphor ʒss., gummi mimos ʒj., aq. chamomill ℥iij., liq. ammon. acet., sacch. albi, āā, ℥j.); an hour after this the patient took one grain of quinine; then another dose of morphia, and so on, until the symptoms decreased, which was the case with all patients hitherto treated in this manner. In one, two, or five days, profuse perspiration came on, and the secretion of the urine, the lochia, and the milk was considerably increased; together with this a great improvement took place in the general condition of the patients. If the stomach was so irritable that it did not keep the medicines, nitrate of silver was first given, in order to diminish the gastric irritability; this had the best effect, as after it the medicines were well borne.

With regard to prophylaxis, I may mention, that quinine, even in large doses, (of twenty to thirty grains per diem before delivery,) did not prevent the occurrence of the fever, nor did it diminish the intensity of it when pres-

ent. On the other hand, fumigations with aqua regia, which have been recommended by Mr. Wilson, proved to be of the greatest use, as after them no other case of the endemic occurred.—*Med. Times and Gazette. Nov.* 30, 1861, *p.* 565.

121.—ON THE PATHOLOGICAL ANATOMY OF PUERPERAL FEVER.

By Professor Buhl, Munich.

Professor Buhl having examined the bodies of fifty women who died of puerperal fever, states that a constant and characteristic appearance is a pappy, red or dark brown or greyish-black mass lining the inner wall of the uterus, giving forth sometimes a gangrenous and sometimes a putrefactive smell. It is this matter which supplies the poisonous infection of puerperal fever; but as to the cause of the production of the fever differences of opinion prevail; some regarding it as the consequence of the immediate passage of poisonous matter into the wound, while others think that a preliminary poisoning of the blood by miasmata takes place, the corrupted mass being only a secondary result. Anatomically, we may distinguish two forms of puerperal fever—puerperal pyæmia and puerperal peritonitis—forms which may be clinically distinguished, as it is of importance in prognosis that they should be so.

Puerperal pyæmia does not usually prove fatal before the ninth day, and frequently not until after the third week. It is chiefly met with where the disease does not put on an epidemic form, the veins being the channel of infection; coagula, accompanied by suppuration, being found in the veins of the walls of the uterus, in a pampiniform plexus or in a spermatic vein. In no instance did the author ever find both spermatic veins obstructed, and in only one case was the entire vena cava inferior filled with adherent coagula. These coagula and their subsequent caseous metamorphosis are quite sufficient to establish the existence of puerperal pyæmia, the so-called metastatic abscesses being seldom met with. Œdema of the lungs and ecchymosis of the pleura were frequently met with by the author.

The puerperal peritonitis was more frequent, more violent, and more rapidly fatal than the puerperal pyæmia, inasmuch as death sometimes occurred within two days after delivery, and in but few cases was delayed to the third week. Of the 32 cases of this variety only 2 were chronic, proving fatal in the course of six or eight weeks. In all the cases purulent exudation was found, in 18 instances occupying the tubes, and in 14 the subserous tissue of the uterus. The two conditions were found combined in only 4 instances, and a plugged condition of the veins was observed only in 5 instances. Of the 18 instances in which puerperal pyæmia occurred, in only 2 was there pus in the tubes, and in only 1 subserous effusion of pus; so that of 20 cases of tubal suppuration, in 18 peritonitis was present, and of the 14 cases of subserous suppuration peritonitis occurred in 13. On the other hand, of 23 cases of purulent coagula of the veins in only 5 did peritonitis occur, and in all these there was subserous or tubal suppuration also, and in 16 cases in which these parts exhibited no pus, no peritonitis took place. The disease of the veins thus bore no relation to the occurrence of peritonitis. It results from these facts, that peritonitis may arise either from the immediate passage of the poisonous material from the uterus through the tubes, or from the conveyance of this from the inner wall of the uterus by the lymphatics. The supposition that the pus may have proceeded from the peritoneum into the tubes is negatived by the fact of these having been free of it in fourteen cases; and the pus of the periuterine, subserous tissue, or of the lympathic vessels, must be regarded rather as a consequence than a cause of the peritonitis, inasmuch as it was absent here in twenty instances. The prognosis is not alike in these two modes of origin of the peritonitis. That induced by pus from the tubes is a much slighter and more simple inflammatory process, met with when there is little or no epidemic extension of the disease; while

the peritonitis resulting from lymphatic absorption is a much severer form of disease, proceeding or accompanying general infection, and is especially met with in the epidemic form.

In both of the principal forms of puerperal fever, besides the morbid uterine appearances there were found—1. Almost constantly, swelling and watery infiltration of the retroperitoneal, inguinal, and (though seldomer) the mesenteric glands. 2. Osteophytes on the internal surface of the cranium. 3. In several cases, especially in pyæmia and lymphatic absorption, a distension of the cortical substance of the kidney, together with microscopical appearances corresponding to the acute stage of Bright's disease. In only two of fifty individuals was tuberculosis found.—*Med. Times and Gazette, Feb.* 1, 1962, *p.* 117.

122.—ON PAINFUL MUSCULAR AND FASCIAL CONTRACTIONS ALONG THE VAGINAL CANAL.

By Dr. J. Y. Simpson, Professor of Medicine and Midwifery in the University of Edinburgh.

Professor Simpson said that he had lately seen a number of cases, and he had seen them from time to time for years, where painful muscular or fascial contractile bands existed in the sides or along the course of the vagina. He had known some of these cases to have been mistaken and treated for various alleged affections of the uterus or its appendages. The pains complained of were sometimes principally sympathetic or reflex, and referred to the uterus or other parts, and often aggravated by all movements calling the pelvic muscles into action. He had under his care at present a patient whose chief complaint was a constant disagreeable pain in the sacral region; another who had the same severe kind of pain in the left iliac region; while a third could not walk because of the pain which she felt in the pelvis whenever progression was attempted. In this class of cases the uterus and ovaries would, on careful examination, be found healthy, but a tense, corded transverse band could be felt at some part of the vaginal wall, and usually, if not always on one side of it, and placed, as it were, more or less deeply beneath or below the mucous membrane. The band or cord was most commonly placed about an inch above the vaginal orifice. It varied considerably both in thickness and tenseness in different cases. When the cord was touched and stretched with the finger, the patient complained of more or less severe suffering; and this was the pathognomonic mark of the disease. Sometimes the patient only experienced pain at all when the vagina was touched; and these cases usually came under treatment in consequence of being unable to submit to marital intercourse. He had one patient under treatment who could not bear at first to allow herself to be examined vaginally without chloroform, because of the pain experienced from the touch of the finger. Painful and distressing as these cases were, they were very amenable to treatment,—division or rupture of the tight and contracted band being usually sufficient to afford complete and often instantaneous relief; and in the milder form of cases, sedative applications were sometimes sufficient. The method he had usually adopted for the cure of very severe cases of this kind was, after chloroforming the patient, to divide the tight band by means of a tenotomy-knife introduced underneath the vaginal mucous membrane. It was a bloodless operation, and had never been attended with any worse consequences than the formation of a thrombus, which had taken place in one patient and had delayed her recovery. He had tried also to effect the object of stretching or rupturing the band by dilating the vagina forcibly with the fingers, whilst the patient was asleep with chloroform. The principle of cure was the same as that employed for the relief of fissure and spasmodic contraction of the orifice of the rectum. But less severe means were occasionally successful. Patients afflicted with this complaint were usually relieved, and sometimes cured, by the daily introduction into the vagina for a length of time, of local sedatives, such as belladonna ointment and chloroform. A small cup-like indentation was made with the finger in an ordinary belladonna pessary; a

few drops of chloroform having been poured in, and then shut in by putting a piece of ointment over the orifice, and then the whole was introduced into the vagina, where the ointment slowly dissolved, and became absorbed along with the chloroform. As to the probable nature of these painful contractions, he (Dr. Simpson) could not supply any very definite answer, but he thought they depended in different cases, either, first, on a kind of permanent spasm of some of the muscular fibres around the vagina, of the same nature as the spasm of the sterno-cleido-mastoid muscle, which produces torticollis; or, secondly, they were due to contractions going on slowly in some portions of the pelvic fascia, perhaps resulting from a kind of subacute inflammation, and resembling those often painful contractions of the palmar fascia, which are the acknowledged cause of "crooked-in-fingers." Dr. S. believed that the common anatomical seats of these painful vaginal contractions were either in the bundle of muscular fibres forming the anterior border of the levator ani, or in the duplicatures or edges of the pelvic or recto-vesical fascia at the points where the vaginal canal perforates the fascia and receives insertions and prolongations from it. These contractions sometimes appeared in patients in whom no previous disorder of any of the pelvic organs could be ascertained to have existed; and he had lately seen one patient who was the subject of it, and who had never been able to allow her husband to approach her, so that in her the morbid condition must have been present before marriage, although she had never been in a position to be made aware of its existence. Instances, however, like this last oftener belonged to a class of cases where apparently the stricture was not, as in the preceding class, in the course of the vaginal canal, but was situated at its very orifice, independently apparently, in most, of all disease there except supersensibility and spasm of the sphincter of the vagina, but traceable in others to hyperæsthesia of the mucous surfaces of the vulva or vagina, resulting from irritable eruptions or other morbid states of these mucous surfaces.—*Edinburgh Medical Journal, Dec.* 1861, *p.* 594.

123.—NEW INJECTING APPARATUS.

By Dr. J. Y. Simpson, F.R.S.E., Edinburgh.

Dr. Simpson showed how a tube, such as is used for the introduction of carbonic acid gas into the vagina, might be modified so as to serve along with an ordinary beer bottle, instead of a syringe, for applying injections and douches to the rectum and vagina. Dr. Scott of Musselburgh had been in the habit of employing the bottle and tube for some time instead of an enema syringe, and for the purpose of introducing food into the stomachs of insane patients who refused to swallow. A bottle filled with the required fluid, and stopped with the cork through which the tube passes, having been raised and turned upside down, when the cork was partially withdrawn some air rushed into the bottle and sent out its fluid contents through the tube. He (Dr. Simpson) had got a cork made with a small secondary tube, which served to admit the air into the bottle, so that the partial withdrawal of the cork could be dispensed with, and whilst the bottle was held up with one hand, the other was left free to hold and direct the nozzle of the tube. The stream that issued from the tube might be made to run with a greater or less degree of force according to the height to which the bottle was elevated. Another simple form of injecting apparatus, on the same principle, consisted of the usual tin canister with which some enema apparatus were provided, perforated at its lowest edge, and having a long caoutchouc tube attached to this perforation at one end, and terminating at the other extremity in the usual form of enema nozzle. This apparatus acted well, and was cheap. He had used it for injecting both the rectum and vagina. When filling the apparatus the nozzle of the instrument must of course be held higher than the canister; and when using it after it is filled, and the nozzle inserted, then the canister is merely raised higher than the tube, and the liquid flows

into the rectum or vagina without the jerking or *per saltum* motion of the common enema pump. A sufficient amount of elevation of the canister gave the fluid all the force that was necessary to send it into the rectum by mere gravitation. Valves in enema apparatus were expensive, and always going wrong. In this apparatus there were no valves.—*Edinburgh Medical Journal, Dec.* 1861, *p.* 595.

124.—REMOVAL OF A FIBROUS TUMOUR FROM WITHIN THE ABDOMEN.

By I. Baker Brown, Esq., Senior Surgeon to the London Surgical Home.

[The patient was 34 years of age. She had had a tumour on the right side of the abdomen for seven years. This had been diagnosed as a fibrous tumour connected with the ovary so long ago as three years.]

On examination, Mr. Brown diagnosed a large fibrous tumour of the right ovary, perfectly moveable and devoid of adhesions. Her general health appearing good, Mr. Brown, on consultation with his colleagues, thought this was a case in which the whole mass might be removed by abdominal section. He, however, pointed out to the patient the great risk incurred, and advised her to consult with her friends, telling her that the tumour was not of a character that would rapidly kill her. Not until after several postponements, several consultations, and the patient's earnest wish to have the operation performed was the matter decided. Mr. Brown gave as his reason for operating:—1st. The perfect mobility of the tumour, showing it was devoid of adhesions. 2nd. That, although one could not be certain, still, from the fact that the tumour began on one side, he suspected that it was ovarian. 3rd. The rapid growth of the tumour. And, lastly, the patient's earnest desire to have it removed, she saying that, from its great size and weight, it was very inconvenient, and life was a burden to her in her present condition. To prepare her health she was ordered—℞. Tr. arnicæ, tr. ferri mur. ää ℥v., ter die sumend. To go out frequently in the open air, and to have warm baths twice a week, with generous diet, including porter.

June 12th. Getting stouter and stronger. Ordered—℞. Hyd. bichl. gr. 1-24, tr. cinchonæ ʒi., aquæ ad ℥i. M. ft. haust. ter die sd.

June 21st. The patient being placed under the influence of chloroform, Mr. Brown prepared to operate. Before doing so, however, he stated that, although a tumour of the right ovary had been diagnosed, nothing certain could be known till an incision had been made. His first step would, therefore, be exploratory. Having made the median abdominal incision, the tumour was exposed; in doing which, there was a little hemorrhage from the latter being slightly cut. To the surprise of every one, when the tumour was examined, it was found not, as had been imagined, to implicate either ovary, but to grow from within the walls of the uterus. After consultation with Dr. Hall Davis, Mr. Harper, and other gentlemen present, it was resolved to proceed. Mr. Brown, therefore, secured the broad ligament and fallopian tube of each side by the callipers; then passing two double ligatures, through the tumour, as low down in the uterus as was judged would embrace its full extent, he firmly tied each half. All above the ligatures was cut off. A small fibrous growth outside the walls of that part of the uterus which was left was also tied. The stump of the uterus and the callipers were kept outside. Ten iron wire sutures were inserted, to close the edge of the wound, a bandage put on, and the patient placed in bed. During the operation, æther was substituted for chloroform, as the latter did not seem to agree with her.

On examination of the tumour, it was found to be a large, so-called fibrous one, growing originally within the walls of the fundus of the uterus, the coats of which it had pushed up with it in its own growth until at last they had burst. The tumour had grown considerably above this boundary, and its upper third was covered only by its own capsule. The connection of the tumour with the uterine walls was very intimate, and no doubt this tumour

was one of those so ably described by Dr. Paul Broca, of Paris, as hysteromas (*vide* "Costello's Cyclopædia of Practical Surgery," vol iv., art. "Tumours," p. 469), which he distinguishes from fibrous as a tumour in which both the accidental tissue and the normal tissue of the uterus are united. He says: "These tumours are not purely fibrous, inasmuch as they contain elements analogous to those that form the muscular substance of the uterus, viz., the fibre cells of Kölliker. Their tissue, formed of a mixture of fibrous tissue and fibre cells, is, therefore, but a repetition of the structure of uterine tissue. Hitherto they have been met with only in the uterus itself, or in the adjacent organs."

On section, it was found that nearly one-third of the uterus had been taken away. There was also found two smaller tumours within that portion of the uterus which had been removed. Weight of the tumour, 7lbs. 5 ounces.

[She progressed favourably, but ultimately sank, and died eighteen days after the operation.]

Post-mortem examination.—July 9th, 8 p. m., twenty-seven hours after death.—The body generally very much emaciated. On examining the abdomen it was found that the pedicle had retracted within the abdominal walls.

The upper three-fourths of the wound had firmly healed; the edges of the lower part had separated about three inches, and to their inner surface the pedicle was firmly adherent. On opening the abdomen, the lower part of the intestines were found glued together, and to the pelvic fascia by very firm adhesions, evidently the result of peritonitis, though there were no signs of inflammation having taken place in the last few days. The adhesions between the bowels and the pelvic fascia were so firm as to require a knife to separate them. There was a large quantity of pus in the iliac vein, &c., the coats of which were thickened and contracted. There was rather more than one-third of the *uterus* left, and, on cutting through it, the vessels were found filled with pus.

The *heart* and *lungs* were sound, but paler than usual. The *liver*, *spleen*, and *kidneys* were also normal, but rather pale.

There was no pus in the *liver*, nor in any part of the body, except those above mentioned.

Remarks.—It has become a serious question in my mind since the termination of this case, whether it would not have been better to disregard the inconvenience and sufferings of the patient, of which she complained, and have allowed the tumour to go steadily growing, than to have attempted its removal. Even when found by the exploratory incision not to be a fibrous tumour of the ovary, as was so generally suspected, whether it would not have been better to close up the wound without interfering with it. In several cases which I have since seen, very much resembling this one, I have decided not to interfere by operative measures, and it appears to me that the practical fact to be gained from it is the clear elucidation of this peculiar form of growth as contra-distinguished from those of the polypoid and fibroid tumours within the uterus.—*London Medical Review, Jan.* 1862, *p.* 320.

125.—ON CAULIFLOWER EXCRESCENCE OF THE OS UTERI.

By Dr. J. Braxton Hicks, Assistant Obstetric Physician to Guy's Hospital.

[In all cases in which removal of the cervix, or such portions of the growth as are removable is effected, considerable benefit is at once visible.]

To drag the uterus down within range of the knife, is in many cases impossible, to say nothing about the risks of hemorrhage; to continue the ligature around the cervix for many days is a very severe, painful, and offensive operation; and both have been, doubtless, avoided as much as possible for this reason. Any plan, therefore, which will not require the uterus to be dragged down, which can be completed at once, and free from chance of

hemorrhage, which is liable to occur after knife or scissors, at the same time without any increase of danger to the patient, must commend itself to all. The chain-écraseur fulfils the latter conditions, but not the first; for it will not act otherwise than in a straight line with the shaft. In some cases the cervix can be brought within its scope, and in these it will be very safe and useful; but in those where it is impossible to bring down the uterus, it is useless.

To combat this difficulty, after many experiments of various kinds of material, I have found a rope made of annealed steel wire to approach nearly to the strength of the chain. The rope should be made of four or five smaller ropes, each composed of four wires of the size used for sutures. They should be twisted very loosely, but carefully; if too tightly, they are more likely to break. I have had a modification of the shaft of the screw-écraseur adapted to this rope, one much lighter and neater than those in ordinary use, which is needlessly heavy for all demands of obstetric surgery. If the hand cannot pass the rope round the tumour, I use two tubes like those of Gooch's double canula, from the end of each of which a loop of twine protrudes, the ends of which appear at the other end of the tube, to which they can be fastened. The wire rope is passed through these, and both tubes are brought together in the centre of the rope, and introduced into the vagina. When they have reached as far up as desirable, the tubes are separated and carried round the tumour, like the ordinary double canula. When they have met at the opposite side, both ends of the rope are passed through the eye of the écraseur, which is pushed up as far as the level of the encircling rope; when this is accomplished, the latter is properly fastened. To release the loops of twine, untie at the base of each tube, and pull carefully by one end of the twine, keeping the tubes steady the while; the tubes are now to be withdrawn from the vagina. The screw of the instrument is now to be carefully worked, as in the chain kind.

This method is peculiarly applicable to *intra*-uterine polypi, from the flexibility of the wire. When long lengths of wire are required, the screw can be made to wind the rope on a drum. For small polypi, a wire rope of six threads will be found sufficient for every purpose. It is requisite that great care be used in the construction of the instrument, in order that the wire should not bear on any angle or edge; every part on which the strain rests should be smoothly rounded off. By means of such an instrument, the cervix can be cut through without dragging the uterus down.

To pronounce, therefore, the removal of the cervix under the condition above shown either a hazardous or a useless operation, as its removal was formerly designated, is wide of the true state of the case. We are indebted to chloroform for altering the whole aspect of operations, and those opinions expressed prior to its introduction can scarcely be quoted at the present time. No doubt, without its assistance, the pain in many operations might cause the operator to pause before he inflicted it without commensurate benefit; but pain being removed, the question is simply confined to the risk and the advantage.

It is under these circumstances that the excision of the cervix uteri, in this form of malignant disease, by the wire rope or chain écraseur, is a comparatively easy and safe operation, and I would recommend in every case possible that this treatment be adopted, removing the cervix as high up as possible, to afford a better chance for complete extinction of this miserable disease.

The second point to which I would call attention is the use of styptics. The vitality of this growth is feeble; its cells are so delicate that their contents can easily be coagulable through the walls. Thus, a certain portion can always be killed by the application of tannic acid in powder. I have found that a saturated solution of alum, holding in suspension tannic acid, in the proportion of two drachms to the half-pint of saturated solution, will, in the very soft forms, if applied every day, reduce gradually the more tender parts of the tumour; but it has less effect on those parts which approach healthy tissue in character; consequently, the nearer the lotion approaches the deeper structures, the less effect it will produce on the growth. Not-

withstanding this, the effect of a styptic is always beneficial, because it checks both the sanious and serous discharges, and limits thereby the drain on the system. Sometimes a lotion of the above strength cannot be borne; in that case it answers very well if mixed with equal parts of water. I prefer tannic acid over other forms of tannin, inasmuch as we know the exact amount of tannin used, and, being insoluble, it remains behind on the growth and in the vagina, whereby its effects are prolonged. With regard to the other points of treatment, I need not enter upon them here, they being already well recognised, except to dwell upon the advantage the majority of cases experience by the use of iron and quina, either or both combined. The sustention of the system by these and other tonics, and by restraining the discharges, are almost invariably attended by marked beneficial results, I may say, in almost every case of malignant disease, and in the class under consideration particularly so.—*Guy's Hospital Reports, Vol.* vii. *p.* 251.

126.—ON CAUTERISATION BY ELECTRIC HEAT IN THE TREATMENT OF CERTAIN DISEASES OF WOMEN.

By Robert Ellis, Esq.

Mr. Ellis, in a paper read before the Obstetrical Society of London, commenced by defining the action and effects of cauterisation by heat, and then described the instruments invented by him for producing this effect by the electric current. The cauteriser originally introduced by him in 1852 was of very simple construction. It consisted of a straight tube of silver, ten inches in length, containing a piece of stout copper wire clothed with an insulating substance. These formed the two poles of the apparatus, and metallic connexion was made between them at the upper end by a fine platinum wire, while at the lower they were in communication with the battery by two wires secured by a clip. Under the platinum wire was introduced a piece of porcelain, which acted as the cauteriser, being made white-hot on the completion of the galvanic circuit. Up to the date of the introduction of this instrument only the heated wire had been used, and the author was the first to collect the heat by this means, and thus adapt it for practical use. For all cases in which a considerable surface required to be treated, this instrument was the most suitable. But for a great majority of the cases coming under notice, the following was a more convenient form.

The cauteriser consisted of the ordinary handle used by dentists for destroying the pulp of carious teeth, with the button for making and breaking the galvanic connexion. But at its upper end two stout wires, of about seven inches in length, were attached, by being soldered into two silver sockets, which fitted in the ends of the wires issuing from the handle. By this means a tool twelve inches long was formed, capable of reaching the os uteri if necessary. The cauterising end was formed of a piece of platinum wire, shaped like an inverted V.

Hitherto, the great obstacle to the success of the electric cautery had been the great size, the cost, and the complicated form of the battery required. In the battery exhibited by Mr. Ellis, which was little larger than a glass tumbler, one cell was sufficient, and was sometimes so active as to melt the platinum wire. This battery consisted of a cylindrical glass jar, with a wide mouth fitted with a cap of gutta-percha. Within was a cylindrical roll of amalgamated sheet zinc, a quarter of an inch thick, six inches deep, and about seven in circumference. Enclosed by it was a porous earthenware cylinder containing a roll of platinum coil immersed in strong nitric acid. As in all Grove's batteries, the outer cylinder contained dilute sulphuric acid. The peculiarity of the author's battery and the principal secret of its energy lay in the size and form of the platinum roll. Seen in section it presented a stellate arrangement. It was formed of a sheet of platinum folded upon itself, somewhat like a frill, and thus presented a very large negative surface.

The only expensive part of the instrument was the platinum, the value of which was about fifteen shillings. The rest of the instrument cost about two shillings and sixpence. By two stout copper wires (No. 15), made soft by heating to redness, and then plunging into cold water, the electric current was conveyed to the cauteriser, and the platinum wire was instantly heated to a glowing whiteness.

The diseases to the treatment of which the author considered this instrument likely to prove a valuable aid, were: obstinate ulcerations and congestions of the cervix uteri, resisting all other treatment for a long time; vascular growths from the os and canal of the cervix, which often give rise to tremendous hemorrhages; also especially the vascular tumours or caruncles of the female urethra; warty excrescences; and a peculiarly obstinate form of patchy inflammation, apparently of scattered glands on the mucous surface adjoining the meatus, and which frequently give rise to intolerable pain and pruritus. It may also be useful in closing the minute holes left after the operation for recto and vesico-vaginal fistulæ.

The author selected, as illustrations merely, three cases as typical of the benefit obtained from the use of the electric cautery:—

The first was a case of ulceration and hypertrophy of the cervix uteri. The patient, aged 37, married, sallow and unhealthy-looking, had suffered more than six miscarriages in succession, and was in a state of great exhaustion. The general symptoms and cachexia were of the well-known type. The cervix uteri was greatly enlarged, livid, and sanious. After some preliminary treatment, the electric cautery was applied on two occasions with very little pain. In a short time the whole aspect of the diseased surface was altered, and the patient recovered with little, if any, further interference.

The next case was one of vascular caruncle of the meatus—a disorder more than commonly rebellious to ordinary treatment. A lady in the country had suffered for many years with one of these growths, the extreme annoyance and distress of which at length induced her, after a fruitless attempt to obtain relief from the application of lunar caustic, by the advice of a medical practitioner, to apply to the author. The growth was cut through the base by a ligature drawn very tightly, and soon afterwards it was firmly touched with the cautery. The patient returned home well, and had continued now for some years without any sign of her old disorder returning.

The last case was one of intense pruritus of the external genitals, connected with the inflamed patches already alluded to. These patches were almost incurable by any ordinary treatment, and the disease had existed for fifteen years. The cautery was used to each of these spots in succession, and with the happiest effects—the disease gradually disappearing, and the mucous surface regaining its original healthy aspect.

In closing his paper, the author remarked that it could not be too much insisted upon that there was a peculiar benefit arising out of cauterisation by heat, which placed it in a different category from all other agencies of this kind, and which especially adapted it for obstetric use—and that was, the contractility following its use, and the remarkable modifications of vitality which it imposed on the structures treated by its means.

Mr. Ellis exhibited to the Society the battery and the cauteriser; the latter was at pleasure raised to a white-heat so intense as to kindle gas, and it could be passed through several folds of paper without encountering the least resistance.—*British Med. Journal, Jan.* 4, 1862, *p.* 20.

127.—ON ULCERS OF THE OS AND CERVIX UTERI.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

The following table may prove useful, and exhibits the varieties of the simple ulcers of the cervix uteri in their order of frequency, with their diagnostic characters and outline of appropriate treatment:—

Variety.	Characters.	Treatment.
1. *Indolent Ulcer.*	Cervix hypertrophied, of a pale pink, and hard. Os patulous to a small extent. Ulcer of a rose red. Granulations large, flat, insensitive, and edge of the ulcer sharply defined. Discharge: Mucus, with a little pus, and occasionally a drop of blood.	For a few times the caustic pencil. Afterwards, several applications of solution of nitrate of silver in strongest nitric acid.
2. *Inflamed Ulcer.*	Cervix tender, hard, a little hypertrophied, hot, and red. Vagina hot and tender. Ulcer of a vivid red. Granulations small and bleeding. A livid red border round the ulcer. Discharge: A muco-pus, yellow and viscid, with frequently a drop of bright blood entangled in it.	Occasional leeching; hip-baths (warm); emollient injections. Then acid nitrate of mercury several times, succeeded by the solid lunar caustic, potassa fusa or cum calce.
3. *Fungous Ulcer.*	Cervix soft, large, spongy to the touch. Os wide open, so as to admit the finger. Ulcer large, pale, studded with large and friable granulations. Discharge: A glairy, brownish mucus, frequently deep-tinged with blood.	At first the caustic pencil. Subsequently, nitric acid solution of nitrate of silver, or acid nitrate of mercury; electric or actual cautery.
4. *Senile Ulcer.*	Cervix small, red, a little hard. Ulcer small, extremely sensitive, of a bright red colour. Granulations very small, red, and irritable. Discharge: A thin muco-pus.	Potassa fusa, or strong nitric acid with nitrate of silver, once or twice, at *long intervals*. Then solid sulphate of copper in a pencil.
5. *Diphtheritic Ulcer.*	Cervix of ordinary size; a little hot, dry, and tender. Ulcer covered in patches with a white membrane, adhering closely; irritable, and readily bleeding beneath. Discharge: A thin, acrid mucus, without pus, but occasionally tinged with blood.	At first, electric cautery, potassa cum calce, or acid nitrate of mercury, two or three times, at long intervals. *No nitrate of silver.* Subsequently, stimulant applications—tincture of iodine or sulphate of copper.

I may be allowed to express my conviction that the more carefully these varieties of the uterine ulcer are studied, the greater will be found the practical value of their diagnosis, and the fewer the instances of failure in their treatment.—*Lancet, April* 12, 1862, *p.* 379.

128.—ON THE TREATMENT OF LARGE OVARIAN CYSTS AND TUMOURS.

By T. Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

[Ovariotomy is a dangerous operation certainly, but its results are quite as favourable as those of many other capital operations. In successful cases the patient is restored to perfect health; and if only one ovary has been removed she may bear children afterwards.]

When ovariotomy is decided upon, I believe observance of the following rules will assist in securing a favourable result:—

1. The operation should be performed in an airy room, the temperature of which should not be below 70° Fahr., but need not be raised to a degree uncomfortable to either patient or surgeon.

2. The patient should be placed in the horizontal position, on a firm, narrow couch, opposite a good light, and be well covered with flannel everywhere except the abdomen and face.

3. She should be spared the fear and shock of the operation, by chloroform.

4. The incision should be made in the median line. It should not pass nearer than two inches to the symphysis pubis, and may commence just below the umbilicus. It may be extended upwards to the left of the umbilicus, and along the median line, as far as may be necessary to expose the tumour and separate adhesions. A large empty cyst may be removed through an incision three inches long; and an incision of five inches is amply sufficient for the removal of very large tumours, provided each cyst be with-

drawn as it is emptied, or groups of small cysts be withdrawn unemptied one after another.

5. Any bleeding vessel in the abdominal wall should be secured before the peritoneum is divided.

6. When ascitic fluid surrounds an ovarian tumour, some of it may be allowed to escape; but the tumour should be pressed forward by an assistant to retain the fluid till the last as a protection to the intestines. Any adhesions found, whether there be ascitic fluid or not, between the cyst and abdominal wall, should be carefully separated by the hand whilst the cyst is full, great care being taken not to rupture any cyst. Adhesions to omentum or intestines are better left until the cyst is empty and the adherent viscera can be seen.

7. As soon as the tumour is freed from parietal adhesions, it should be tapped by a large syphon trochar. As the fluid escapes and the cyst becomes flaccid, it is either to be fixed by a hook and withdrawn, or drawn over the canula and tied over it, to prevent any ovarian fluid from entering the peritoneal cavity.

8. As the cyst escapes, flannels wrung out of water at 96° are to be carefully wrapped round it to protect the peritoneal cavity. As secondary cysts are drawn to the opening, they are to be emptied, either by the trochar being passed on through the canula which is still tied within the first empty cyst; or by opening this cyst, passing one hand within it, and breaking down secondary cysts, while the other hand withdraws the tumour as it is emptied.

9. When there is a solid or semi-solid mass, so large that it will not pass easily through the opening, this must be carefully enlarged to the necessary extent.

10. If, as the tumour is withdrawn, omentum, mesentery, or intestine, are seen to be adherent to it, the adhesions must be carefully broken down with the fingers, or divided with the scalpel or scissors. When intestine is so firmly adherent that it cannot be safely separated, the adhering portion of cyst should be cut away, leaving it attaching to the intestine, but removing the inner secreting lining membrane of the cyst.

11. Any portion of omentum separated, should be carefully held to see that no bleeding vessel is returned. Any portion which appears to be much altered from the natural condition, or torn in the process of separation, should be cut away; and any bleeding vessel should be stopped by torsion, or by ligature. If by ligature, the ends should be brought out through part of the wound. The ligature should not include any portion of omentum—simply the bleeding vessels.

12. When the whole of the tumour has been withdrawn, it will be found to be attached to one side of the uterus by a pedicle which varies very much in length and breadth, and always contains large blood-vessels. This pedicle is first compressed by the chain of an *écraseur*, or by a clamp, close to the junction of the pedicle and cyst. The tumour may then be cut away, great care being taken so to arrange the flannels that no ovarian fluid can enter the peritoneal cavity.

13. The lips of the wound are then held apart, and the opposite ovary is to be examined. If diseased, it must be withdrawn, its pedicle secured, and the diseased organ cut away. If healthy, it is left undisturbed. A careful examination is then made, to see if there be any bleeding vessel at the spots where adhesions have been separated. Any such vessel may be secured by torsion, or by the pressure of a needle passed across it. Any blood or ovarian fluid in the abdomen or pelvis must be carefully removed by soft sponges wrung out of water at 96°.

14. The upper part of the wound is now to be closed by passing gilded harelip pins through the whole thickness of the abdominal wall, at intervals of an inch. Each pin should perforate the skin about an inch, and the peritoneum about half an inch from the incision on either side; so that when the opposed surfaces were pressed together upon the pin, two layers of peritoneum are in contact with each other. These two layers adhere together very rapidly; prevent pus or other secretions of the wound from entering

the peritoneal cavity; prevent adhesion of omentum or intestine to any part of the inner aspect of the wound not covered by peritoneum; and secure such firm union that a ventral hernia cannot occur after recovery.

15. The peduncle is next to be secured permanently. When the clamp does not drag on it too much, it may be left lying across the wound; but it will often be better to secure the pedicle permanently by ligature, and remove the clamp which has been used to secure it temporarily. Close below the clamp, the peduncle is transfixed by a needle which carries strong twine; and each ligature is so tied as to include a portion of peduncle of about a finger's breadth. One general ligature is then tied tightly around the whole as a security against bleeding from any vessel which may have been punctured. The smaller the portion included in each ligature, and the tighter it is tied, the more rapid is the subsequent process of separation. When the ligatures have been securely applied, the clamp is removed, and any superfluous portion of cyst is cut away; but care must be taken to leave enough beyond the ligature to prevent any danger of this slipping off.

16. When the peduncle is so long that the stump (or portion strangulated by the ligature) can be fixed outside the abdominal cavity, it is to be brought out at the lower part of the wound, and fixed there by a harelip pin, which is to be passed through it as well as through both edges of the wound. It is important that the ligatures should be on a level with the skin, and that the stump should be surrounded by dressing which separates the sloughing tissue from the raw surface. When the ligatures are fixed at the level of the peritoneum, raw surfaces surround the sloughing stump, and a sloughing condition of the wound, or putrid infection of the whole system, may possibly result.

17. When the peduncle is so short that the stump cannot be brought to the surface without great traction upon the uterus, the common practice has been to bring the ligature through the wound, and fix the free ends securely outside. In one case, they were carried through the inguinal canal beside the round ligament, and the abdominal wound was closed. In another, they were cut off short and left, the wound also being closed. It remains for further experience to determine which of these plans is to be preferred, or if only the vessels should be tied, instead of the whole thickness of the pedicle; or if acupressure can be applied successfully; or, if it would be safer to trust to the *écraseur*.

18. In whatever manner the pedicle may be treated, the wound must be finally closed by a sufficient number of superficial wire sutures, to bring the opposite edges of skin into accurate contact; and the wound may be brushed over with collodion, as an additional protection against the fœtid sanies which very soon begins to surround the decomposing stump.

19. The patient is then dried, and placed in a warm bed; a hot linseed poultice, or a hot water cushion, is placed over the abdomen; and the whole is secured by a flannel belt, which is kept in its place by a pair of thigh straps.

20. The principles of after-treatment are, to secure extreme quiet, comfortable warmth, and perfect cleanliness to the patient; to relieve pain by warm and moist applications to the abdomen, and by opiate enemas; to give stimulants when they are called for by failing pulse or other signs of exhaustion; to relieve sickness, by ice or iced drinks; and to allow plain, simple, but nourishing food. The catheter must be used every six or eight hours, until the patient can move without pain. The harelip pins are removed on the third day, unless tympanitic distension of the stomach or intestines endanger reopening of the wound. In such circumstances they may be left for some days longer. The superficial sutures may remain until union seems quite firm. When the pedicle is kept without, the dead stump and ligatures come away between the third and tenth days; but when the stump is left within the abdomen, the ligatures have remained for several weeks. The putrid sanies which surrounds the stump must be carefully cleansed away; and the wound and surrounding skin must be protected from it by means of greased lint or collodion. The dressing should be covered by a muslin bag

containing dry charcoal, or a powder of tar and gypsum, to prevent the appetite of the patient from being affected by any bad smell.

These details may appear to be needlessly minute; but I feel sure that it is upon the careful observance of little things that a great deal of our success depends. I have more than once had too much reason to lament the neglect of some of the rules which I have endeavoured to enforce.

Before concluding, I will lay before you the result of my own personal experience in the radical cure of ovarian disease.

a. Two patients have been radically cured by simple tapping.

b. Of five patients injected with iodine, three have remained without refilling for more than two years, and still remain well. In the fourth, secondary cysts have since grown to a large size, and ovariotomy (which might have been performed before the injection) now appears too hazardous to recommend. In the fifth case, the cyst has refilled after two injections: but the patient will try it a third time, as the coexistence of a fibroid tumour of the uterus, and the general state of health, forbid ovariotomy.

c. The cases in which I have punctured by the vagina and the rectum are still under observation; and I have not yet arrived at any further rule of practice than that before suggested.

d. In two patients, a free incision was made into a large mass of small cysts, in order to afford relief from pressure. The relief was given, and life somewhat prolonged; but both patients died after a few weeks.

e. In two cases, after making the first incision for ovariotomy, the operation was not proceeded with. In the first case, because the intestines were in front of and around the tumour. This patient suffered very little from the incision; and died four months afterwards from spontaneous rupture of a cyst into the peritoneal cavity. In the second case, adhesions were found to be very firm; and, as it had been arranged before the operation with the patient and her friends that if firm adhesions were found the operation should not be continued, I at once closed the wound. The patient had no bad symptoms. It was about a year ago, and she has been tapped several times since.

f. In twenty-six cases, I performed complete ovariotomy (thirty, see former note). Of these twenty-six patients, sixteen recovered perfectly. Fifteen (seventeen, see former note) are still alive, and in good health. One bore a child thirteen months after operation. Had the disease been allowed to run its natural course, or had simple tapping been the only surgical treatment adopted (none of the cases being suitable for iodine injection), I believe hardly one of these fifteen (seventeen) women would have been alive now; and, if any had lived, they would have been in the distressing state of all poor women who suffer from large ovarian growths. Had I selected my cases by refusing to operate where I was not very confident of success, I may say that scarcely a patient would have been lost. Some apparently desperate cases have recovered; and this has been the justification for operating in cases equally desperate. But it has led to several deaths which might have been delayed for some few weeks by thinking more of the reputation of surgery than of the prayers of the patient. It may be imprudent to operate in desperate cases; but the surgeon, if he fail, is not so much blamed as when he operates upon a person in tolerable health; and, if he succeed, the success is more brilliant. I think, however, we must look higher than this. We must remember that it is our duty to relieve suffering and save life, if we can; and, if we see a patient who must die soon—whose few remaining days or weeks must be days of suffering and sorrow—who (fully understanding that the effort to relieve her may hasten on her end) is anxious to be relieved—who may possibly feel that even death would be a relief—and who calls upon a surgeon for help—I believe, and act up to my belief, that it is the bounden duty of the surgeon to do his best, unless he is convinced that no help he can afford can be of any avail. If the result be unhappy, as it often must be in such cases, he will bear the disappointment with fortitude, knowing that he has done what he believed to be the best for his patient. But when, almost contrary to his expectations, almost beyond his hopes, he

finds a poor, weary, worn-out creature, fast sinking helplessly into the grave, restored in a few weeks to the full enjoyment of perfect health—she who was a burden to her family again the helpmate of her husband, the active mother of her children—the surgeon reaps that rich reward which you well know is so dear to all whose noble mission it is to relieve suffering—"the blessing of her who was ready to perish."—*British Medical Journal, Dec.* 28, 1861, *p.* 679.

129.—ON OVARIOTOMY; THE MODE OF ITS PERFORMANCE, AND THE RESULTS OBTAINED AT THE LONDON SURGICAL HOME.

By L. B. Brown, Esq., Surgeon to the London Surgical Home.

[The operation of ovariotomy is now a recognised and indeed a favourite operation—and the mortality from it has been shown to be less than after many other capital operations. The difficulty of an accurate diagnosis is often considerable, especially in cases complicated with cancer. Much, however, depends upon the history of the patient and her family, in arriving at a correct conclusion.]

Adhesions at the present time rarely prove an obstacle to the completion of the operation; they are either broken through with the hand, or divided with the knife or écraseur. When necessary to secure any by ligature, he advised the use of silver wires instead of thread or twine, allowing them to remain within the abdomen after cutting them short and close.

The author next fully described his mode of performing ovariotomy. He advises the pedicle to be enclosed in a clamp—the ordinary carpenter's callipers—as near to the the tumour as possible, and kept externally. Its advantages are, that it can be removed in from one to three days; the wound heals quickly, and convalescence ensues in two or three weeks. If the pedicle proves to be very short, and pain is complained of, the clamp is to be removed in a few hours.

The preliminary measures to be adopted previous to the operation were then described, and their importance shown as bearing on the subsequent results. Amongst others, the author advised the observation of certain atmospheric conditions, and the avoidance of any proceeding when the atmosphere was low and heavy, with an absence or deficiency of ozone, and in that condition generally which we describe as depressing. If greater attention were paid to atmospheric changes, the author thought that there would be much less of gangrene, pyæmia, low fever, &c., so frequently witnessed after operations.

The after-treatment was also dwelt upon, and finally, the analysis of the cases. Of the latter, ovariotomy has been performed nineteen times by the author in the London Surgical Home up to the present time. Of these, thirteen have been recoveries, and six deaths. The details and special particulars of the whole of these cases were given in a series of tables. The ages varied from 18 to 56. Of the successful cases, eight were under the age of 30, and five above; whilst of the unsuccessful, one was 21, and five were 30 and upwards. The duration of the disease in the successful cases was from four months to six years; six were within the first year, or ten within two years, and three over the latter period. Nine were single, and four were married, and of the latter, two only had had children. Five had undergone tapping from one to three times. In the unsuccessful cases the duration of the disease was from two to ten years; four were married, three of whom had had families of from three to six children. Four of the fatal cases were tapped from one to six times. The general health was very good in five of the successful cases; in six it was but middling; in one it was shattered, and in another bad. In the unsuccessful cases four had bad health; one was in good health, and another had good health up to six weeks before the operation.

With respect to the operation, the incisions varied from three to seven

inches long; in eleven it did not exceed five inches. The tumours were multilocular in eleven, and unilocular in two of the successful cases; in the unsuccessful they were mutilocular in four, unilocular in one, and more or less solid in the sixth, containing hair, teeth, bones, &c., and no doubt congenital. Adhesions were found in all except four of the successful and one of the unsuccessful cases. These varied very much, being either very slight and easily broken down, or firm, strong, and unyielding; some were numerous in all directions, requiring to be cut or ligatured. Chloroform was given in all the cases; in two it had to be discontinued, but the patients suffered no pain; in two instances ether was substituted during the latter part of the operation. The pedicle was retained outside of the abdomen in all but two of the cases, the callipers being used for the purpose of holding it. In all the operations performed lately, the wound had been closed by silver-wire sutures, simply twisted. Of the causes of death in the six fatal cases, in two instances it clearly arose directly from the operation itself: in the other four, conditions were found which chiefly brought about this result; in one case—that of the solid tumour—there was much old disease found; in another, the patient had been a hard drinker, was tapped five times, the belly being filled with forty-five pints of ascitic fluid, independently of the contents of the ovarian cyst, there was softening of the liver, and death occurred in six days. In the other two cases, diarrhœa carried off both—one in eight days, and the other in eighteen after the operation. In one of these, cancer of the duodenum was found, wholly unsuspected during life. The author concluded by stating that all these operations had been witnessed by gentlemen from various parts of the world, of Great Britain, and the metropolis, many of whom had watched the results from day to day and week to week.

Mr. SPENCER WELLS said that the recommendation of the author of the paper to tie adhesions with silver wire, to divide the adhesions, and then cut off the wire short, leaving a portion of dead tissue strangulated by the wire within the peritoneal cavity, was advice which he should not be disposed to follow. The material of which the ligature was composed could be of little importance. Either wire or twine would equally cause gangrene of the strangulated part, and there would be great danger that the whole system might be poisoned by absorption of the fetid matter of the decomposing slough. He felt convinced that if this practice were followed the mortality after ovariotomy would increase. He preferred a ligature of wire-rope, tightened by a screw, to the calliper-clamp; and he thought (after trying both seton-wire and harelip-pins for the closure of the wound) that harelip-pins were generally to be preferred. The lateral pressure made by the cotton twisted round the pins fixed the two wounded surfaces more closely and securely than the wire. He differed entirely from the author in his preference to flannel over sponge in cases where it was necessary to cleanse the peritoneal cavity from blood or ovarian fluid. A soft, clean sponge did the work more effectually, and there was not the danger, as there was with flannel, of leaving portions of fine wool, or *fluff*, adhering to the peritoneum. He also thought that the author's advice would lead to a want of care in thoroughly cleansing the peritoneal cavity from ovarian fluid. In a recent case he had been led by some such advice, and by what he had heard of the practice of other surgeons, to leave some ovarian fluid in the cavity, thinking that as the intestines were exposed it might be more dangerous to sponge away the fluid than to leave it; but the result had very much disappointed him. There was little else unfavourable about the case, yet the patient died, thirty hours after operation, of acute peritonitis, and the whole membrane was covered by a layer of the albuminous portion of the fluid. The peritoneum seemed to have acted as a dialyser, allowing the water to pass through, and leaving a coating of albumen on its surface. He (Mr. Wells) could hardly suppose that either inhalation of the vapour of turpentine after the operation, or the exhibition of arnica before it, could have any great influence upon the result. He had never used either, and his results were not less satisfactory than those of the author. It was singular that he had had exactly the same number of cases in the Samaritan Hospital as Mr. Brown had had in his Home—

namely, 19. Mr. Brown's returns showed 13 recoveries and 6 deaths. His (Mr. Wells'), 11 recoveries and 8 deaths. His cases in private practice had been 15, of whom 8 recovered and 7 died. The total gave 34 cases, with 19 recoveries and 15 deaths. He trusted that Mr. Brown would also give the Society the result of his *whole* experience of ovariotomy. He (Mr. Wells) had not been able to do more than compare their first 20 cases; and it was singular that with these 20 the number of recoveries and deaths was directly reversed, for Mr. Brown had had 13 deaths and 7 recoveries; while he (Mr. Wells) had had 13 recoveries and 7 deaths. He was also of opinion that in some of the fatal cases brought before the Society this evening, the operation ought not to have been performed. The patients died of serious organic disease of the heart, or some other important organ—cancer of the intestine, for example; or the uterus was so closely surrounded by adhering pelvic portions of the ovarian tumour that separation could not be effected. Such conditions could generally be detected during life, and ought, with rare exceptions, to lead the surgeon to refuse to operate. It often became our duty to operate in very unfavourable cases, and many of his own fatal cases had occurred under conditions so unfavourable that a good result could not fairly be expected; but the operation was decidedly contra-indicated in any case of serious organic disease of heart or lungs, or of malignant disease in any important organ.

Dr. ROUTH said he had seen a large number of cases of ovarian disease; he spoke, therefore, from experience. First, he believed we could never be certain beforehand on some of the most important points of diagnosis in ovarian disease. Extensive adhesions had been often diagnosed before operation, which were afterwards found to be limited or absent. In other cases their absence was predicted, while the operation revealed that they were very firm and numerous. The presence of co-existing ascites greatly increased the difficulty, so as to make a preliminary tapping necessary before ovarian disease could be made out at all. Secondly, he had known some of the most eminent men in the diagnosis of ovarian disease mistake a large cyst of the kidney, extra-uterine pregnancy, and fibroid disease of the uterus, for ovarian dropsy. Thirdly, Mr. Wells objected to the expediency of operating in cases of cancer of the intestines, which he stated was always to be made out before operation. In one of Mr. Brown's cases there was cancer of the duodenum with a large ovarian cyst in front of it. How could such a disease be made out? It was simply impossible. In honesty he (Dr. Routh) was bound to add that post-mortems often revealed the coexistence of other disease which not only could not have been made out before operation, but which could not even have been suspected. Fourthly, his impression was that the non-existence of adhesions, and a perfectly healthy peritoneum, were not necessarily favourable to recovery. He had seen such cases followed by severe peritonitis; while others, in which the peritoneum was almost entirely disorganized by extensive adhesions, which needed very great force to tear them asunder, recovered without one bad symptom. Lastly, he thought that the practice of sponging out the contents of a cyst, if accidentally effused during the operation, was better than leaving the fluid in the cavity of the peritoneum.—*Lancet, March* 22, 1862, *p.* 303.

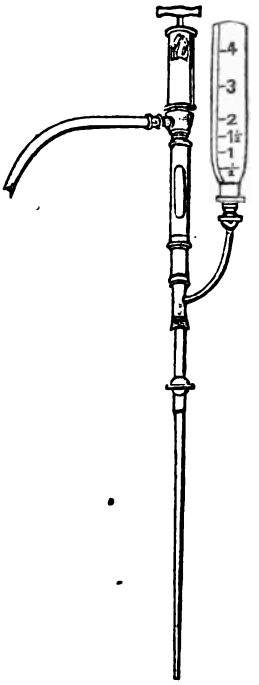
130.—TROCHAR-SYRINGE FOR INJECTING OVARIAN CYSTS.

The instrument shown in the accompanying sketch has been contrived by Mr. Spencer Wells in order to enable the surgeon to empty large ovarian cysts, inject solution of iodine, and remove it again, if necessary, without any risk of admission of air.

The trochar is fourteen inches long, and is covered by a canula of gum elastic. The trochar is used in the ordinary manner; but as it is withdrawn, the canula is pushed on to the bottom of the cyst. A syringe, a stop-cock, and a graduated glass vessel for the iodine, may be so secured that every

drop of ovarian fluid may be removed, and the entrance of air is prevented during the escape of the fluid, the entrance of the iodine, and the withdrawal of any portion of the iodine. Messrs. Weiss were the first to make this instrument.

A strong watery solution of iodine in solution of iodide of potassium appears to be preferable to an alcoholic solution. The symptoms, in some fatal cases, have been those of alcoholic rather than of iodine poisoning. The solution Mr. Wells has generally used has been a scruple of iodine and half a drachm of iodide of potassium in an ounce of water.



When a cyst holds from ten to twenty pints of fluid, from one to two ounces of this solution may be left in the cyst. When a cyst holds from twenty to fifty pints of fluid, it would probably be better to inject eight, ten or twelve ounces of the solution, so that the whole of the lining membrane may be acted on, and then to remove the greater portion of the solution.

These were the rules Mr. Wells laid down in a paper read at the last Annual Meeting of the British Medical Association. He said that the injection of iodine may be practised after tapping in cases when a cyst is single, or when one cyst is so far developed beyond others as to be the chief cause of distress. But it is dangerous, and must be useless, in cases of compound cysts or semi-solid tumours. It would seem to be advisable in cases of large single cysts, where simple tapping has proved useless, or where something forbids ovariotomy. Of five patients he injected, three remained without refilling for more than two years. Two still remain well. The third has been injected a second time, as the cyst began to refill about two years after the first injection. In the fourth case secondary cysts have since grown to a large size, and ovariotomy (which might have been performed before the injection) now appears too hazardous to recommend. In the fifth case, the cyst has refilled after two injections; but the patient will try it a third time, as the co-existence of a fibroid tumour of the uterus, and the general state of health, forbid ovariotomy.—*Medical Times and Gazette, Jan.* 4, 1862, *p.* 23.

131.—ON VESICO-VAGINAL FISTULA.

By Dr. Marion Sims.

[The following is an abstract of a clinical lecture delivered at the Charité, at Paris, by request of M. Velpeau.]

The principal points, said Dr. Sims, in which my practice differs from that usually pursued at the present day, I may arrange as follows:—1st, as regards the position of the patient: 2nd, as concerns the manner in which I pare the edges of the fistula; and 3rd, the number of sutures I apply, and their distance from the lips of the wound.

In the commencement of my practice I was in the habit of placing my patients on their knees and elbows, the head being depressed and the pelvis raised. This posture I abandoned; it presented two great disadvantages—one in rendering the administration of chloroform impossible or nearly so, the other in fatiguing the operator by obliging him to remain standing. I now place my patient on her left side, her thighs flexed at a right angle with the pelvis, and maintain the upper part of the trunk in pronation, with the

sternum towards the bed or table, on which there must be neither pillow nor cushion. By this position of the trunk the organs of respiration are not compressed, nor are the abdominal viscera pressed into the pelvis so as to displace the bladder or vagina. The speculum of which I make use has a special form; its blades curve towards each other so as nearly to meet at their distal extremity, an arrangement which prevents the possibility of injury to the vaginal coats. I divide the operation into three stages: 1st, the paring of the lips of the wound; 2nd, the passing of the ligatures; and, 3rd, the adjustment and torsion of the metallic sutures.

For the first stage four instruments are requisite—a pair of straight or curved scissors, a bistoury, a tenaculum, and a pair of forceps. With the tenaculum I seize the vaginal mucous membrane near one of the angles of the fistula, and with the bistoury slightly incise the *vaginal* mucous membrane. Raising this tissue with the tenaculum, I with a pair of straight scissors cut through the whole thickness of the edge of the wound *down to* the vesical mucous membrane, which it is all important to avoid wounding. I then proceed to attack a neighbouring portion, and so on until the whole surface is pared.

In the second stage I use a tenaculum, catch-forceps, and needles threaded with a double silk, having a loop at one extremity. Raising a lip of the wound with the tenaculum, and holding the needle by means of the forceps, I insert it into the tissues near an angle of the fistula, at about a third of an inch from the free margin of the wound, taking care when bringing it out not to include any of the vesical mucous membrane. Guiding the point of the needle on the tenaculum, I withdraw the forceps, and draw the point forwards by reapplying the instrument to that extremity. In reapplying the needle in order to complete the stitch, I commence in the same way from the lip of the fistula, and cause the needle to emerge at a distance of a third of an inch from the wound. When one suture is thus complete, I hitch both ends into a notch made in a light piece of stick until required, and proceed. The needles, which are nearly straight and fine, should be introduced obliquely, for fear of wounding the vesical mucous membrane; and the stitches should be five, six, or even more in number. When all are *in situ*, I take the loop of the first, pass through it a silver wire, which I twist so as to be securely fastened, and by gentle traction substitute for each silk suture one of silver wire.

In the third stage, the operation is completed by the twisting of the silver threads or wires. For this purpose I have a small metallic conductor, furnished at its extremity with a square plate somewhat similar to the handle of the common grooved director. Into the fissure of this instrument I insert the two ends of one of the silver sutures, and give a gentle twist to draw the thread tight; which done, the ends are to be cut so as to leave a length of about two-thirds of an inch in connexion with the wound; these I bend back at a right angle, so that the force of the torsion may not act upon the lips of the fistula but on the wires only, and then complete their fastening by a few more twists with a forceps.

Before carrying back the patient to bed, a catheter must be introduced; this instrument is shaped like an S, and its vesical extremity is pierced with little pin-holes. The bladder should be first washed out with an injection of water, and the catheter left *in situ* and changed at least twice a day. The diet must be generous, and the administration of morphia in doses sufficient to produce constipation is indicated; a quarter of a grain being given twice, the first dose immediately after the operation and the second at night. The sutures may be removed on the eighth or ninth day, for this purpose the twisted end is drawn forward by the forceps until the part which has been united is brought to view, and *then only* is the silver wire to be divided. These details, added Dr. Sims, may seem minute and elaborate, but I believe them all important, more so than the skill of the surgeon or the brilliant manner in which he may achieve the operation.—*Lancet*, *Dec.* 7, 1861, *p.* 560.

132.—INTRA-UTERINE FIBROUS POLYPUS; REMOVAL BY DR. HICKS' WIRE-ROPE ECRASEUR.

Under the care of Dr. J. Braxton Hicks, Guy's Hospital.

[The patient was a woman 43 years of age, who, until admission had been suffering from violent menorrhagia and profuse leucorrhœa. The uterus was enlarged, the os thin, and distended to the size of a crown-piece, the cervix obliterated, and a tumour just within, evidently causing the distension.]

A few days after entry, the patient was placed under the influence of chloroform, and removal was effected as follows:—The annealed steel rope was passed to the base of the polypus by the two canulæ, fitted with a wire loop passing up within each, which were then carried round the growth. The two ends of the rope were threaded through the eye of the shaft, and one fastened to the traversing hook, the other to the fixed pin. Having withdrawn the wire loop, the canulæ were removed, and the screw was employed. The section was completed in a minute. The instrument having been removed from the uterus, the polypus was found rolling free within its cavity, and its extraction was easily effected by means of a pair of small midwifery forceps. The operation was completed in less than ten minutes. The patient was able to leave the hospital in a week, and recovered rapidly without a bad symptom.—*Lancet, Feb.* 1, 1862, *p.* 121.

133.—TREATMENT OF ENCYSTED TUMOUR OF THE LABIUM BY FREE INCISION.

Under the care of Mr. Tatum, at St. George's Hospital.

The diagnosis of encysted tumours of the labia is generally easy enough, although they have been mistaken at times for a femoral hernia. The long duration, moveable character, true cystic feel, and the absence of impulse on coughing, will readily lead to their true appreciation. The methods of treatment in ordinary use consist of simple puncture, the seton, injections of iodine, and dissecting out the cyst. All these we have seen employed with success; but the mode for which a preference seems to be shown is the last, one strongly advocated by Mr. Baker Brown in his work on the Surgical Diseases of Women. Dr. Oldham employs the seton at Guy's Hospital with, it is stated, invariable success. This is not the rule in the other hospitals; for a fistulous opening has been observed to follow, which necessitated subsequent dissecting away of the remains of the cyst.

On the 20th of January, a woman, aged about 35 years, was admitted into St. George's Hospital, with a tumour of the right labium, as large as an orange, which had been growing for the period of ten years. It was pendulous, hung down between the thighs, and resembled a hernia; but its true nature was quite apparent. It caused the patient considerable inconvenience, and much interfered not only with locomotion, but attention to her ordinary avocations; it was at the same time an unsightly object.

On January 23rd, chloroform being administered, the cyst was punctured and emptied of fluid. The latter was of a dark chocolate-brown colour, of creamy consistence, and unpleasant odour, and no doubt contained, as Mr. Tatum supposed, degenerated blood (which may have impregnated a previously straw-coloured fluid), the result of some possible injury. In this respect it greatly resembled the contents of a hematocele in the male subject. The cyst was now slit up with a scalpel, as much of it being dissected away as could be got hold of, and the wound filled with lint, to heal up by suppurative action.

The patient is doing extremely well, and her radical cure is a question of a few weeks at furthest. She had not undergone any previous operation.—*Lancet, Feb.* 1, 1862, *p.* 121.

134.—ENCYSTED TUMOUR OF THE LABIUM; ABLATION OF THE ENTIRE CYST.

Cases under the care of Mr. I. B. Brown and Mr. Hillman.

We think that most surgeons are agreed on the propriety of dissecting out the entire cyst, when circumstances will permit of it, for then the cure is complete. Mr. Brown urges the necessity of care being taken, in the after-dressing, to ensure a healthy granulating surface at every spot. This may be accomplished, he writes, either by dressings of dry lint, or by a cerate made of turpentine, oil, and resin cerate, equal parts; or by touching the surfaces with nitrate of silver.

A female, aged 33, was admitted into the Home, in November last, with a cyst in the left labium, of the size of a pigeon's egg, which had existed for six years. She was in perfect health, and her limbs were remarkably well developed. Under the influence of chloroform, on the 14th of November, Mr. Brown carefully dissected out the entire cyst. This was an operation of some delicacy, as its walls were extremely thin. The fluid contents were of a light-brown colour. Her recovery was perfect in the course of a few days. Mr. Brown's experience, in a large number of cases, is to the effect that nothing short of removal is of permanent benefit.

[The following is a case operated on by Mr. Hillman, at the Westminster Hospital.]

In the present instance, it was determined to dissect the cyst completely out, and this was an operation of some nicety, as the tumour was comparatively large (the size of an apple), and its walls thin. The patient was a woman aged 28 years, who had been the subject of the cyst for several years. Under chloroform, on the 1st of October last, it was dissected away, but burst immediately before complete ablation was accomplished. The contents were a yellowish-green, watery fluid, with a most intolerable odour, and in bursting some of this fluid was ejected from the cyst to a considerable distance. Recovery was complete.—*Lancet, Feb.* 1, 1862, *p.* 121.

135.—ON SUBSTANCES EXPELLED FROM THE GENERATIVE PASSAGES: THEIR NATURE AND DIAGNOSTIC SIGNIFICANCE.

By Dr. Graily Hewitt, Physician to the British Lying-in Hospital.

[From the substances, solid or otherwise, and said to have been expelled from the generative passages of women, we are often called upon to decide questions regarding the presence of pregnancy either present or past. An accurate conclusion is often very necessary, and no "off-hand" opinion ought to be given.]

In the very practical and valuable work of Dr. Montgomery, the substances which may be expelled from the generative passages are enumerated as follows:—"1, an early ovum; 2, a mole; 3, uterine hydatids; 4, the membrane produced in dysmenorrhœa or other conditions of uterine derangement; 5, membranous formations from the vagina." This does not, however, include all the substances which may present themselves to be examined and reported on; and the arrangement here adopted will be somewhat different from that of the author just alluded to.

A. In the first place, we shall consider those substances expelled or removed from the generative passages more or less resembling *flesh* in their outward form and appearance. By the term "flesh," or "fleshy" masses, they would be popularly designated; and it is therefore useful practically to group them together.

Now the "fleshy" substances may be found, on examination, to be composed of—1, an ovum cast off at an early period of its growth, or arrested in its development, and retained in the uterus, constituting what is popularly known as a *mole*; 2, the placenta, or a portion thereof, retained for a time,

and subsequently expelled; 3, polypus of the uterus spontaneously detached and expelled; 4, fibrous tumours of the uterus similarly separated; or, 5, coagula of blood.

1. Is the substance examined *an early ovum?* In order to answer this question satisfactorily, the observer must be acquainted with the structure of the ovum at an early period, and be able to recognise its several parts. If any part of the body or members of the fœtus be found in the mass expelled, there can, of course, be no doubt in the matter; we have to do with an abortion. When no part or parts of an embryo are to be found, we are obliged to search for other marks by which we may recognise the product in question as the result of conception. We proceed to search for one of the following structures: the decidua materna, or external envelope of the ovum; the decidua reflexa, internal to the latter; the chorionic villi; the umbilical cord; &c.

After soaking the specimen in water, it should be carefully examined in that fluid, a blunt probe and forceps only being at first used in manipulating. In ova which come away *en masse* (a rather rare occurrence) during the first three months or so of pregnancy, the external covering will be the *decidua materna;* and this membrane is recognised by its ragged, uneven appearance under water, by the presence of those bodies described first by Dr. Montgomery as "decidual cotyledons" on the external surface, by its pyriform shape, and by the fact that there are three openings—two superiorily, corresponding to the Fallopian tubes; and one inferiorly (which latter may, however, be closed), corresponding with the neck of the uterus. The decidua is further characterised by the smooth and velvety appearance of that surface of it which is internal; and by the fact that on this surface many minute openings are visible, giving the membranes a cribriform look. After the fourth month, this decidua is found thinner, and some of the characters just alluded to less marked. This decidua may come away by itself, torn or separated from the ovum, or filled with the degenerated structures of the ovum.

The *decidua reflexa* is a fibrous membrane, thinner than the decidua materna; and it encloses the ovum proper in great part. The most characteristic structure of the ovum is, however, the *chorion membrane*, and the *villi* which proceed from its external surface. The early ovum, separated from the decidual coverings, is a closed bag covered by little delicate processes, the villi of the chorion, giving to it a shaggy appearance. In a very early ovum, the villi are small and simple; but in a more advanced ovum—of two months, for example—each original villus has given off branches arranged on it somewhat like the roots of a tree. Later still, these villi are longer, and they are then found *on one side only* of the ovum; those on the other side, not participating in the development described, shrink and disappear for the most part. The presence of chorion villi may be considered quite conclusive as to the fact of impregnation and previous conception, whether the embryo be found or not; but it is a question whether the same degree of diagnostic value attaches to the presence of the decidua materna; for it is an established fact that a *membrane* may be expelled from the uterus quite independently of conception, and possessing many of the characters of the decidua as above described. The difference between the two is that, in the decidua resulting from pregnancy, the decidual cotyledons are present; but not in the other. Such, at least, is the distinction at present recognised.

If a small sac were found, having attached at a point of its internal surface the remains of the umbilical cord, this would be conclusive on the question of impregnation. In the case of early ova expelled from the uterus, one of several results may be observed. The ovum enveloped in the decidua may come away entire; the decidua may remain behind for a time; and the chorion membrane, covered by its villi, and enclosing the embryo, be expelled by itself. The chorion and amnion may remain *in utero*, as well as the decidua; the embryo escaping from the uterus. In such a case, the membranes, fœtal and maternal, would be subsequently expelled.

Moles.—The various substances known under the designation of moles,

and which are the products of conception, are for the most part the result of arrest of development, which may co-exist with continuance of growth, of some portion of the ovum. The "fleshy mole", as it is termed, consists of an ovum between the membranes of which blood has been effused. The blood effused has coagulated, and the result is a mass, the parts of which are glued together and separated with difficulty. The villi of the chorion must be carefully sought for; and they ought in all cases to be found where there is no reason for suspecting that the ovum, with its chorionic investment, has previously escaped. The presence of organised membranes and chorion villi distinguishes the "fleshy mole" from simple clots of blood, and from other substances to be presently alluded to more particularly. It must be recollected that the chorion villi do not become developed so as to constitute what may be called the placenta, until about the fourth month of gestation. It is difficult to imagine that a fœtus which has lived until the placenta has been formed could undergo a process of complete absorption; but there are very numerous cases on record to show that degrees of development of the chorion villi short of the production of a placenta frequently co-exist with entire absence of the embryo, which in such cases perishes at so early a period that its size is very inconsiderable, and it escapes detection.

There is another kind of true mole, the "hydatidiform" or "vesicular" mole, the description of which will come presently.

2. *The Placenta.*—The fleshy mass expelled may be a part or the whole of a "placenta." When a woman has been delivered at or near the full time, and no placenta has come away, a fleshy-looking mass expelled a week or two after would in all probability be the placenta; and in such a case the signs of previous delivery would be present, the history of pregnancy, &c. The size, shape, &c., of the mass, and the presence of the umbilical cord, would externally indicate it to be the placenta. The expulsion of a retained placenta is, at least when the retention has existed for some time, usually preceded by an offensive discharge; but the placenta has occasionally been discharged apparently fresh, and without signs of decomposition.

Such cases are rarely open to much chance of misconception; but the nature of the case is not so obvious when the fœtus has been expelled at an earlier period of gestation. In cases of abortion at the fourth or fifth month, the placenta may be retained for some time, its removal having been neglected or escaped attention at first. Cases are on record which show that the placenta may be retained within the uterus after abortion, for months and even years. An instance in point is quoted by Montgomery from Morgagni.—(See Montgomery, op. cit., p. 259.) More than one case of the kind has indeed come under my own observation. Meanwhile, its presence in the uterus has occasioned generally severe hemorrhages. An early placenta would be of about the size of a pigeon's egg; later, it would be larger. If recognised as a placenta, it would indicate a previous conception. The substances which might be mistaken for a placenta are, a fibrous tumour spontaneously expelled, or a fibrous polypus similarly removed; and in both cases the symptoms, hemorrhages, &c., might be somewhat alike. The structure of the fleshy mass must then be carefully examined, to settle the point, if evidence as to previous pregnancy be wanting, or if that which is obtainable be open to suspicion. An early placenta is rounded, curved on one side, and smooth on the other. The presence of the umbilical cord attached to one side of it would be conclusive; but this might be worn away or torn off close to the placenta. A section of the mass would, however, show vessels arranged in a peculiar manner, radiating from the centre of one surface.

3 and 4. *Fibrous polypi* of the uterus and *fibroid tumours* are sometimes expelled spontaneously from the uterus. Externally, these bodies might be easily confounded with a placenta, the more especially as the preceding hemorrhages might be considered evidence of abortion having occurred. Polypus of the uterus and fibroid tumours frequently produce abortion; and in certain cases abortion may occur in the first place, and the expulsion of the polypus, which gave rise to the abortion in the second. This sequence

happened, as I had reason to know, in a case under the care of a gentleman in the country; and the polypus which came away was considered, until after it had been more carefully examined, to be the placenta. The structure of a polypus or of a fibrous tumour differs widely from that of the placenta, the former presenting a fibrous texture, generally dense, and sometimes very firm; but now and then, in the case of a polypus, more spongy and loose. The insertion of the umbilical cord would be, of course, wanting. Fibrous tumours of the uterus spontaneously expelled may be found to have become altered, degenerated into a *fatty mass*, as in a case related by myself in the *Transactions* of the Pathological Society, vol. xi. p. 173; or solidified by *calcareous matter*. Generally we find a previous history of "frequent and severe hemorrhages," when these uterine outgrowths have been expelled. The spontaneous expulsion here alluded to is not a frequent termination of their history.

5. *Coagula of blood* (blood-polypi, Kiwisch) retained within the uterus for some time, and expelled subsequently in a more or less firm condition, require to be discriminated from the bodies hitherto alluded to. Coagula may form within the uterine cavity in connexion with uterine hemorrhage of all kinds; after labour, in consequence of the presence of polypi, cancer of the uterus, profuse menstruation, &c. The uterine cavity is not, as a rule, very tolerant of the presence of clots; and for this reason they do not generally remain there sufficiently long to have become firm and dense. They are frequently, as Scanzoni remarks, connected with previous abortions. When the coagula are tolerably recent, they are easily broken down under pressure, or after soaking in water. Fibrous organised bodies are not to be broken up in this manner. When polypi of the uterus are present, coagula sometimes come away having a circular form like segments of rings. The polypus at the same time excites hemorrhage, and prevents the escape of the blood; and the rings in question are thus formed. Coagula not recent may present a tolerably firm, dense, greyish, fibrinous-looking surface. The want of organisation in the mass, the presence of blood-corpuscles, would assist in the diagnosis of the nature of the substance. The centre of the mass, moreover, generally exhibits a clot of a darker colour, comparatively unaltered, which was the original nucleus of the formation.

B. *Bodies more or less resembling "skin"* may be conveniently considered together under the designation of *membranous formations*. The skin-like substances in question may have their origin in the vagina or in the uterus.

1. *Exfoliations from the Vagina.*—Under certain circumstances, the lining membrane of the vagina separates in the form of thin translucent flakes, which sometimes come away in great quantities. Dr. Tyler Smith designates that condition of the vagina present in such cases as "epithelial vaginitis." The flakes in question are composed of the scaly epithelium of the vagina, and under the microscope exhibit the well-known appearances of this form of epithelium. It is necessary to place them in water, in order to render obvious the characters of these exfoliated products.

2. The *dysmenorrhœal membrane*, as it has been called, but concerning the real nature of which the amount of our present knowledge is by no means satisfactory, is described as an exfoliation of the lining membrane of the uterus—a sort of skin occasionally expelled from the uterus, independently of conception, after a catamenial period, exhibiting a certain degree of resemblance to the decidua lining the uterus during pregnancy. The best observers for the most part agree in stating that this membrane is neither more nor less than the mucous membrane of the uterine cavity, hypertrophied and cast off. The formation of the membrane is supposed to be connected with the catamenial nisus; and it has been called the dismenorrhœal membrane, on account of its association with those varieties of uterine pain known under the name of dysmenorrhœa. The membrane in question is smooth internally, rough and slightly flocculent externally. When thrown off in a single piece, it presents three apertures corresponding to the apertures communicating with the uterine cavity, and is of a pyramidal shape. It is generally expelled towards the end of the catamenial flow, which, as a

rule, is more profuse than usual. It is unlike the products of vaginal exfoliations just alluded to, in being thicker. The distinction of this dysmenorrhœal membrane from the decidua of an early ovum might, under certain circumstances, be difficult, as already stated; viz., when the supposed decidua is unaccompanied by any part of the chorionic structure. The concomitant circumstances will assist in the diagnosis: thus the "dysmenorrhœal membrane" is not expelled at one catamenial period alone, but on successive occasions; whereas, in the case of an abortion, the same thing is not likely to recur, or, at all events, with the same marked periodicity.

3. *The Covering of the Early Ovum.*—Portions of the decidua uterina, the decidua reflexa, the chorionic sac, &c., may come away in the form of membranous substances. It is unnecessary here to repeat what has been already stated as to the diagnosis of the nature of these bodies.

C. *Vesicular Bodies.*—Little bladder-like substances, single or connected in series like beads, may be expelled from the uterus. These bodies were formerly considered to be hydatids of the uterus. They really result from certain alterations of the chorion villi, and they are always the result of conception. The embryo perishes at an early period, and the chorion villi, continuing connected with the uterus, maintain a slow growth, the *development* being arrested. The vesicular-like bodies are thus the result of dropsical swelling of the chorion villi. It appears that the period of pregnancy during which the chorion villi may take on this peculiar form of degenerative growth is limited, probably not later than the middle or end of the third month. If the embryo perish after the chorion villi have become pretty intimately connected with the decidua serotina, but before the placenta has become formed, while the villi are still allowed to retain a certain degree of connexion with the uterus, they may continue to grow; but *development* is arrested, and the bladder-like bodies are the result.

With the presence of the vesicular mole, watery discharges are occasionally associated. The hydatidiform mole may attain a considerable size, remain several months in the uterus, a few of the bladders from time to time breaking and discharging fluid from the os uteri; and the mass may come away altogether, or clusters of the vesicles may be expelled at intervals.

Connected with this subject is an important practical question which I have in another place attempted to answer; viz.: "*Can a portion of retained placenta take on the hydatidiform change?* The placenta belonging to a mature fœtus cannot, if healthy at the period of the birth of the child, become the seat of the hydatidiform change, the chorion villi having long since disappeared and become converted into blood-vessels. The only circumstances under which hydatidiform bodies might be subsequently expelled from the uterus, and give rise to the supposition that they arose from degeneration of a retained placenta, are, as I believe, the following:—Firstly, in cases of double conception; when, one embryo having perished at an early period, the membranes thereto belonging have undergone the hydatidiform degeneration, and are not expelled from the uterus, together with the normal placenta. In illustration of this position, a case may be referred to which was published in the Lancet for 1846, vol. i., p. 430, in which a hydatidiform mass (in bulk about three pints) was expelled *together with* a normal placenta. In this case, it is most probable that there was a double conception; and if the remains of the diseased ovum had not been expelled *with* the normal placenta, but some weeks or months subsequently, the case would have come under the above category. A second possible case is, that a portion of the chorion villi may become separated organically from the fœtus at an early period, and undergo the hydatidiform degeneration; whilst the remainder grow and nourish the fœtus up to the full time. . . . If the diseased portion were retained in the uterus, the supposition before alluded to might arise."—(*Obs. Trans.*, vol. i., p. 263.)

True hydatids may in very rare instances be expelled from the generative passages. They probably originate in the abdomen, bursting into this cavity from the liver; and they may possibly penetrate through the uterus, or into the vagina. True hydatids are closed sacs one within another; while the

vesicular bodies resulting from chorionic transformation are arranged in a series like beads on a string with slender peduncles or intervening connecting portions. The well-known "hooklets" are usually found when the cysts are really of hydatid origin. I have met with a case in which death took place; several hydatid cysts were found in the abdomen, the pelvis, &c.; and, had life been prolonged, some of these might have burst into the vagina or uterus. In the case in question, the patient was a young unmarried woman.

D. *Factitious Bodies.*—Lastly, the observer must be cautioned as to the occurrence of cases in which, for a variety of reasons, women exhibit substances which they are desirous of leading the practitioner to believe have been expelled from the vagina. The careful examination of the bodies in question is, or should be, sufficient always to detect the fraud.—*British Med. Journal, Dec.* 14, 1861, *p.* 627.

136.—*Instrument for the removal of Polypi of the Uterus.*—By Dr. Tyler Smith.—[This instrument was exhibited at a meeting of the Obstetrical Society of London.]

It consisted of a rod and winch with double canula, made sufficiently strong to carry wire, and bear tension enough to cut through the neck of a polypus at once. In his opinion the best practice was to remove polypi from the uterine cavity, and not to wait until they had been extended into the vagina. Many of the deaths in cases of polypus arose from delay in their removal, causing excessive and long-continued hemorrhage, or the poisonous effects of the decomposing mass, when the ordinary ligature was used, and many days occupied in cutting through the polypus. With this instrument and a single or stranded iron or copper wire, it was generally possible to remove a polypus in a few minutes, whenever the os was sufficiently open to admit the canula.—*Medical Times and Gazette, Dec.* 14, 1861, *p.* 621.

137.—CASE OF UTERINE HYDATIDS.

By Thomas Tweedale, Esq., Uppermill, Manchester.

On August 24th last, I was called upon to attend Mrs. ——, aged 33, in her third confinement. On arrival, I found her almost pulseless, from excessive hemorrhage. On inquiry I ascertained that she had suffered from hemorrhage for twenty-five weeks previously, but not to such an extent as to require medical assistance.

Externally the uterus seemed very much distended, in fact the woman was under the impression that she would have twins, and had an idea that she could feel their movements. On making an examination per vaginam, I found the os uteri dilated to the size of half-a-crown piece, but could not distinguish any presentation. Fearing danger from the continued hemorrhage, I at once introduced my hand into the uterus, for the purpose of turning the fœtus if necessary, and was surprised to find its contents break down under very slight pressure.

Having satisfied myself there was no fœtus, I withdrew a portion of the contents, and found them to be hydatid cysts, averaging the size of full grown grapes. I then re-introduced my hand into the uterus, and removed the whole of them, weighing between five and six pounds. The uterus at once contracted, and the hemorrhage ceased. In a few hours the mammary glands performed their functions, and the case proceeded as one of natural labour.

No unfavourable symptoms setting in, the woman rapidly recovered, and when I last saw her she was following her usual employment.—*Medical Times and Gazette, Nov.* 16, 1861, *p.* 513.

138.—*Tuberculosis and Uterine Disease.*—The following case is of interest as showing the connexion which may exist between certain diseases. A woman aged 27, began to suffer from dysmenorrhœa after her fourth confinement. The courses were tardy and painful, and she also suffered from leucorrhœa, disturbed digestion, oppression in the stomach, flatulence, emaciation, loss of power, vertigo, palpitations of the heart, &c. At last symptoms of phthisis ensued,—viz., a distressing cough, hæmoptysis, sweat in the night, diarrhœa, and fever in the evening. There was a dull percussion sound below the right clavicle, and the respiratory murmur was indistinctly heard. On examining the womb the collum uteri appeared swollen, the two labia of the os were quite ulcerated, and suppurated freely, while mucus flowed away from the cavity of the organ. The cervix uteri was then cauterised twice a-week, whereby a cure of the abnormal secretion and ulceration was brought about. At the same time the digestion improved, the patient gained strength and flesh, the fever disappeared, the percussion sound lost its dulness, and the pain below the clavicle was also gone, so that the patient could be discharged from the hospital as well as could be expected.—*Medical Times and Gazette, Nov.* 30, 1861, *p.* 564.

139.—ON NÆVUS.

By Thomas Bryant, Esq., Assistant Surgeon to Guy's Hospital.

[Mr. Bryant divides nævi into three classes—cutaneous, subcutaneous, and mixed.]

The distinction between these three classes of nævi is practically of great value, as the treatment must be modified according to each form.

In the purely cutaneous nævus alone are caustics of any value. Nitric acid, or potassa fusa, carefully and freely applied, in such cases are often sufficient to obtain a cure; but if the nævus involves the deeper subcutaneous tissues, it cannot be expected that any benefit will accrue from their application. The acid or caustic acts only upon the part to which it is applied; and as it can only be applied to the surface, it is only upon that that any influence can be exerted. As a consequence, it should be remembered, even in such a simple case as nævus, that a correct practice can be only based upon a correct diagnosis; and that if the diagnosis of this purely cutaneous nævus is mistaken, the practice based upon it is sure to fail.

In the simple and uncomplicated subcutaneous nævus the application of external remedies is palpably inexpedient; and it is in these that the value of the subcutaneous ligature, or of the injection of the perchloride of iron, is often most admirably illustrated. The former I regard as the most certain and most rapid method of cure; the treatment by injection is always uncertain, and not to be relied upon, although in exceptional instances it acts very favourably. It is a practice which I now rarely adopt, as I find that other treatment is more certain in its effects, and more rapid.

When a subcutaneous nævus can be isolated, the subcutaneous ligature should be selected; and when this isolation cannot be carried out, the treatment by seton is most valuable. A few threads passed through different portions of the growth, and left in for a few days (that is, sufficiently long to excite adhesive inflammation), will generally prove sufficient to destroy the nævus; the effused fibrin, by its subsequent contraction, strangulating the minute vessels, and thus allowing the degeneration of the vascular growth.

If one application of many setons fails to effect a cure, a second should be attempted, and a third or fourth should not prove discouraging. Perseverance will generally be followed by success; but a repetition of their application should not be carried out until the immediate effects of the former have passed away.

In the mixed or third variety of nævus—that is, where both the skin and subcutaneous tissue are involved—the application of ligatures to the whole, or excision, where it can be carried out, is the best treatment.

The form of ligature should be adapted to the size and situation of the nævus; it may be tied in one, two, three, or more portions; pins may be used, or otherwise, according to the fancy of the operator, or necessities of the case; the object to be attained is thoroughly and completely to strangulate the growth, and if this is done it is immaterial as to the means—efficiency is the only test.

When the nævus can be isolated, as in those forms which are completely pendulous, excision is the simplest and most efficient remedy; the base may be fixed by a clamp, and on its central side ligatures may be passed so as to include the whole of the base of the tumour; on its distal side excision may be performed. One clean incision being made along the side of the metallic clamp, the actual cautery may then be used to prevent all chance of hemorrhage if any portion of the divided nævus should have been left, and the clamp may then be removed; the ligatures, as previously applied, will maintain the edges of the wound together, and a lineal cicatrix will alone remain. This practice has proved successful in many instances, and when it can be repeated it should be carried out.

In cases in which, from their position, or from their diffused nature, neither form of practice as just indicated can be employed, the treatment by seton is most valuable. It is a practice which of late years has fallen somewhat into disrepute—why, I know not; but, from considerable experience in such a practice, I can most conscientiously advise its re-introduction.

It must not, however, be forgotten that these nævi have a natural tendency to degenerate and to undergo a cure; their rapidity of development and situation should alone lead us to adopt any surgical treatment. If situated in any portion of the body where disfigurement is of no consequence, and the nævus shows no disposition to enlarge, it may with safety be left alone, for it is tolerably certain that as years advance it will degenerate and become a simple and harmless tumour.—*Guy's Hospital Reports, Vol.* vi. 1860, *p.* 70.

140.—ON THE TREATMENT OF NÆVUS.

By Frkd. Le Gros Clark, Esq., Surgeon to St. Thomas's Hospital.

1. The simplest form of the disease, and that with which we are most conversant, from its frequency, is the skin nævus. This is usually congenital, though it may be so small as not to attract attention until its growth leads the mother to seek advice. This form of the morbid growth is generally raised a little above the level of the healthy skin, and of a bright scarlet hue. This is to be distinguished from the "claret marks," which are permanent stains of the skin, and have no tendency to extend. Pressure, in the true skin nævus, disperses the blood for a moment, but the colour immediately re-appears when the pressure is removed. 2. The subcutaneous nævus has its seat in the areolar tissue immediately beneath the skin, and presents a swelling more or less elevated, and of a doughy feel, subsiding also under pressure, but rendered tense and often blue when the little patient cries and struggles. 3. These two forms may be combined originally, more frequently, however, the subcutaneous nævus subsequently involves the skin, as it spreads in different directions. There does not, however, appear to be the same tendency, on the part of the cutaneous nævus, to involve secondarily the areolar tissue: many of these skin affections will radiate over a large surface, without dipping into the deeper textures at all. 4. The mucous surface is subject to conditions similar to those affecting the skin, or, I should rather say, such parts as are near to the orifices, as the lips, tongue, vagina; other textures, as muscles, are also occasionally the seat of this vascular disease; but such tumours are usually supplied by larger vessels, and pulsate like aneurisms. These supplying arteries are convoluted and elongated, and thus aid in forming part of the tumour. Of the same character are, apparently, those tumours which affect, for instance, the orbit, known by the rather indefinite title of "aneurism by anastomosis."

The treatment of nævus necessarily varies according to the form of the disease and the texture in which it resides. I will speak briefly of each, stating the results of my own observation and experience.

First, of *Cutaneous Nævus*. I think there is scarcely any part of the skin which I have not seen affected with this common form of the disease. It is probably most frequent on the head and face; but I have repeatedly seen it on the extremities, and on all parts of the trunk, as well as on the female genital organs; the interior of the mouth and the tongue are also sometimes the seat of this superficial form of vascular growth. The only effective treatment is simple destruction of the diseased tissue; pressure, or any other modified method, is, in my experience, useless; the skin must be destroyed. For this purpose I employ either strong nitric acid or caustic potash; I give the preference to the former, as being more under control; but I occasionally use the latter, when I want to produce a deeper eschar. In applying the nitric acid I use a small glass rod, and provide myself with a piece of blotting-paper, wherewith to absorb any that is superfluous. The acid should be applied about one line beyond the diseased patch, and should be allowed to remain on the part, or be re-applied until the loss of colour, and change to a yellowish hue, assures you that the destruction is complete. A piece of adhesive plaster is the only necessary dressing; and the eschar should be allowed to separate spontaneously. When the potassa fusa is rubbed on a nævus, it is well to be provided with some vinegar, to neutralize its action when you are satisfied that enough is done.

The *Subcutaneous Nævus* may be treated in various ways; and I have repeatedly employed acupuncture, seton, injection, and ligature, besides another method to which I generally give a preference, and of which I will speak presently. Acupuncture is both tedious and uncertain. A broad, lance-shaped needle should be employed, and the healthy skin perforated at least half an inch from the swelling; by repeated thrusts, radiating in different directions, the diseased growth may be thus perforated and cut up; and the benefit to be anticipated is due, in part to the pressure of the extravasated blood, and partly to inflammation. I cannot, however, recommend this method to you, as it is uncertain, and may even irritate the growth, and the bleeding is often very troublesome, even from a small puncture. But, with the addition of a caustic probe, I have found this method invaluable. The proceeding, which was suggested to me by the risk attending injection, is this: I melt some nitrate of silver in a watch-glass, and coat the extremity of a fine probe with it. After cutting up the tumour, and removing the lance-shaped needle, I immediately introduce the caustic probe along the same track, and, moving its point about, thus dissolve the caustic in the interior of the tumour. The advantages are these: You are able to apply and limit your caustic solution to the diseased texture, whereas an injected irritant will often diffuse itself into the healthy areolar tissue. There is no destruction of skin; and the track of the needle being cauterised, there is no risk of bleeding. Moreover, a track is secured for the exit of pus which subsequently forms, and the disease is cured by this inflammatory destruction of its texture. A large subcutaneous nævus may be treated in this way in segments.

Of injection I can speak favourably, except for the risk just alluded to; diluted iodine, or a solution of alum, answer the purpose: but I have entirely neglected this method since I have employed the caustic probe. Vaccination is sometimes effective, but uncertain, and sometimes attended by much inflammation. It is applicable when the nævus is cellular and extensive. A seton answers the purpose in some instances, but I do not recommend it, on account of the continual irritation it produces, and the troublesome bleeding which may follow its introduction. Extirpation of a superficial nævus with the knife I have never employed, for the obvious reason that the attendant hemorrhage would be dangerous; but this method of removal seems to be the only suitable one when the disease is developed in a muscle.

Many instances have occurred to me where it has been requisite to apply various modes of treatment in the same case. As, for example, where the labia and vagina were the seat of the disease, for the cure of which both the

ligature and nitric acid were required; or, again, as in the case already referred to, where the lower eyelid was involved both superficially and subcutaneously, and in which nitric acid and the caustic probe were used effectually, without being followed by any ectropium.

The employment of ligature, in my experience, is both safe and certain; but it usually involves the loss of skin, and leaves a consequent cicatrix. I give preference to this mode of operating where the scalp is the seat of the disease, and, as is usual here, both skin and subcutaneous areolar tissue are involved. The best mode of stangulating a small nævus is to transfix its base with two hair-lip pins, passed through at right angles to each other, and then encircle them with a ligature. I generally puncture the swelling before finally tying the knot, to allow of the escape of the expressed serum. The points of the pins should be then cut off, and strips of adhesive plaster be interposed between them and the skin. Where the diseased growth is very large and pulsating it may be desirable first to dissect up flaps of integument, after making a crucial incision across it; or it may be requisite to tie such tumour with divided ligatures, passing them subcutaneously around it, and including separate segments in different nooses. This is not difficult, and is an effectual mode of dealing with large subcutaneous nævi; but in treating smaller ones, with some exceptions, I now generally employ the caustic probe.

I have never been tempted to tie the supplying arteries of these tumours, and I cannot learn that the experience of others is very encouraging in this respect. Nor has it occurred to me to have a case requiring ligature of a main arterial trunk. This operation has succeeded in some instances, but entirely failed in others.

There is one trivial form of this disease, for such it appears to be, which I have been frequently requested to treat in young persons who are beginning to pay some attention to personal appearance. I mean small crimson spots on the cheeks or nose, where they are very unsightly, and have a tendency to spread. If you cannot discover the supplying vessel in these cases, the best way is to touch the spot with nitric acid. But in some instances you may notice the crimson artery creeping along to its destination. Then you may cut across this vessel, and immediately touch the spot with nitrate of silver. When the tongue is the seat of the disease, nitric acid or caustic potash should be used; I prefer the former. I have seen a few of these cases, and have found them very troublesome, sometimes requiring a ligature. The tongue should be carefully dried before applying the acid, and the nitrate of silver may subsequently be rubbed freely over the surface, whilst still moist with the acid.

The most singularly diffused exemplification of this disease with which I have met was in an out-patient at the hospital some years since, whom I unfortunately lost sight of soon after I first saw him. In this case the limbs and trunk presented a multitude of the scarlet skin nævus, in patches varying in size from a fourpenny piece to half-a-crown. There must have been scores of them.

I would remark, in conclusion, that in treating subcutaneous nævus, as I have described, with the caustic-probe, I encourage suppuration by the application of water-dressing or a bread poultice. The discharge, as I have said, finds its way out along the track of the needle; occasionally, though but rarely, another point of exit is formed. The swelling is slow in subsiding, but the cure is manifested by the altered density of the texture, the loss of any blue tinge the skin may have presented, and the absence of any change in size when the child cries. I have now treated a great many cases in this way, without any untoward occurrence, or the necessity of having recourse to any other expedient to complete the cure.—*Medical Times and Gazette, Jan.* 25, 1862, *p.* 76.

141.—NÆVUS IN THE CENTRE OF THE FOREHEAD OF AN INFANT: TREATMENT BY EXCISION.

Under the care of Mr. GAY, at St. George's Hospital.

An infant, five months old, was brought into the operating-room of this institution on Thursday, the 24th ult., having a nævus in the centre of the forehead, just below the limits of the hair follicles. It was of the size of a small nut, and dipped, from the surface of the skin, above which it was prominent as a deep-red vascular growth, through the integument quite to the pericranium. As the mother had observed a steady increase in the growth, Mr. Gay removed it, and that by excision in the following manner:—He first passed a hare-lip needle through the skin, below the base of the nævus, so that the ends emerged about one-eighth of an inch on each side beyond the extreme edge of the growth. He then removed the whole of the nævus by incision close to its margin, and instantly brought the sides of the wound together by means of a figure-of-8 ligature over the needle, as in cases of hare-lip. As the needle arrested only the bleeding in its immediate neighbourhood, it was necessary to put in two more, one on either side of the first, and to close the wound by ligatures as before. This *completely* stopped the hemorrhage; when the whole was dressed with lint soaked in cold water. A very few drops of blood were lost, the needles were taken away on the fourth day, and the child presented at the hospital on the following Thursday with simply a linear mark indicating the seat of operation. The wound had perfectly healed.

The nævus, on subsequent examination, was found to occupy the cutis in its entire depth, and to make it somewhat thicker than the surrounding healthy skin. It was covered by healthy epidermis, which could be peeled off after maceration; it consequently possessed the normal function of separating healthy epithelium, which in its turn became healthy epidermis. The næval tissue, although defined and distinct in every respect from the cutis in which it was imbedded, could not be separated from it. The one passed into the other, as though but a local modification of its histological elements. These elements consisted, after maceration, in a considerable quantity of yellow, elastic, wavy fibre, convoluted as well as straight capillaries (?), the *débris* of blood-corpuscles, and other granular matter.

Modern surgery approves of the ligature as a means of extirpating these growths. Vaccination is seldom successful, and in Mr. Gay's experience, the other methods of local irritation for the purpose of setting up adhesive inflammation in the tissue have been equally futile. The ligature has been preferred, because excision has been supposed to be dangerous, owing to the hemorrhage attending it. But the ligature is open to two very decided objections. The first is, that the strangulation of these growths, whether the skin is included or not, is followed by severe pain, often constitutional irritation and fever, and (when the growth has been on the head or in the neighbourhood of nervous centres) sometimes death, preceded by convulsions and other symptoms denoting injury to the nervous system. Mr. Gay has seen this happen, and appeals to surgeons for like experience. The second objection arises from the large and tardily-healing wound and resulting cicatrix. And it must be remembered that cicatrices grow, and therefore, on the more exposed parts of the body, this is a very considerable evil. As to extirpation, Mr. Skey says that, "unless wide of the disease, it is necessarily attended with copious hemorrhage, and its objection is obvious." Mr. Fergusson objects "on account of the size of the wound and consequent scar;" and "besides," adds this eminent surgeon, "there are few instances in which it is admissible." Nélaton also objects, on account of the loss of substance involved, the size of the cicatrix, and the hemorrhage, owing to which, he says: "Plusieurs enfans ont même succombé entre les mains des operateurs." (Elemens de Pathologie Chirurgicale.) The American surgeons, Drs. Physutt, Reese, Mott, and Jamieson, have extirpated them; but they tie any bleeding vessels, and fill up the wound with charpie, so that it

is left to granulate. The size of the cicatrices is a a grave objection to this mode of treatment subsequent to the operation.

There can be no reasonable doubt that a clean-cut wound is better in every respect than the same extent of wound made by ligature, to say nothing of the process of sloughing, and the chance of having to repeat the ligature to some part of the growth in case of the first proving partially abortive, as it sometimes does; and that the only remaining objection to excision is the hemorrhage. In both a loss of tissue is unavoidable.

To avoid the hemorrhage, and at the same time all but the smallest and most inconsiderable scar, Mr. Gay is now in the habit of treating these cases in the way just described, and has found no inconvenience to follow its adoption. The number of needles by which the tissues below the base of the nævus are to be transfixed must depend upon the size of the growth; and should hemorrhage from any point follow the closing of the wound upon the needles already introduced, one or more may be added afterwards, and thus the blood can be completely staunched. The needles should be inserted at a right angle to the long axis of the tumour, for obvious reasons, and the incisions should come to a point at the extremities. The skin should be cut with an exceedingly sharp knife, and the cut edges be clean. In this way *immediate union of the edges* will be secured, and scarcely a mark will remain.

This mode of treatment is most suitable for infants, before the nævus has extended itself by growth, and whilst the skin is elastic, and admits of adaptation by stretching. Perhaps the larger nævi of the scalp may not admit of it. As yet Mr. Gay has had slight experience to warrant his recommending it. He, however, strongly recommends the extension of the excision method, as far as it can be practically carried; and for the larger nævi, or those situated in skin that is not sufficiently extensile to admit of filling up the wound made in their removal, some modification might be suggested that will meet the difficulty, and give in the end the same advantages over the ligature.—*Lancet, Dec.* 14, 1861, *p.* 568.

142.—ON THE REMOVAL OF NÆVI.

By J. C. Christophers, Esq., St. John's Wood.

[A variety of operations have been proposed and adopted for the removal of nævi occurring in situations where hemorrhage is difficult to control and where disfigurement is sedulously to be avoided. Few achieve all that should be sought.]

The operation is very simple, requiring only a needle and some waxed silk. It is easily and quickly performed as follows. A double ligature is passed underneath the centre of the nævus. Divide the ligature; pass one half of it through the skin above the part required to be removed; pass the other half through the skin below the part required to be removed. Tightly tie the two ends of the loop including the inferior half of the nævus; tightly tie the two ends of the loop including the superior half of the tumour. The four ends of the ligature are now tightly and alternately tied the one to the other, and the operation is completed, the whole nævus being effectually strangulated by being entirely enclosed in a double circle by the ligature both from within and without.

The advantages claimed for this operation are—that the risk of hemorrhage is avoided by it, and that neither needles nor apparatus remain to annoy the infant, the mother, or the nurse; that the pain entailed by its performance is not severe, that it appears to cease with the operation, and that further surgical interference is not needed to perfect the cure; that it leaves less deformity or scar than the operations by excision, needles, or caustics, as the sound skin surrounding the nævus is made by the tightened ligature to take its place, and is there held as by two points of suture; that it is applicable to all cases; that it can be used in situations where other methods cannot be applied; and that it is suited to nævus of all forms, whether they be

prominent or flat, superficial or deep, extensive or circumscribed.—*Lancet*, *April* 5, 1862, *p.* 366.

143.—ON THE STATE OF THE PUPIL IN HYDROCEPHALUS.

By Dr. S. WILKS, Assistant Surgeon to Guy's Hospital.

The state of the *pupil* was remarkable in being contracted, for dilatation is the rule, especially if there be much effusion in the ventricles. It is highly probable that the condition of the pupils depends upon accidental circumstances, as, for example, what part of the brain is especially affected in any individual case. The rule in hydrocephalus is a sluggishness of the pupil at the early period of the disease, and a dilatation towards the close; how far this depends on implication of those parts of the brain whence the optic nerve has it source, or how far on affection of the nerve itself from surrounding inflammation, is difficult to say. Contraction of pupil in cerebral affection is generally, we believe, observed in those very severe forms of apoplexy where a large quantity of blood is poured out into the ventricles and runs down to the base; also in those cases where a sanguineous effusion has occurred in the substance of the pons varolii. It is clear from this, that the pupils are variously affected as different parts of the brain are involved. As showing how slight a matter will influence the size of the pupils, we may mention a case which we witnessed some years ago; it was that of a man who had long been bedridden with softening of the brain and large ventricular effusion; on our first visit to him in this state, we made a note of the fact of one pupil being larger than the other; on a second visit, the pupil which had been the smaller was now the larger, and *vice versa*. It was then remarked that he was lying on different sides on these two several occasions, and that it was in each instance on the lower side that the pupil was dilated. We requested that before our next visit he should be placed on the side on which he lay when first seen; this was done, and it was then found that a change had again occurred, and the same pupil was dilated as in the first instance. So also on subsequent occasions, which enabled us to prove that the gravity of the fluid in the ventricles was sufficient to influence those parts of the brain whence the optic nerve had its source.—*Guy's Hospital Reports*, *Vol.* vi., 1860, *p.* 104.

144.—ON THE TREATMENT OF DIARRHŒA IN CHILDREN.

By Dr. WILKS, Physician to the Royal Infirmary for Children.

[Most cases are cured by a little castor-oil, or tincture of rhubarb, or some slight astringent mixture. If, however, the case is protracted the best remedy is hæmatoxylon.]

The mixture Dr. Wilks uses is composed of extract of hematoxylon, ipecacuanha wine, and tincture of opium, made up with the ordinary chalk mixture. This is a very capital mixture where ulceration of the bowel exists, as in phthisis. Dr. Wilks's favourite remedy, however, is a mineral acid. Sulphuric acid he has often found successful, but also disappointing, and has thus preferred a mixture which has been spoken of lately as a new discovery. —nitric acid with tincture of opium. This may be found recommended in the first edition of Maunsell and Evanson's work on "The Diseases of Children," published many years ago, and in which the authors speak of it as a well-known remedy, and used by the late Dr. Baillie in combination with infusion of simarouba. The re-discovery (if the term may be used) is a sufficient proof of the value of the remedy. Dr. Wilks finds it, above all others, useful in those chronic cases which come to the Infirmary. Should the case put on a dysenteric character, he adds some vinum ipecacuanha. Indeed, in very long-standing cases, the acid with the ipecacuanha will often effect a cure when opium and all other remedies have failed. It is a remarka-

ble fact, that although ipecacuanha seems to have as specific an effect on the intestine as any remedy which is deserving of the name, and was actually introduced into Paris two centuries ago under the name of *Radix antidysenterica*, yet many medical men do not employ this drug for gastric or intestinal complaints; and thus for years we have heard of the treatment of dysenteries by various remedies, to the exclusion of this one. Of late, however, it has again been used in India with its old and wonted success.

It should always be remembered that "diarrhœa" is a general term, denoting merely looseness of the bowels; and it is for the medical man to determine on what cause this may depend, as a great variety may exist. The remarks here, however, apply to those cases especially amongst children where we know, from examination after death, that no very marked organic change has occurred, and therefore the simple term "diarrhœa" must be employed. In some of these cases where the nitric acid has failed, a mixture containing nitrate of silver has been given with success. This is a formula introduced into the Pharmacopœia of the Infirmary by Dr. Willshire, who has used it for years with much success. The dose is one-sixteenth of a grain in ʒj. of water.

There is extreme difficulty in arriving at a conclusion as to the relative merit of the ordinary medicines for diarrhœa, as it often happens that two cases may come to the institution on the same day apparently alike in every respect, and to which the same medicine is given; and yet one is soon relieved, whilst the other patient receives no benefit. Then, by employing another medicine, a cure is immediately effected. Again, if in a third case this same medicine be given, the diarrhœa will continue until the remedy, which was unsuccessful in the previous case, be now administered. It is remarkable how often a change of medicine of this kind appears necessary, and what is beneficial for one case is useless in another. It may be, that two cases which appear exactly alike, are not absolutely so, but require different means from the onset. And what is more probable is, that in one case the original cause of the disease may have been long removed, although a chronic morbid condition of the mucous membrane may remain, whilst in another the irritating cause, whether that is improper food, water, or air, may still be present. The cause being persistent, the disease is so also, in spite of the counteracting influence of the medicines. All such causes may be removed in the case of a private patient, and a correct conclusion be arrived at regarding the value of any particular medicine, but amongst the poor, who flock to our charitable institutions, although an opportunity is offered of comparing together large numbers, yet the particular circumstances of each cannot be so well known.

Dr. Wilks also states that amongst the children patients, a large number come under the class of atrophy, a condition generally ensuing on weaning, or occurring before this period if the mother's milk be deficient or bad, or from other causes. In these miserable infants the limbs waste, the belly grows big, the chest falls in, and the head often enlarges. There may be difficulty of breathing, rolling of head, diarrhœa, &c., and, according as any of these is prominent, so the case may be treated by directing the efforts against one of these maladies. In such cases it is often found far better than treating the diarrhœa, to put the child on cod-liver oil and steel wine, when, as the general condition improves, the individual symptoms cease.—*Med. Times and Gazette, March* 8, 1862, *p.* 238.

MISCELLANEOUS SUBJECTS.

145.—ON THE DISCRIMINATION OF URINARY DEPOSITS BY THE UNAIDED SENSES.

By Dr. WILLIAM ROBERTS, Physician to the Manchester Royal Infirmary.

[The more exact methods of clinical research require both time and opportunity, and it is most desirable, especially in hospital practice, to possess a means of gauging the urine that shall be expeditious, yet sufficiently trustworthy to be relied on with certainty. Exactitude is by no means to be underrated, yet the use of the microscope and test tube require an amount of time not always to be spared for their employment. We believe that the amount of information concerning a urine, which may be obtained by the unaided senses of smell and sight, far exceeds both in precision and extent what is usually supposed.]

The most certain knowledge is to be obtained concerning the amorphous urate deposit, *uric acid*, *oxalate of lime*, the *triple phosphate*, and certain colouring and odorous principles derived from certain articles of food, as asparagus, or from certain medicines, as rhubarb, turpentine, and copaiba. The whitish and greyish amorphous deposits are more difficult and often impossible to distinguish by simple sight and smell; but a very shrewd guess can usually be given as to the presence or absence of blood and pus, and the amorphous phosphate of lime; also as to whether the urine is concentrated or dilute, fresh or stale, pure and clean, or dirty from extraneous impurity.

The information conveyed by the sense of smell is highly important. The reaction of the urine can be told thereby, in the greater number of cases, with ease and certainty. If the urine have the usual faint urinous odour, it is always acid; but if the secretion have an ammoniacal odour, its reaction is necessarily alkaline from carbonate of ammonia. And not only is the volatile alkali thus detectible by the odour, but even alkalescence from fixed alkali can be recognised by the same means. As alkalescence from these two causes have a widely different significance, it is very important to discriminate the one from the other. A urine alkaline from fixed alkali is quite devoid of the ordinary urinous smell; it exhales a peculiar sweetish, aromatic odour, resembling the fresh urine of the horse or ox, which may be artificially imitated by adding liquor potassæ to ordinary urine.

A knowledge of the reaction of the urine is an important and often decisive step towards the recognition of a urinary deposit. Before proceeding to indicate the special marks by which various deposits may be identified, I must remind you that urinary deposits fall into two well-marked natural divisions—namely, (1) *true precipitates*, and (2) *sediments*. The first division embraces those substances which normally exist in urine in the dissolved state, and become precipitated, and consequently clinically recognisable, under certain contingencies of quantity, temperature, reaction, and concentration, which it is not my purpose now to examine: their *presence* in the urine is natural, it is only their *precipitation* that is unnatural. The second division includes all those organic forms and substances—epithelium, pus, blood, casts of tubes, oily globules, spermatozoa, confervoid growths, and extraneous impurities—of which the *presence* in the urine is abnormal or accidental; and which, whenever they are present, form a deposit of necessity, from their total insolubility in the urine under any circumstances. The

members of the former group can be, in the majority of cases, readily distinguished by the unaided senses by their mode of precipitation or crystallization; whereas the members of the second group appear amorphous to the unaided sight, and cannot thereby be distinguished from each other. Blood is exceptional to this rule in virtue of its red colour; pus likewise, when the urine is alkaline, may be identified by the tenacious stringy mass which it forms, and which passes commonly under the erroneous name of muco pus. Epithelium, casts of tubes, spermatozoa, together with pus in acid urine, are altogether unrecognisable without the aid of the microscope. I dismiss them accordingly from further consideration at present, and return to the first group, or *precipitates*.

Most of these are not deposited until after emission, and the manner of their deposition is often so distinctive that it suffices for their identification. It is consequently necessary to collect the urine in a glass vessel immediately after emission, in order that their mode of deposition may be correctly observed.

Uric acid is recognised by its occurrence in crystals, the crystals being nearly always of such a size that the crystalline character can be recognised with the naked eye, by the *orange* or *brown* colour of these, and by the invariably acid character of the urine. Generally the brown crystalline specks are seen studding the sides of the glass, or dotting the mucous cloud at the bottom, or there may be coarse brownish streaks and bands on the sides of the glass, with a floating film on the top; more rarely uric acid forms a distinct layer of deposit. It is only when the crystals are very small, that a difficulty occurs with uric acid. In albuminous urines uric acid sometimes forms very minute crystals, wholly devoid of crystalline appearance to the naked eye. These form a reddish deposit, much resembling the amorphous urate, but their greater density usually suffices to distinguish them to a practised eye.

The *amorphous urate* is easily recognised by its soft, cloudy, uncrystalline appearance. The urates, like uric acid, have a strong affinity for the colouring matter of the urine, and become tinted by it; the pink, red, and orange urates, by being more deeply coloured than the urine from which they fall; but occasionally the urates occur of a very pale fawn, and may then be mistaken for the amorphous phosphate of lime. There are two conditions which, however, afford a ready means of escaping this error. The first is, that the urate is always associated with an acid state of the urine, whereas the phosphatic deposit is associated with an alkaline urine; and secondly, the amorphous urates leave a peach-like bloom on the surface of the fluid and on the sides of the glass, which is quite peculiar, and when the glass is inclined from side to side, this film, or bloom, comes into view, and identifies the amorphous urate with the utmost certainty.

Oxalate of lime.—A deposit of oxalate of lime is nearly always associated with an *acid* urine; it is sometimes, however, present in a feebly alkaline urine, but, never, so far as I know, in a *highly* alkaline urine. Oxalate of lime always occurs in very minute crystals, and it is usually represented as a deposit inappreciable to the naked eye, and requiring the microscope for its detection. It is not so, however; indeed, so far is this from being the case, that, using the precautions already mentioned, oxalate of lime is the most easily and certainly identified of all urinary deposits by the unaided sight. This arises from the peculiar manner in which it is deposited on the sides and bottom of the glass. The sides of the glass are seen to be covered with innumerable exceedingly delicate striæ, running in bands, with a transverse or oblique direction, giving an appearance as if the glass was finely scratched on its inner surface. At the bottom of the glass you will find a still more characteristic appearance. You see a soft, pale grey mucous-looking settling, surmounted by a snow-white undulating coverlid, with a well defined surface. One or other of these appearances is quite characteristic of oxalate of lime. The scratched appearance of the sides is due simply to the deposition of an infinite number of very minute crystals along the invisible lines left after toweling the glass. The white coverlid over the

soft grey cloud is composed of pure oxalate of lime; a little of this put under the microscope will disclose myriads of minute octohedra. Let me, however, caution you against confounding the streaks produced by uric acid with those due to oxalate of lime. The former may be distinguished by the scratches being coarser, and likewise tinted more or less deeply of a brown colour; and, again, with uric acid there is absence of the coverlid appearance at the bottom of the glass, which is peculiar to oxalate of lime.

The unmixed *amorphous phosphate of lime* is associated with an alkaline state of the urine from fixed alkali. It is of a greyish-white soft appearance, less coloured than the urine from which it has been deposited, very light, and sinking slowly to the bottom. It leaves the sides of the glass perfectly clean, and when fully settled has a broken irregular surface. There is likewise frequently an iridescent film on the top which is not composed of distinct crystals.

The *crystalline phosphate of lime* forms a dense snow-white deposit, usually in alkaline (not ammoniacal) urine; occasionally, however, the urine is neutral or even faintly acid, but never sharply acid.

Neither of these deposits is capable of being identified with certainty, unless the alkalescence of the urine be well marked; the former is liable to be confounded with other amorphous deposits, and the latter with the ammoniaco-magnesian phosphate.

The *triple phosphate of ammonia and magnesia* is usually readily recognised by its large, brilliant, colourless crystals, and by the ammoniacal smell of the urine. The crystals either dot the sides of the vessel and the deposit at the bottom, or form a glancing film like burnished silver, on the top, or both. Very often the amount of ammonia present is too small for detection by the smell, and then the crystals must be discriminated by their own characteristics.

A freely ammoniacal urine has always a deposit; and it is composed of a mixture of the amorphous phosphate of lime and the ammoniaco-magnesian phosphate. A urine alkaline from fixed alkali has generally a deposit, but not always; sometimes it is perfectly clear, and this arises, as I believe, from the occasionally very scanty proportion of lime existing in the urine.—*Lancet, May* 10, 1862, *p.* 481.

146.—ON ARTERIAL MURMURS IN INCIPIENT PHTHISIS.

By Dr. W. S. Kirkes, Assistant Physician to St. Bartholomew's Hospital.

[The following paper was originally read before a private society for the study of chest diseases, in April, 1858. Subsequent observations have convinced the author more and more of the value of this sign in many doubtful cases of incipient tubercular disease of the lungs.]

The fact that a murmur, or *bruit*, coincident with the heart's systole, but independent of any cardiac disease, may, in many cases of incipient phthisis, be heard in one or other subclavian region, has long been known. Dr. Ogier Ward, I believe, was the first publicly to direct attention to this fact; at least, I have not met with any earlier allusion to it than that in a paper communicated by Dr. Ward to the Medical Gazette about twenty years ago. Therein he speaks of "a peculiar murmur heard in certain parts of the chest of persons presenting symptoms of phthisis," and he says "it occurs in such parts of the chest as percussion or auscultation would indicate as the seat of crude tubercles." Dr. Latham, however, was probably then, if not long before, familiar with the murmur in question, for in his Clinical Lectures on Diseases of the Heart, delivered many years ago at St. Bartholomew's Hospital, he speaks very strongly about it, though at the same time it must be observed that his remarks only apply to a murmur heard in the situation of the pulmonary artery, and not high up in the subclavian region. He says, "Fancy a line drawn from the left side of the sternum along the upper edge of the second costal cartilage, and continued an inch along the second rib;

and another line drawn from the sternum along the lower edge of the third costal cartilage, and continued an inch along the third rib. Between these two lines a space is included, in the whole or in part of which a murmur is often audible coincident with the systole of the heart, when no such murmur can be perceived either in the præcordial region, or in the course of the aorta, or in the carotids, or in any part of the arterial system, but here, and here only. It is a gentle bellows-murmur, quite obvious to the ear, and unmistakeable in its character. Of such a murmur, often audible in this situation exclusively, I am certain as a matter of fact, and certain, too, of its very remarkable accompaniments. I have witnessed it in those who were undeniably consumptive, or in those who were too justly suspected of being so. I cannot say in what proportion of the phthisical it occurs; but I am continually meeting with it." It is clear, then, that Dr. Latham was quite familiar with these arterial murmurs, and that he attached some value to them as a diagnostic sign of phthisis. Since this account was published, however, very little has been written on the subject, casual allusion to it in various works on diseases of the chest being nearly all I have met with. I think, however, that the subject deserves further examination, especially since the murmur in question is frequently attendant upon incipient phthisis, and also since there is often so much obscurity in the other physical signs of early tubercular deposit in the lungs—a little feebleness or coarseness of the vesicular murmur, slightly prolonged expiration, and a somewhat diminished resonance on percussion being nearly the only manifestations of incipient phthisis; and even these are often of very doubtful nature, and rarely exist in a marked degree until a considerable amount of deposit has taken place. Any additional physical sign, therefore, by which the diagnosis of incipient phthisis may be facilitated cannot fail to be of value, especially if it be one easy of detection, such as a well-marked arterial *bruit*.

It may be as well to mention some of the more striking peculiarities by which the murmur in question is distinguished from most other forms of murmur; several of these peculiarities are noted in the extract just given from Dr. Latham's work. Other additional points I have noticed myself.

1. First, it may be repeated that the murmur is independent of a murmur in the region of the heart; this is an important point, for if a cardiac murmur existed, its transmission might, of course, be supposed to explain the one heard in the subclavian region; but again and again I have found the heart's sounds quite healthy when the subclavian murmur was well marked. The conclusion arrived at, therefore, must be that the murmur has its origin at or about the part where it is heard, and not in the heart.

2. Next, as to the situation of the murmur. Dr. Latham speaks of it only as occurring in the neighbourhood of the main trunk of the pulmonary artery, and therefore limited to the left side of the chest, and to that part of the left side where the pulmonary artery is situated, namely, the junction of the second and third costal cartilages and their intervening space to the sternum. But according to my own observation, it occurs even more commonly, much higher than this situation, close under the clavicle, especially towards the humeral end of the bone; and, moreover, it occurs nearly, if not quite as frequently, on the right as on the left side of the chest; in fact, it occurs on that side and in that situation where we often have reason to suspect the existence of tubercular deposit.

3. Then there are certain peculiarities in the murmur itself. Its intensity varies greatly, ranging from the faintest whispering bellows-murmur to a loud harsh roaring sound. On one occasion, a man with symptoms of phthisis presented such a loud and rasping murmur in one of the subclavian regions that I suspected he had an aneurism there; but on examining him again, a few days afterwards, the murmur had almost completely disappeared; and subsequent observation of the case proved that it was simply one of the class we are considering. This variableness in intensity of the murmur even in the same patient, is another striking peculiarity which it often presents. Even while listening, a murmur which was harsh at first will often gradually become fainter, and may even completely disappear for a while. So fugacious indeed

is the murmur sometimes, that I have frequently known it disappear and reappear several times while a patient has been under examination. It may often be noticed, too, that the murmur is much influenced by the respiratory movements; its intensity being often greatest at the end of full inspiration, or just at the turn when expiration begins. Sometimes, indeed, the murmur is heard only at that time, disappearing completely during expiration, and only occurring again at the end of the next full inspiration. Its intensity is influenced, too, as is that of most other systolic murmurs, by the degree of vigour with which the heart is acting; being loudest when the heart is contracting vigorously, as when a patient first enters the room, or is somewhat excited, and becomes fainter as the temporary excitement subsides.

Such are some of the chief peculiarities of the murmur. Then comes the question, "To what cause is the murmur due?" This is the most important part of the inquiry: and I confess there is much difficulty about it; but the following remarks may probably help towards a satisfactory solution of the difficulty:—

1. Is the murmur simply anæmic? *i. e.*, brought about by an impoverished state of the blood, and a corresponding weak state of the vessels. I do not think it is; for if it were so it would most probably be heard at the base of the heart, and along the aorta, and in most of the main branches of the thoracic aorta; but, as observed, the subclavian murmur is usually independent of any cardiac or other murmur, and, moreover, it is commonly heard in one subclavian region and not in the other, a fact quite inconsistent with the supposition of an anæmic orgin. Again, if it was purely anæmic it ought to be heard in all cases of anæmia; and it ought not to occur in cases which are not anæmic; but neither of these results is found to happen; for it is rare to meet with the murmur in anæmic cases independent of tubercular deposit, while it is frequently heard in suspected phthisical patients before any marked signs of anæmia are developed. Again, too, if it were anæmic it ought to be constant, or nearly so, like other arterial anæmic murmurs, whereas it is, as I have said, often most fugacious—fading, disappearing, and reappearing again and again even during the same examination.

2. This variableness in its intensity, and even in its existence, naturally suggests that the murmur is due to some cause which is not always in operation, but only exercises a temporary influence. Now, nothing is so likely to be of this nature as pressure; for we can readily understand that with the continual changes taking place in the respiratory and circulatory movements within the chest, arterial and other canals, with their contents, will be continually exposed to varying degrees of pressure. Some examination of this as the probable cause of the murmur is necessary.

It is, perhaps, scarcely requisite to call to mind that simple pressure on a given part of an artery is sufficient to cause a murmur, by accelerating the current at the part compressed, and thus increasing the force with which the particles of blood come into collision with those in the succeeding wide part of the canal. Dr. Latham pointed out this fact very clearly, showing that in children with yielding thoracic parietes it is easy to produce a murmur in the pulmonary artery by exercising a little extra pressure with the stethoscope at or about the second left intercostal space near the sternum; and Dr. Jenner has since confirmed this observation by several additional examples. It being well-determined, then, that artificial pressure on an artery, by narrowing its calibre at a given part, may give rise to a murmur, we can readily understand that a like result—namely, an arterial murmur—may ensue when a large arterial trunk is compressed by any solid material such as cancerous or tubercular matter deposited in parts contiguous to it. In order to the production of such a murmur, it is not perhaps enough that the parts surrounding the artery, say the tissue of the lung, should be simply consolidated, but probably they must be so affected as actually to press upon and narrow the artery at some point; as, for example, when a deposit ensues in such a situation as to project above the general level, and this in the immediate proximity of an arterial trunk.

That arterial murmurs in different parts of the thorax may thus be the re-

sult of pressure exercised by a tubercular lung, is demonstrated by an example mentioned in Dr. Hope's great work "On Diseases of the Heart." "I had a patient," Dr. Hope says, "in the St. Marylebone Infirmary, in whom I, as well as the apothecary, Mr. Hutchinson, noticed a distinct murmur along the ascending aorta on some occasions, and not the slightest in others. I was much perplexed, and could not make up my mind as to the existence of valvular or aortic disease. The patient died of phthisis; and on post-mortem examination it was found that the anterior edge of the left lung, completely indurated by tubercular deposition, pressed so exactly on the ascending aorta as actually to have taken its mould, though without adhering. It was now recollected that the murmur had always been heard when she lay on her back or inclined to the right side, but not when inclined to the left; hence we ascribed it to pressure of the lung on the aorta when the position of the body caused it to gravitate towards the right side."—(Third edition, p. 391.) There can be little doubt that Dr. Hope's interpretation was correct; and it clearly suggests to us that tubercular and such-like deposits in other parts may, when in close proximity to a larger artery, compress it and so give rise to a murmur.

I incline, then, to believe that in the majority of cases in which this subclavian murmur occurs in incipient phthisis, it is due to unwonted pressure exerted on some large arterial trunk, diminishing its calibre at the compressed part. The cause of pressure is, no doubt, the tubercular matter deposited in the pulmonary tissue. But then the inquiry occurs, What artery is compressed? This, no doubt, will vary according to the seat of the tubercular deposit. If it occurs below the apex of the lung or near the situation of the pulmonary artery, this vessel may be the one compressed and the one which gives rise to the murmur, as so often noticed by Dr. Latham. The same artery, too, may be compressed and probably generate a murmur in cases where neighbouring bronchial glands are enlarged and filled with tubercular matter. But when the deposit occurs, as it usually does, in or closely adjacent to the very apex of the lung, the arterial trunk most likely to be compressed is the subclavian. This artery lies upon, and actually indents the apex of the lung for a distance of an inch and a-half or two inches on the left side, rather less on the right. Now, when the apex of the lung is consolidated by tubercular deposit, we can readily understand that it will tend to exercise a greater degree of pressure on the subjacent artery than would healthy vesicular structure, and would therefore be likely to diminish the calibre of the vessel at that part, and thus give origin to a murmur. This supposition is strongly confirmed by the fact that the murmur is usually loudest at the end of full inspiration; for it is intelligible that when the vesicular structure of the neighbouring portion of lung is fully distended with air, the consolidated portion will exercise a greater amount of pressure on the adjacent parts than when the vesicular structure is comparatively empty. The subclavian artery then, I would suggest, is probably the usual source of the murmur so frequently heard when there is reason to suspect tubercular deposit in or about the apex of the lung; and when heard it may generally be held to indicate that the tubercular deposit is exercising pressure on the vessel. But although the subclavian artery is probably the most common seat of the murmur, yet the aorta, the innominate, and the carotids are all in immediate proximity to the portions of lung most usually the seat of tubercular deposit, and may therefore be compressed by it, and give rise to a murmur. There is, of course, another view which may be taken as to the cause of the murmur; viz., that it may be the result not of pressure on a main artery, but simply of impediment to the transit of blood through the consolidated pulmonary tissue, whereby the pulmonary artery and its main branches become obstructed. But I cannot clearly understand in what way such impediment can give rise to a murmur presenting the characters already mentioned, and I am therefore inclined to ascribe very little influence to this condition.

With regard to the diagnostic value of the murmur, I may add that although its existence may be regarded as a strong additional sign of tubercular deposit, yet its absence must by no means be held as negativing the existence of such

disease, for it is quite intelligible that even a considerable amount of deposit may exist without being so situated as to exercise sufficient pressure on any large artery to produce a murmur. Then, again, I would repeat that so far as I have been able to determine, the murmur is an attendant on the *earliest* stage of phthisis, that namely of tubercular deposition; and on this fact its chief diagnostic value depends, for in the stage of softening the physical signs of the disease are too obvious to need any additional confirmation. The presence of the murmur in the earliest stage, and its absence in the stage of softening, are quite intelligible on the view that pressure by a consolidated lung is the cause of the sound, for it is in the first stage *especially* that this condition is likely to exist, the lung in the later stages being more or less softened, broken down, and hollowed into vomicæ. Moreover, in the later stages the total amount of blood is diminished from deficient nutrition, and the muscular power of the heart is also lessened, so that the blood is propelled with less force.

There is just one other point which I may mention; care should be taken when listening towards the humeral end of the clavicle not to press too hard on the subclavian artery in its course outside the chest, for in some cases I believe I have by such pressure induced a murmur which did not previously exist; and this is a caution which should be especially observed in thin and anæmic subjects.—*Med. Times and Gazette, May* 17, 1862, *p.* 503.

147.—NEW FRACTURE APPARATUS.

By C. Evans, Esq., House-Surgeon to Birkenhead Hospital.

[The patient was admitted with severe injury of the right arm, consisting of a compound comminuted fracture into the shoulder-joint, a comminuted fracture at the middle of the humerus, a compound fracture into the elbow-joint, and a simple fracture of the radius. The lesion of the soft parts was most extensive.]

At a consultation of the surgical officers it was proposed to remove the arm at the shoulder-joint. Fully admitting the seriousnss of the case, I begged to be allowed to try first the effect of my new splint for a few days, believing that it presented advantages which could not be secured by any of the usual appliances. I am happy to say that my anticipations were fully realised, and at the end of four months the lad left the hospital with a thoroughly useful limb. I saw this lad after the lapse of twelve months, when it was difficult to detect any marked difference between his two arms.

Since the above case occurred, I have used my splint in four other instances, where, though there was not the same amount of injury, unusual mischief existed as well in the shoulder and elbow joints as elsewhere in the limbs, with equally satisfactory results.

The splint to which I have referred consists of a bar of iron one inch wide and an eighth of an inch thick, bent at the shoulder and elbow joints, and armed with five carefully covered pads, two of those being fixed, one at each extremity of the bar, and three of them capable of sliding into various positions. The upper or shoulder pad, with a basis of flat metal, is so bent as to fit accurately on the shoulder, extending over the scapula and clavicle, and arresting, by the assistance of a strap passing through the opposite axilla and buckled by its ends to the shoulder pad, their movements. The other fixed pad is intended, by being secured round the wrist, to prevent all shifting of the instrument or motion in the injured limb. The other three pads are made moveable—first to facilitate the dressing of the wounds where these exist, and secondly to assist the close apposition of the limb to the splint. So far as my observation has extended, I know of no form of injury to either the shoulder or arm, including fracture of the clavicle, to which it is not better suited than any form of apparatus that has been hitherto employed.—*Lancet, May* 10, 1862, *p.* 486.

148.—ON THE SUBSTITUTION OF IRON WIRE FOR THREAD AND SILK AS LIGATURES FOR ARTERIES.

By Thomas Nunneley, Esq., Leeds.

[The fine wire introduced by Dr. Simpson "is not applicable as a ligature for vessels or other small objects, as it is not sufficiently flexible to allow of its being drawn home in a knot, so as either to cut equally and efficiently through the inner coats or to certainly close the bleeding orifice without fear of its slipping off. Hence, probably, Dr. Simpson's idea of employing lateral pressure by a steel pin—acupressure, as he named the practice—for closing an open artery. Though this method has been tried in some cases with success, it is not one which either has or is likely to obtain general employment, as few men will dare to leave a divided large artery so apparently insecure, and presenting so little of the conditions shown by research and practice to be essential for permanent closure." For some time past the writer has been endeavouring to procure wire sufficiently fine, strong, and flexible to admit of its being used instead of thread ligatures. He has succeeded in getting some which possesses these qualities.]

There are two thicknesses: one is probably about No. 42 or 43, but as I have not seen any gauge finer than No. 40, I am unable to say positively its exact number; the thicker portion is No. 37. The finer is adapted for vessels smaller than the brachial artery, and might possibly be used efficiently for larger; but for these I should rather employ the thicker wire, since it is considerably stronger than the other, and yet it is sufficiently fine and flexible to admit of being drawn home in a knot. Both will be found to cut well through the inner coats of a vessel, leaving the outer one entire, upon which the wire holds well. In using the wire ligature too much force must not be employed, or it will cut itself out; and some little care in using a steady, equable pull is necessary, particularly when securing the knot, for by a sudden jerk the wire is apt to give way at or near the second twist of the knot. This should be avoided; for though I have found in many experiments upon the dead subject, when I have purposely broken the wire in this manner, that the knot even then commonly is fast, still it would not be so secure as if both ends were entire. One end of the wire may be cut off in the usual way, or both may be cut close off, as has been recommended and practised by Dr. Sims with silver wire. The former plan I prefer; for in the latter, contrary to what has been asserted, the wire has not uniformly been found to remain quiescent, or without exciting irritation and suppuration. With the view of rendering the iron wire still more flexible, I have had it annealed, by which it becomes almost as flexible as thread, but this is at the expense of its strength, so much so that though No. 37 retains sufficient tenacity to be employed, the finer is rendered so weak as to become useless. Whether by a more careful annealing this may be prevented remains to be ascertained. Though it is not necessary, I would remark, that the least touch of oil at the part where the knot is to be drawn will facilitate the running of the wire, and also that a tolerably long piece should be used, so that it may be passed over the fingers; otherwise a steady, firm pull is not so easily obtained, for the wire is so smooth that it is apt to slip over them as the knot is drawn. For sutures about the eyelids, where the skin is thin, or about the face or head, where it is of importance to avoid all irritation, or on the face, where marks are objectionable, the fine wire will be useful, for it is so small as to leave hardly a trace of its traject. I need hardly remark, that the wire ligature will be somewhat longer in coming away than an ordinary thread one.—*Lancet, May* 10, 1862, *p.* 486.

149.—CHLOROFORM AND CHLOROFORM JULEP.

By Dr. Thomas Skinner, Physician to the Liverpool Dispensaries.

There are two physical properties possessed by chloroform, which, so far as I am aware, are not alluded to in any of our text books on materia medica;

namely—1. Its solubility in alcohol, and subsequent miscibility in water; 2. Its miscibility, if not its solubility in water.

Lately, while engaged experimenting in order to ascertain the probable composition of Davenport's chlorodyne, I accidentally discovered the following valuable facts.

1. If chloroform be dissolved in rectified spirits of wine, of specific gravity ·838, at 60° Fahr. (L. P.), in the proportion of from one to sixteen minims of chloroform in a fluid-ounce, the resulting liquid is entirely and freely miscible with water in all proportions. On adding minim by minim of chloroform to the measure of thirty or thirty-two minims to the ounce of the mixture, the solution ceases to be miscible with water in any proportion; the chloroform spontaneously precipitating in small globules, which ultimately coalesce.

2. If chloroform be added to distilled or any good drinking water, in the proportion of half a fluid-drachm to a pint (twenty fluid-ounces), and briskly agitated, the resulting liquid is perfectly clear and bright, and no globules of chloroform are precipitated, nor can any be detected with a miscroscope by a power equal to 250 diameters. On adding more chloroform *gradatim*, the point of saturation is not arrived at until the proportions are a fluid-drachm to a pint of water. Sixty-four minims to the pint render the mixture quite opalescent, and much of the chloroform is precipitated. At the point of saturation, a drop of any essential oil shaken with the mixture will determine the separation of the chloroform. If the whole fluid-drachm of chloroform be added *at once* to a pint of water, and shaken, the resulting liquid will not be so clear as when it is added gradually.

Bearing these interesting facts in mind, I resolved to put them to some practical use; and I beg to suggest the propriety of there being two new officinal preparations of chloroform, the names and formulæ for which shall be as follow:—

Spiritus Formyli Terchloridi (commonly called chloric ether):—Chloroform, ℥ v; rectified spirit of wine, sp. gr. ·838 (L. P.), Oj. Mix. Dose, ℥ ss to ℥ ij.

Mistura Formyli Terchloridi (or Chloroform julep):—Chloroform, ℥ ss; pure water, Oj. Mix thoroughly, with brisk agitation, for a minute or two, in a vessel capable of containing double the quantity. Dose, ℥ ss to ℥ ij.

Either preparation may prepared from the methylated chloroform; but hitherto I have preferred the unmethylated for administration by the *prima via*.

A chemical nomenclature has been chosen, in deference to the possible and not improbable fears of the patient.

Remarks.—The first preparation requires little to be said about it beyond this: that, although it is already very well known to medical men, and to both wholesale and retail druggists, yet, strange to say, its actual composition and the method of preparing it have been kept a secret from the profession, and from the members of the drug-trade as a body. I have been informed that Professor Neligan gives a formula for *chloric ether*, and that there is one wholesale house in London that makes it according to his formula. I can only say that the house alluded to must sell chloroform for chloric ether, as Dr. Neligan's formula is for chloroform, which at one time passed under the cognomen of chloric ether—a less questionable title than that given to the solution of chloroform in spirit of the present day. (See Neligan on *Medicines*, 2nd ed., 1847, p. 257; and subsequent editions—1851, for instance, p. 301.)

In this town alone there are not two preparations of the so-called chloric ether alike, either in composition or in the invaluable property of being miscible in water. In the April number of the *Pharmaceutical Journal*, Mr. Norman Tate, of this town, has shown that, out of thirteen specimens of chloric ether obtained by him from various druggists in Liverpool and Birkenhead, there is a difference of 9¼ per cent. between the strongest and the weakest samples. As regards the formula for chloric ether above given, I can ever, from considerable clinical experience, that if it be prepared from

bona fide materials, it will equal in medicinal virtue the best chloric ether in the market.

With reference to the other preparation, the chloroform julep, I believe it has hitherto been quite unknown; but the simplicity of its composition, the pleasantness of its taste, the fragrance of its odour, its great and undeniable power as a diffusible stimulant, and its capabilities as a safe and excellent vehicle for administering other remedies, alkaline, acid, or neutral, bitter, sour, or fœtid, need only be alluded to in order to insure it a ready acceptance by the practitioner of medicine. This preparation is reported by some who have tried it to be reckoned a specific for toothache and other painful affections of the mouth and gums. It is simply used as a lotion, wash, or gargle.

The strengths of the foregoing preparations have been made considerably below their respective points of saturation, partly in order to render the dose less irritating to the mouth and throat, and partly to prevent the separation of the chloroform on the addition of an essential oil or such like medicine to a mixture, particularly if prepared from the julep.—*British Medical Journal, May* 10, 1862, *p.* 489.

150.—ON PRACTICAL LITHOTOMY AND LITHOTRITY.

By Henry Thompson, Esq., Assistant Surgeon to University College Hospital.

[The following resumé gives clearly and briefly the relative advantages of the two operations of lithotomy and lithotrity in different cases.]

I. For all cases of calculus during the periods of infancy, childhood, and boyhood—which may be regarded as from one to twelve or fourteen years of age,—the following course appears to be most judicious:—

To practise lateral lithotomy as the rule: the mortality from which varies during this period from one in eleven to one in thirty cases; the mean of the whole being about one in fifteen cases. The exceptional cases are those in which the stone is only a little too large to pass by the urethra, and therefore *small*. For these there is no occasion to perform lithotomy. Opposed as I am to lithotrity in children as a rule, for reasons already named, I nevertheless believe that when the stone is so small as to be pulverised by a single crushing with a suitable lithotrite, it is the simplest and best method of proceeding.

With regard to median lithotomy in children, there appears to be no objection to it, provided a director or gorget conducts the finger into the bladder. At the same time I do not know that it offers any particular advantages.

II. In adult cases we have to decide between lithotomy and lithotrity; and if the former is indicated, to point out the particular form to be employed.

First: Lithotomy, or Lithotrity?

The special indications for these operations must be considered as they relate to two separate classes of patients:—

(*a*) In tolerably strong and healthy adult patients.

(*b*) In feeble and diseased adult patients.

This division is of more practical value than a division founded on age, because the indications of age are less strongly marked after manhood has been attained than before. If age be regarded alone, the period between twenty-five and forty years gives the best results,—namely, one death in 10 cases from lateral lithotomy. But the data are comparatively few, since it is the period of all others when stone is most rare. The question of age and its effects in adults is therefore included in the major one of constitutional condition.

In the class of healthy adults.—If the calculus is of small or of medium size, and single, whether it be soft, friable, or compact, and supposing it proved by preliminary examination that instrumental manipulations can be easily performed and are well borne, it may be crushed.

But the term "medium" here used in relation to size, although admitting

of a little extension for calculi of uric acid and the urates, must be restricted quite within the limits assigned for those of oxalate of lime. Their spheroidal general outline makes a medium sized calculus (one inch in diameter) quite large enough, if not sometimes too large, for lithotrity.

If there is any special or exceptional ground for rejecting lithotrity in such cases, some form of central perineal operation is well adapted to them. The median, if the stone is small or medium; the medio-bilateral, if it is of full medium size: the latter offering more room at small risk when it appears to be required. Probably the result by Buchanan's operation would be equally good; its present achievement in adults is at the rate of 8 cases with one death. The median, in adults, from Mr. Allarton's collected cases, has given one death in 7. The lateral, from 510 cases of adults, in our own table, (but including stones of the largest size,) one death in 5¼ cases. If, on the other hand, the stone is large, and especially if it be also one of compact structure, the lateral operation would, I believe, generally be the best to select. For a large and friable stone lithotrity may be successful; but such an one offering where the patient's age is between twenty-five and forty years would probably be as well dealt with also by lateral lithotomy.

The question to be considered for (*b*) *feeble and diseased adult patients.*

1st. Where there is no marked disease of the urinary organs, but feeble and failing strength:—

If the calculus is of small size and friable, and instrumental contact is well borne, there is no question that lithotrity should be the operation selected. If of medium size, one would also incline to perform it, if possible; but if hard and compact in structure, median or medio-bilateral lithotomy would probably be a safer proceeding.

2nd. Where well-marked disease of some portion of the urinary organs exists:—

In stricture of the urethra, especially if it has existed long, and is well marked, lithotomy is preferable to lithotrity in all cases but those of the very smallest calculi.

In hypertrophied prostate, with a quiet condition of the bladder, lithotrity is successful in small and even with medium-sized stones; but with an irritable condition of bladder, and with compact and large stones, lateral lithotomy seems far preferable, and in such seems more successful than median operations.

In cases where the bladder cannot expel its contents on account of loss of power in its coats—i.e., atony—and not on account of enlarged prostate, no objection lies on that ground merely against lithotrity; indeed, such a condition is much more favourable to success than that of undue irritability of the organ. This is now known to every practical lithotritist, although it is a condition which not long ago was held to contraindicate lithotrity.

In marked disease of the bladder, cystitis with constitutional disturbance, tumours simple or malignant, or if the existence of sacculi may be suspected, lithotrity is generally inadmissible.

Finally, for cases of either class where the stone is of extremely large size, such as are now rarely met with,—say from four ounces upwards,—I doubt whether experience can indicate on the whole a safer method than the lateral operation. We have seen Mr. Crichton's results—viz., eleven cases with two deaths. At University College Hospital, Mr. Liston employed a double incision of the prostate—i.e., an incision of the right lobe as well as of the left—in lateral lithotomy, for nine cases of unusually large stone, losing two.

The high operation offers, perhaps, as good a chance, if the bladder be distensible, and the patient is not corpulent. Data, however, are wanting to determine the value of this as well as that of the recto-vesical operation in relation to extremely large stones.

Such, then, are the general principles which extended observation and experience indicate, in my opinion, to be our guides in selecting the proper operation for given cases. Nevertheless, it is not presumed that every example can be brought to rule and measure, so to speak, and be mathematically

fitted to the process requisite for it. Let it be well understood that they are held forth as principles to indicate the way, not as rigid laws knowing no exception. By some, I am aware, I shall be charged with limiting the application of lithotrity. No doubt more is *possible* by that process. I doubt whether it is prudent to push it further. *It must be our aim to reduce the stones to the process—that is, to detect them early, and consequently small, rather than to extend the process to large and compact stones.* It is true that a stone of that character may be removed in certain exceptional cases by lithotrity, where great tolerance of manipulation by the bladder is exhibited. I may be permitted to allude to one, as an example, perhaps the largest of its kind ever removed by crushing in this country, in the practice of my friend the president, since I had the pleasure of operating during his absence on several occasions. Not less than three ounces and a quarter of exceedingly compact uric acid stone were removed in that case with success; the patient's age being 80 years, and his power of emptying the bladder entirely lost. But this is wholly exceptional, and not to be regarded as a precedent. Had his bladder been irritable, instead of unable to expel its contents, the result would have been impossible. No, I believe that it is certainly true that an undue attempt to extend the province of lithotrity beyond moderate limits will produce nothing but disaster.

Now, I do not know that I could offer a more pertinent illustration of the manner in which I would apply the principles here laid down, nor better exemplify that they are not mere theories on my part, but have been carried into practice, than simply to name the cases that have occurred in my own practice during the past four months while I have been engaged in preparing the material for these lectures, and I desire to point to no better result of their application than the cases in question form. By going further back, I could easily multiply material and occupy your time, but I could not illustrate more effectively. I therefore confine myself within the limits named.

[The lecturer then gave the particulars of five cases in which he had operated during the preceding four months: one by lateral lithotomy, one by medio-bilateral, and three by lithotrity, besides alluding to other cases seen but not operated on at the same period, which need not be presented here.]

I could add numerous illustrations of the advantage of discovering calculus in the early stage, of which two of these cases are examples. I read five cases of this kind, two or three years ago, at this Society, exhibiting the calculi, in order to demonstrate what appears to me the importance of this practice; for I do not hesitate to affirm, that lithotrity for a small, compact, or friable stone, occurring in a tolerably healthy subject, devoid of unusual local susceptibility, at any age above puberty, is, if properly performed, an operation almost wholly devoid of danger. Such a statement cannot be made of any other method of operating for stone.

It follows, then, since every calculus is during a portion of its history, small, and passes generally not very rapidly through the early stages of formation; and since deteriorated health much more frequently succeeds to than accompanies its formation,—it follows, I repeat, *that calculus of the bladder is removable, if its presence be early diagnosed, in almost every case, with very little danger.*

No proposition can be more logically inconvertible than this, yet the practice of the past, so far as it depends on diagnosis, though steadily improving, has done so by slow and gradual degrees. Its truth, however, pregnant as it is with the happiest results for humanity, only requires to be known and acted on in order to achieve at once a great advance, and one which I am sure is within our reach. In this department, as in every other of the wide domains of medicine and surgery, diagnosis is the one thing needful. That diagnosis is slowly reducing the size of the stone year by year, and has been doing so during some time past, is demonstrable from our museums. Take that of Norwich, for example,—the most perfect and complete record, literally "graven on stone," that the world possesses of calculous experience. When the art of surgical diagnosis was young, the stone was often overlooked until its large size and long existence precluded the possibility of the

oversight continuing. Hence amongst the stones removed there in the end of the last century are seen masses which now most rarely come to light. I calculated the sizes presented during each consecutive ten years down to the present time, and noted the significant fact that they are gradually decreasing in size. In other words it has been successively true, at different periods, that the stone now is detected in an earlier stage of its existence than formerly. Thus, the first or earliest 100 stones in the Norwich Museum have an average weight to each of 8·64 drachms; the second 100, 7·07 drachms; the succeeding 400, 5·95 drachms.

But how much too late is still the stone detected! Ought any man who complains of his symptoms, within reach of competent skill, to carry a stone in his bladder for more than twelve months! nay, more than eight, or six! And if detected thus early—if discovered when it assumes the size of a bean, or at most that of an almond, how often would it be necessary to lithotomize an adult? There can be no doubt that skill in diagnosis is greatly on the increase; and from this fact I may venture to foretell, without any pretension to the gift of prophecy, that year by year the stone will be detected sooner and sooner still, and that therefore, *pari passu*, lithotomy will become less necessary, and lithotrity more often applicable. Add to this the fact that the unchangeable conditions of anatomy leave little more to be effected, probably, in the mode of removing stone by the knife; while the improved applications of mechanics to lithotrity are year by year perfecting the process.

I say nothing here of chemical solvents in the treatment of already formed calculus. Of some service in exceptional cases they undoubtedly are. But their special application is to the earlier stage of the calculous diathesis, antecedent to true stone-formation; for the solution of those particles which aggregated form the stone. For the action of solvents we require minute mechanical division of the material to be acted upon; and here we have it. It is but one step further still in the application of diagnostic science, which recognises in the persistent undue deposit of crystalline material in the urine, and certain associated symptoms, that calculus-formation is imminent or impending; and internal solvents arrest the formation of precipitated deposit, while the erring tendency of the economy is gradually corrected by medical treatment, diet and hygiene. But of this enough; for the terms of our subject are purely surgical, and there is no further need that I overstep that imaginary line which is supposed to trace the confines of Medicine and Surgery; a subject which nevertheless illustrates well the truth that no man can be a good practical surgeon who is not also an intelligent physician.—*Lancet*, *May* 3, 1862, *p.* 452.

151.—THE ALKALINE HYPOPHOSPHITES AND THEIR MEDICINAL PROPERTIES.

By John Taylor, Esq., Liverpool.

[The reports by different medical men on the action, whether beneficial or otherwise, of the Alkaline Hypophosphites in Phthisis, vary so much that Mr. Taylor was early led to the conclusion that the same drug could not be employed by the different experimenters. On preparing the hypophosphites for himself he soon found that the diversity of opinion respecting their merits mainly depended on the constitution of the salt used—*i.e.*, whether a hypophosphite, or a phosphite, or a phosphate, more or less combined with a carbonate.]

The first startling fact I observed was, that not more than two grains of a pure hypophosphite of potash or soda can be taken for a dose without exciting nausea and a painful feeling within the anterior part of the thorax, much increased by a frequent repetition of the dose; nor is it surprising that such a feeling should ensue when it is considered that a hypophosphite is the most soluble oxide of phosphorus in the animal secretions, and is at once admitted into the venous circulation. The primitive mode of giving phosphorus to stimulate nervous power was in solution with oil or ether; but these loose

combinations were readily decomposed by emulsion formed with the oil, and the separate absorption of the ether, leaving the phosphorus to act corrosively on the stomach. The hypophosphite, therefore, is the most preferable mode of administration, and is superior to the phosphite, which is only partially soluble without the aid of lactic and carbonic acids; and to the phosphate, which is still less soluble even with that aid, and combined with glucose or sugar, by which means the phosphites and phosphates in food and medicine are rendered assimilable.

The theory of Dr. Churchill and his supporters (as briefly stated in "Braithwaite's Retrospect," vols. xxxvii. and xli., abridged from the Lancet and Medical Times) is that the *cause* of phthisis is the unrepaired loss of phosphorus from the blood and pulmonary tissues, and that the timely introduction of this element into the vital organization is the panacea for the direful malady. "The physiological effects of the soda, lime, potash, and ammonia hypophosphites are twofold," says Dr. Churchill: "they increase nervous force, and are the most powerful hæmatogens; possessing all the therapeutic properties of phosphorus, without the danger formerly attending its use." And further, he adds, "I regard them as prophylactic and curative in every stage of phthisis;" and "I know they will prove not only as sure a remedy in consumption as quinine is in intermittent fever, but also as effectual a preservative as vaccination in small-pox." Experience has not confirmed this extravagant encomium as respects the "*curative* in every stage;" the word *palliative* would be more in accordance with fact, when the remedy has been pure, and with auxiliary combinations. But in the earlier and middle stages, the guarded introduction of an alkaline hypophosphite into the blood produces a glowing influence,—as a respiratory excitant, expanding the chest; as a pyrogenic, increasing animal heat and nervous force, and removing erratic pains; and, as a hæmatogen, forming a nucleus for the rallying of red globules; it increases appetite and cheerfulness, and controls expectoration, night-sweat, and diarrhœa. These points I have repeatedly proved in a series of tabulated private cases, and from cases committed to my charge in the Liverpool Workhouse and Royal Infirmary. The degree of benefit derived from a hypophosphite in phthisis depends upon the skill with which the adjuvans and the corrigens are combined. Unguarded by a mild tonic, a carminative, a stimulant, or an anodyne, as the case may indicate, a genuine alkaline hypophosphite is repulsive to the nerves of the stomach, and the patient refuses to take a second dose, not of one drachm, but even of two grains. My favourite adjunct is compound tincture of gentian, with cinnamon water, and sometimes a little morphia, with the view of obtaining its quiet admission into the circulation, without which no good effect could be reasonably expected. In some cases of extreme debility from cavernous abscess and profuse expectoration, wherein ethers and opiates were barely retaining life, the superaddition of the hypophosphite of potash produced a magical effect, invigorating the muscles so that the chest could be fully expanded, and the lost power of progression was restored, the cough suppressed, sleep induced, and the expectoration much diminished. But this improvement was not steadily maintained, unless the patient was able to procure, and made capable of digesting, sufficient nitrogenous and carbonaceous food containing a due proportion of phosphatic salts to repair the waste that had been so long enfeebling him. And I have frequently found, in the course of my investigations, that when once the vital powers had been aroused by a hypophosphite, the ingesta required to be much increased—in fact, proportionate with the increased secretions of the mucous membranes caused by the hypophosphite; otherwise gastric irritability ensued, with its sequelæ. This feature was particularly observable in the workhouse hospital, where the subjects of phthisis were chiefly the broken-down systems of the intemperate and the extremely poor. The disease seemed all the more rapidly to run its course to a fatal termination under the influence of the hypophosphite of potash, through inability to replace the organic particles lost in the course of the disease. In these special cases, a larger amount of alcoholic food, temporarily supplied, would enable the almost devitalized absorbents to accept phosphatic

food; for, as Lehman observes, "the presence of phosphates and an excess of potash salts are the result of necessity in the tissues; and as all plastic secretions notably contain phosphates, the necessity for their reparation is evident, as they are required to supply the first basis of the new tissue."

In this hospital I met with some pleasing instances of the impulsive power of the hypophosphite of potash in renovating the vital powers of some bed-ridden women who were complaining of deep thoracic pains, shortness of breath, muscular and arthritic pains in the limbs, the result of excessive tea-drinking, and the want of phosphatic food before entering the hospital. One mixture, containing half a drachm of the salt, with one ounce of compound tincture of gentian with cinnamon water and a little mucilage, relieved the pains, and gave the disposition to leave the bed and ask for more food. Some hospitals abound with cases of this description, set down as neuralgic and rheumatic, in which food is the proper cure; and an alkaline hypophosphite, scientifically administered, may prove the enabling means for assimilating that food, care being taken, when the capillaries have been excited by a hypophosphite, to follow on with such food as will supply the protein compounds, albumen, fibrin, and casein, with the mineral salts.

The controversy as to the merits of the alkaline hypophosphites has awakened the spirit of research. It has led me to the discovery of the aptitude of these salts for many morbid conditions to which they were previously thought inapplicable, and probably others are pursuing the same course of investigation—viz., proving facts in reference to their power in augmenting nervous force in the secerning vessels; in increasing the natural, and diminishing morbid secretions; allaying nervous irritation, and implanting the cementing principle which gives force to other elements in the blood that have become defective through the inordinate lapse of phosphates. Examples are to be found in gestation, prolonged lactation, dentition, some forms of dyspepsia, anæmia, catarrhal and leucorrhœal discharges, and in myalgia and the muscular pains simulating inflammation. But I shall defer the description of these therapeutic points until I have explained the chemical constitution of the hypophosphites.

Owing to the method of manufacture, as described in authoritative books and scientific periodicals, it is evident the product must be in a great measure a compound of phosphite, PO_3, and a phosphate, PO_5, with some amount of carbonate, the two latter salts prevailing most in the hypophosphite of lime, from the well-known affinity of lime for carbonic acid; yet, strange to say, the other alkaline hypophosphites are directed to be prepared from this salt by double decomposition with their respective carbonates. Correspondence has been had with several manufacturing chemists, who admit this to be the practical mode of preparation: Phosphorus, one pound; calcis, four pounds; water, five gallons—are gently boiled in an open pan until the phosphorus disappears. Now as phosphorus melts at 120° Fahr., boils at 250°, and volatilizes at intermediate temperatures, making the aqueous vapour luminous, and as phosphorus is more dense than the saline solution around it, and consequently remains at the bottom of the pan subjected to the greatest heat of an open fire, sand-bath, or hot hearth, sublimation of a portion of it must inevitably occur. Then as to the remainder, it is first rendered into P_3O; then into the hypophosphite, PO. In this condition it readily passes into a higher degree of oxygenation, and becomes a phosphite, PO_3, when surrounded by an alkaline solution under prolonged boiling. On further boiling and evaporation, the greater part is converted into a phosphate, PO_5. This last change is conformable with the chemical law affecting the sulphites also. Sulphate of magnesia is now made on the large scale simply by boiling a solution of the sulphite in the open air. Dulong, the celebrated chemist, states that when the hypophosphites in solution are boiled in contact with the atmosphere they are converted into simple phosphates; and Rose, the equally eminent chemist, goes further in stating, that when they are boiled in a close vessel, they remain unaltered, provided no excess of alkali is present. From this statement it would appear that the first-formed portion of the hypophosphite is more difficult to preserve than virtue itself, for

it must necessarily be surrounded by excess of alkali. And he further adds: "The conversion into hydrogen gas and a residual *phosphate* is the quicker according to concentration and quantity." It will not now be difficult to understand why one practitioner makes two grains his maximum dose, another five grains, and another one drachm. I do not hesitate to affirm that one drachm of a *pure* hypophosphite of potash (2 H O, P O + K O + *n* Aq.) or soda (2 H O, P O + Na + *n* Aq.) taken at once will produce poisonous effects; and if so, the hospital reports already published of experiments with such large doses of the hypophosphites are altogether valueless.

In the preparation of a hypophosphite the chief points of attention are to keep the temperature below 220° and equally applied around the vessel, and to exclude the air. A water-bath, a steam-jacket, or an oven, answers well. For the exclusion of air, if the vessel be of the common oil-flask form, and the space between the fluid and the mouth be kept constantly filled with vapour, not only is the air excluded, but the issuing phosphuretted hydrogen does not explode within and fall back as phosphoric acid, to form an immediate phosphate. A still better method is to secure a funnel with a loosely-fitting marble, or other spherical body, in it, upon the top of the vessel. But when the operation is carried on a few days at the temperature of 100° to 150°, a stratum of oil of turpentine poured gently on the surface excludes the air, arrests the phosphuretted hydrogen without explosion, and forms a phosphuretted terebinthinate camphor, which imparts a somewhat disagreeable odour to the salt. I have not found the product less efficacious therapeutically. By whatever mode the hypophosphite is prepared, care should be taken in the evaporation to form a dry salt that the temperature of 140° be not exceeded. In one of the experiments in the formation of the hypophosphite of soda, the salt became intensely dried in the oven at about 254° Fahr. On removing it into a closely stopped bottle, a lambent flame of non-explosive phosphuretted hydrogen gas played over the dried salt for several hours, proving that the constitutional water of composition (for it cannot exist without two atoms of water) was decomposed, one portion of the phosphorus taking the oxygen to form phosphoric acid, the other taking the hydrogen, and forming the non-explosive hydrogen gas, H_3P, the explosive kind being $H_{10}P_5$; or as Levrier has it PH_2. When the solution has attained the crystallizable point, a temperature of about 70° to 90° is best. Though there are good reasons for preparing some of the medicinal hypophosphites by double decomposition, there can be none for the potash and soda hypophosphites. Notwithstanding what Rose says of concentrated alkaline solutions and phosphorus being converted the most quickly into phosphates, I find the strong solutions, as 1·075 to 1·100, best adapted for oxidizing the phosphorus quickly; but of course, if boiled in an open pan, his aphorism becomes true. Liq. potassæ or sodæ, 1·075, takes up one-fortieth part of its weight of phosphorus, a portion of which seems to pass into hypophosphorous acid from a state of vapour; but the greater part passes through suboxidation—the yellow oxide which Graham calls "the hydrate of phosphoric oxide," and supposes it to contain one-fifth water. On brisk ebullition this vapour passes off, and burns non-explosively, constituting a great source of waste in manufacturing on the large scale. By experiment I have proved this loss to be one-third. It is easy to distinguish the escaping phosphorus flame by its luminosity, so different from the bright vivid explosions of phosphuretted hydrogen. Weak alkaline solutions produce much waste of phosphorus in the form of an insoluble flocculent hydrate.

Without speculating on the proximate cause of phthisis—whether it is the unrepaired waste of the phosphates from the blood and tissues, as some believe—it is certain that in the course of this and of many other maladies, the secretions and excretions abnormally abound in phosphates, and that in proportion to their excessive loss is the amount of succeeding debility and of sympathetic fever; hence the constant surgical aim, in wounds, abscesses, and sloughing, to assist the digestive organs in the assimilation of phosphatic food. The same necessity exists for a normal phosphatic blood to produce healthy exudations for uniting wounds and fractures. "The idea that

the plasticity of an exudation is dependent on the quantity of fibrin it contains is erroneous," says Lehman. "Some plastic exudations are without fibrin, and some non-plastic contain it. The plasticity of the exudations is constantly associated with the presence of soluble phosphates." The integrity of the textures, therefore, depends upon their supply, just as much as in their first organization, wherein special providential design is exercised. Take, for example, *incubation*. In the yelk of egg phosphates are placed for the *early* nutrition of the embryo chick, and in the *shell* as glycero-phosphate, for the double purpose of affording protection during gestation, and nutritious support during the latter period of embryo development, by endosmosis through the albumen, and the consequent thinning of the shell preparatory to its escape when at maturity.

The providential design manifested in this lower order of vitality should be, and often is, our type for imitation in the ailments of pregnancy proceeding from defective assimilation of food. Human gestation, it is well known, requires the phosphatic salts—so essential for the formation of the child—to be drawn from the food of the mother; when the due quantity fails to be supplied from this source, supplementary demands are made upon *her* tissues. Hence follow defective secretions, dyspepsia, stitches, neuralgic pains, absorption of fat and muscle, pinched features, and a long train of morbid symptoms usually ascribed to uterine expansion, irritation, and reflex excitement. A modern popular plan of treatment is the exhibition of an akaline phosphate, with sugar of milk for aiding its solution, the acid secretions of the stomach and the lactic aid from the digested sugar liberating carbonic acid, which dissolves the phosphate. I have found the hypophosphite of potash, in two-grain doses three or four times a day, combined with half a drop of creosote, mucilage or syrup, and the compound tincture of gentian, much more prompt in action and more certain of gaining admission into the circulation. The superior value of a hypophosphite depends on its ready solubility in the animal fluids and its subsequent conversion into a phosphate. Bischoff observes that "phosphate of lime requires for its solution 2000 parts of water saturated with carbonic acid:" and the phosphates of potash and soda in food require for their solution weak acids, which are not invariably present in the alimentary canal. Thus bread and animal food cease to nourish through the failure of the mucous secretions, and absolute weakness may ensue though copious ingesta are received. These facts form a ground of preference for the hypophosphites over the phosphates as medicines, for the former can reach the lacteals when the latter may not; moreover, the blood being a constantly alkaline fluid, the combined acid of a phosphate may be abstracted, and a more or less insoluble compound ensue, only fit to be removed from the system by the kidneys and other emunctories with the phosphates resulting from effete tissue. Doubtless, in a state of health, the various acid and alkaline secretions of the several portions of the alimentary canal have a solvent action on the nascent phosphates of the food; triple salts are formed, which are absorbed by the lacteals, and conveyed by endosmosis through the portal veins to the liver, there to be assimilated. "Phosphorus is the element which accompanies nitrogenous matter in all its phases," says Corewinder, an essayist on vegetable life. It cannot therefore be unphilosophical in us to imitate nature by accompanying nitrogenous food with the most soluble and absorbable salt of phosphorus, in cases of exhaustion of phosphates from the blood in *prolonged suckling*: in the *dentition* of the strumous child; *leucocythæmia* and general *anæmia*; *catarrhal* and *leucorrhœal* discharges, and all inordinate secretions of *pus*; *myalgia* and the muscular pains *simulating inflammation*; and in some forms of *dyspepsia*; examples of which will be given.

And first as to over-lactation. The same object which induces us in feeble gestation to plant a further quantity of phosphatic salt in the blood, around which the protein compounds may rally, and so supply without material waste the growing demands of the new offspring, applies to over-lactation. Here is a case in point:—

Mrs. B., aged 40, suckling her fifth child, now three months old, has all

the usual pains and stitches in her back, chest, and abdomen, with dimness of vision, nausea at the sight of food, despondency, and a painful sense of dragging in her left breast. Ordered to desist from beer and porter, and to take a mixture containing half a fluid ounce of the unevaporated solution of hypophosphite of potash prepared by myself (equal to one scruple of the dry salt), and half a fluid ounce of compound tincture of gentian, with seven fluid ounces of peppermint-water: one tablespoonful three times a day. Full of ecstacy, she returned for a second quantity, declaring that she "felt new life," that "every dose increased her appetite," and that "all her pains were gone." I observed a freshness in her features and briskness of manner which she had not before; and if she had consented to wean her child I would not have ordered the renewal of the medicine.

One more example, as a type of a large class of cases:—

Mrs. R., a tall, good-looking woman, somewhat pale and attenuated from five months suckling her fourth child, had burning pain at the root of the nipple, and a sense of dragging at the breast; the burning pain remained so long after suckling that she got little or no sleep. She took the mixture as above, with the addition of ten grains of hypophosphite of soda in two fluid drachms of unevaporated solution, and four fluid ounces of mucilage of acacia, supplanting as much peppermint-water, which is my usual custom. I called upon her when she had taken but half the quantity, and found her quite free from pain, increased appetite, and in excellent spirits at the result.

These are striking instances of the power of the alkaline hypophosphites in quickly regenerating the blood, and inducing it to receive the protein compounds.

There is something remarkable in the permanence of effect produced by the hypophosphites of potash and soda in prolonged lactation beyond that experienced in phthisis, which may be partly ascribed to a little free alkali in my first preparations. Guided by the facts maintained by modern physiologists,—that in the blood there is indispensable necessity for free alkali, and that although possessing this property it exudes *free acid* through the capillaries and acid phosphates through the vascular coats,—it is not difficult to perceive how the milk becomes vitiated, the child griped, and the mother a sufferer from muscular pains. I have therefore thought it best to accompany these most soluble salts of phosphorus with an alkali—ensuring their absorption, and neutralizing free acids that might oppose their force; the result in many cases has been beyond my expectation. This fixed necessity for the constant presence of alkali in the blood lies at the foundation of many natural operations; for it is found, as Lehman observes, "that the tendency of oxygen to combine with certain elements is strengthened by the presence of alkalies, whereby the collective organic constituents of the blood are subjected to a process of gradual oxidation by the simultaneous presence of oxygen and loosely combined alkalies." One of these elements so beneficially influenced is sugar from the carbohydrates contained in nutrient matters, "which, for the most part," says a distinguished physiologist, "reaches the blood in the form of grape sugar, and with the aid of an alkali, contributes to the solution of the carbonate and phosphate of lime." So powerful, indeed, is this union, that when sugar is thus associated with an alkali it is capable of taking up combined oxygen, and of withdrawing it from many oxides. In this respect alkalized sugar resembles the chief scientific characteristic of the hypophosphites—viz., their commercial value—that of depositing metals from their solutions by the abstraction of their oxygen. Large quantities of alkaline hypophosphites are made for this purpose in America, where their medicinal application and that of the alkaloid and metallic hypophosphites are best understood. It is probable (for too little is known of vital chemistry to enable us to say with certainty) that the alkaline hypophosphites and the soluble phosphates have a controlling power over the oxygen-absorbing property of alkalized sugar; for it is found that when the proportion of phosphates in the blood has fallen low the sugar sometimes attracts so much oxygen as to be converted into *oxalic acid*, which would immediately poison were it not for its strong elective affinity for lime, which it

abstracts, not only from the blood, but, as Dr. Golding Bird has stated, "from the tissues also," constituting one of the common results of metamorphosis of tissue, and consequent emaciation. These deposits, he observes, "are more frequent in the urine than the phospates;" they are accompanied with great excess of urea, hence the increased density of the urine, impoverished blood, intense despondency, weakness, and loss of flesh. This deposit of oxalates accounts for some of the persistent forms of dyspepsia so difficult of management, and for which, in accordance with the reasons already stated, the alkaline hypophosphites are well adapted. With these views I have prescribed them with marked success in many cases of dyspepsia, chiefly in sedentary young females and middle-aged people who have abstained from nitrogenous food, and lived principally upon tea and cordials. My hospital reports abound with such cases, and the results have been most satisfactory.

I will here relate two cases, types of a numerous class occurring in private practice:—

Miss L., aged 48, has shooting pains through her chest and about the scapulæ, and tenderness on the hypochondrium; occasionally vomits bile; is costive, very weak, and painfully flatulent, with a long catalogue of hypochondriac ills. She lives principally on tea, and has disinclination for any other food. Administered half a fluid ounce of the hypophosphite of potash solution (containing half a drachm of the dry salt) and half an ounce of compound spirit of lavender, in six ounces of cinnamon water; a tablespoonful three times a day. She was relieved in all respects, had a better pulse and softer skin, and could eat beefsteak with a relish. Repeated the mixture, substituting the soda salt for the potash, which is my habit, to avoid the accumulation of any one alkali. In the first selection of the appropriate alkaline hypophosphite I am guided by a natural law defined by Liebig, "that phosphate of potash (2 KO, HO, PO_5—an acid salt) exists in the muscle; and that phosphate of soda (2 NaO, HO, PO_5) exists essentially in the blood; and that the membranes prevent the direct interchange of the salts, otherwise they would be equally distributed over the body. The soda phosphate increases the solubility of carbonic acid in the blood, which phosphate of potash does not; and so necessary is the soda phosphate to the blood, that if the food contains only the potash phosphate, it decomposes the common salt (sodii chlor.) and forms a soda phosphate." So that, as a rule, the soda hypophosphite should be given in blood diseases, and the potash, ammonia, or lime-salt in those of the secretory organs.

As my object is to institute the principles of medicine as respects the alkaline hypophosphites, I have studiously avoided the relation of long cases, and shall therefore give but a brief outline of an every-day case of dyspepsia, occurring in needle-women.

Miss J., a dress-maker, aged 20. Symptoms of milder form than in the last case, but similar in most respects, particularly as to thoracic pains, aversion for food, and weakness, for which tonics and alteratives had been prescribed in vain. Half a drachm of the hypophosphite of potash, combined with compound tincture of gentian, acacia mucilage, and cinnamon water, permanently cured her; of course acting on my invariable injunction that when the capillaries have been excited by hypophosphites in the blood, the daily food should be improved. This point should never be lost sight of, or the patient will attain a flattering degree of improvement, and then fall back; for it would be irrational to expect that a mere provocative of function can sustain the whole fabric of animal life without alimentary support! On this ground cases of phthisis have receded from a very flattering degree of improvement produced by the alkaline hypophosphites. One poor fellow, prostrated by cavernous abscess, took the potash salt with ether and ammonia, with astonishing effect. In a few days his cough and expectoration were suppressed, and his general strength so far restored that, striking his chest with his hand, he called himself a new man. Passing over to Ireland on the deck of a steamer, he wrote thence to his family, acquainting them with his continued improvement; yet in a few weeks the usual results of tubercular disorganization set in, the power of reparation not being equal to the destructive agency.

In anæmia and leucocythæmia I have prescribed the saccharo-alkaline hypophosphites with considerable advantage being induced by the fact recently announced by Chevreul and Scherer, "that when hæmatin is dissolved in alkalies it is able to continue unchanged for a long time, and that on the access of atmospheric air it instantly attracts oxygen and becomes converted into a colourless body." Now, as I have before explained, alkalized sugar and alkaline hypophosphites separately possess the power of abstracting oxygen from surrounding bodies; it therefore seemed to me that their united efforts might afford protection to the hæmatin in the blood, and dispose to its increase from the ingesta; and such is the fact. Two cases in elucidation:—

A. S., aged thirty, a female assistant in a shop, has pains through her chest and limbs, palpitation, short breathing, entire loss of appetite, and loathes the sight of food; has no sleep; is giddy on rising in the morning, and can scarcely dress herself; pulse 60, slow and weak; countenance anæmic; catamenia irregular: in short, though she had been a fine, muscular, ruddy-complexioned woman, she is now so weak that she can hardly walk up-stairs. The combination of hypophosphite of potash with a little syrup and sufficient tincture of gentian to render the medicine acceptable was exactly suited to the case. It produced sleep without an opiate—appetite without tonics, so that in five or six days she relished a beefsteak to breakfast; and after two renewals of the mixture (substituting the soda salt) every function was restored, and with hearty gratitude she observed that "she had never had anything that did her so much good." Many cases of a proximate nature occur daily, in which the action of the most reliable tonics disappoint, however varied the range. I would suggest to my medical brethren the addition, under therapeutic rule, of a medicinal hypophosphite. It has sometimes had the effect of directing the proper action of the remedy that had previously been inert; and, on the other hand, the efficacy of an alkaline hypophosphite has been increased by the addition of a salt of iron formed by a vegetable acid—the citrate for example. It is the opinion of some persons that in order to test the value of the alkaline hypophosphites they should be given alone, in contravention of the time-honoured practice of combination by which various powers with intensity of action are secured. Take, for example, Dover's powder, and the infinitely varied combinations of calomel, opium, and ipecacuanha, by which our ancestry and ourselves have rendered good service. And so I find, in my experience with the hypophosphites, that not only is the basis (curare) needed, but also the adjuvans, corrigens, and constituens. Creosote, in doses of a quarter to half a drop, is a most favourable adjunct; and morphia proves sometimes an indispensable corrective. Ethers, the vegetable alkaloids, ammonia, liq. arsenical. and such metallic salts as do not produce decomposition, are not only chemically compatible but highly efficient combinations, when therapeutically indicated. A very efficient pill may be formed of two grains of hypophosphite of soda or potash, two grains of sugar of milk, one-fifth of a drop of creosote, and one grain of any desired adjunct powder.

The next case of anæmia is that of a lady fifty years of age, the daughter of a retired navy surgeon, a widow with three children. This was a well-marked case, the extreme pallor of the skin and lips being accompanied with gradual loss of flesh, although her appetite was good, and she was taking freely of animal food and porter daily—her alvine and renal functions being well performed. Still her strength gradually decreased, so that within ten or twelve weeks she was changed from being a fine, tall, robust woman, to a feeble invalid, almost pulseless, and scarcely able to turn in bed; so inelastic were the air-cells that she gasped for air, and required the windows and door of her bed-room open in the depth of winter; she was sleepless; her voice, too, was reduced to a whisper; and she had a few aphthæ on the tongue and rectum. During the previous eight weeks she took the most esteemed tonics and hæmatogens without effect; and now, when the case had assumed this desperate aspect and her death was daily expected, I put her under the influence of a hypophosphite in the following form:—Solution of hypophosphite of soda, half a fluid ounce (equal to half a drachm of the

dried salt); chloric ether, one fluid drachm; muriate of morphia, two grains: compound infusion of gentian to eight fluid ounces; one tablespoonful every three hours. The first effect was to infuse new vigour into her respiratory organs. The gasping for air ceased, and she felt that the beef decoction and other nutriment now improved her strength. A week's repetition of the means produced further improvement, she being able to get out of bed without assistance. On continuance of the medicine the morphia was omitted, sleep having now returned; but as the pallor and aphthæ remained, the tincture of sesquichloride of iron was added, without advantage. We were obliged to retrace; and as the potash salt was substituted for the soda, she rapidly acquired muscular strength, being altogether one month under the hypophosphite treatment; the only remaining symptom being numbness of the hands. The inference is, that the hypophosphites formed a nucleus for the formation of hæmatin, on which the protein compounds fixed and formed a pabulum for the secretions as explained in the exordium. The same reasoning applies to the utility of the hypophosphites in most of the conditions in which they are remedial.

The impulsive demand for tribasic phosphate of lime in the construction of the teeth contributes to the disturbing influence called the fever of dentition; and this disturbance is often found to be most pyrexial in those children that have been ill-fed, or that have been too long suckled, both instances showing the want of a due proportion of phosphates. In the robust there is sympathetic spinal irritation, tending to convulsion; in the feeble and cachectic sympathetic nausea and purging, wearing out existence. In both forms have I given hypophosphite of potash with marked success: in the first, or sthenic form, with solution of acetate of ammonia and syrup of rhubarb; in the latter, with acacia and some tonic or aromatic tincture. It is delightful to witness the consolatory effect upon a fractious, pseudo inflammatory child, some cases of minor intensity being cured by the first effort—say eight grains of the hypophosphite in a two-ounce mixture. Those of the asthenic form had considerable dyspnœa, cough, pale dry skin, and a feeble pulse; some requiring ammonia as an adjunct; others not; but in all the impressive power of the hypophosphite, with its appropriate adjunct, was most satisfactory—frequently but simple syrup or mucilage sufficed.

As to *catarrhal*, *leucorrhœal*, and *ichorous* discharges, and excessive depositions of *pus*,—without reciting their pathology, I may venture to say that the most recent physiologists have defined the constitution of mucus and pus to be nearly identical, and differing only from other exudations by the considerable amount of "corpuscles they contain, loaded with phosphates and potash." It is not surprising, therefore, that these disorders should be attended with considerable debility, and that the accompanying fever, resulting from over-depuration of the phosphates and other salts from the blood and tissues, should be more frequently a hectic response than inflammatory. Whatever be the theory, daily hospital practice corresponds with this view. Every effort is made to supply the waste of phosphates by a direct supply of the material lost—from phosphatic food aided by various expedients and vinous stimulants to render that food appreciable. By these means fatal ulcerations of the mucous membrane, and sloughing from pressure on prominent parts, so common in low states of vitality, are prevented. Still, success cannot at all times be commanded, particularly in the ichorous rectal discharges of the intemperate, with fistulous openings too prone to slough to afford a chance of reparation by enlarging the fistulous canal; an alkaline, alkaloid, or metallic hypophosphite, as circumstances may direct, will so far improve the alimentary secretions as to render food digestible that would otherwise be nauseated or rejected, by which means further disorganization may be prevented. On this principle, I have found the auxiliary aid of an alkaline hypophosphite most valuable in cases of over-depuration of pus, and in excessive catarrhal and leucorrhœal discharge, as well as in the graver exudations which gradually and irresistibly wear out vitality. But when advantage is thus gained, it requires to be sustained by a sound nutritious diet, or the remedy will have to be repeated so long as the proper nutrition is withheld. If the low vital

powers exercised in many diseases scarcely effect the solution of the then almost insoluble phosphates of ordinary food, and if, to quote an aphorism, "animal life dies out if not supplied with salts for the blood, though the food in other respects be nutritious," duty points to the use of a soluble phosphorus salt. The familiar experiment of attempting to sustain life in a dog solely by jelly food confirms the aphorism.

In conclusion, I would say that I have not the exalted faith of Dr. Churchill in believing that the alkaline hypophosphites can *cure* phthisis in *all its stages*. They remarkably sustain the vital power, inducing a strong hope of a favourable issue, even under the depressing influence of cavernous ulcers; but in the advanced stage, from the want of the undiscovered antidote to tubercular degeneration, fatal lapses frequently ensue. In the earlier stages, and before vital declension has become a constitutional power controlling every organic function, they have an admirable effect, enabling the best known means to have increased remedial energy, and thus to effect many more cures than formerly.

The superiority of these alkaline salts in controlling night sweats, compared with the acid treatment formerly in use which too frequently induced alternating diarrhœa, is a point also of some importance; and on the whole, if we believe that "disease is cured by natural processes, promoted by means which uphold vital power, and that our duty is to search for antidotes to morbid poisons, at the same time upholding vital power," it appears to me that we have in *phosphorus* and its *salts* the best known antidotes for phthisis. Much has been recently written condemnatory of this opinion; but when it is considered that the phosphorus administered has been of the ordinary stick form, comminuted, or in solution, and the hypophosphites (so called) have been given in doses that would have proved poisonous, if genuine, the opposing argument is not sustainable, and the deductions must necessarily be dissimilar. This result, however, has had the beneficial effect of awakening the spirit of research. It has induced some practitioners who have been fortunate in procuring the best hypophospites extant to give five-grain doses in debility from various causes, and it has enabled me to discover their adaptation to the control of several morbid functions of the neuralgic form, for which the recognized antidotes had been unavailing, in addition to the specialties herein enumerated, many of which, so far as I know, are entirely new to the profession. The most useful in general practice may prove to be their stimulative power on the capillaries of the whole secretory system, another type of which I may add to those already mentioned, in a case of debility after parturition:—

Mrs. P., aged 40, four days after favourable parturition of her fifth child; has a pale, perspirable skin, and a weak, non-inflammatory pulse; excretions natural; thinks she has a good flow of milk, but the child is sucking through the whole night, and when removed from the breast is griped with greenish yellow stools, and cries incessantly until fed with artificial food. I gave the mother a mixture of hypophosphite of potash in compound infusion of gentian. She was relieved of her nervous anxiety the first night; but, on the second, both mother and child had perfect composure, the breasts were full of milk, her appetite was improved, and altogether a most agreeable change in her feelings had occurred, very remote from those she experienced when under suppression of milk from the shock of parturition. In promoting secretions, I am preferably inclined to the potash hypophosphite, for reasons explained early in the argument; but, as a pyrogenic and reformer of tissue in the advanced stages of phthisis and other disorganizations, I think, from the experience I have already had, the amorphous phosphorus will be found superior. I allude to the allotropic form produced by the subjection of common phosphorus in a closed vessel for several hours to the temperature of 400° to 450° Fahr., and which appears as a soft brown powder, incombustible below 500° Fahr., except when mixed with an explosive salt and under friction. Within certain conditions, hypophosphites may be manufactured from it, with the evolution of non-explosive phosphuretted hydrogen H_3P; a great desideratum, no phosphoric acid being formed. At a future time, when

further therapeutic investigations have been made, the results shall be published. Great care should be used in the manufacture of the hypophosphites from amorphous phosphorus, as it will violently explode, even below 200° Fahr., if the vessel has an adherent crust of carbon, sulphur, and metallic oxide, however small, the result of previous operations. This remark applies in some degree to ordinary phosphorus, whenever one or more of these adventitious substances are present.

The plea for giving so much importance to phosphorus and its salts in supporting vital function is, that it is a principal component in the organization of animals and in the circulation of vegetable life. It is computed by Stockhardt that the bones alone of the human body contain from sixteen to twenty-eight ounces of phosphorus besides what is contained as the skeleton of the blood, flesh, and viscera; and as it is the cementing principle and bond of union to the other components of texture or tissue, we frequently witness the rapid disorganization caused by its displacement. Even a little alum in bread, by abstracting the phosphate of lime and forming an insoluble phosphate of alumina, soon softens the bones and makes their strength depend on lime and gelatin; the obvious consequence of which in the long bones of children is frequently a case of rickets, and in the viscera lesions not so well defined but equally destructive to health. It is somewhat remarkable that phosphate of soda, so essential to the blood, is technically used to render fabrics incombustible. It may have the property of controlling the too rapid combustion of the blood in respiration; if so, it is a curious fact in physiology, that a hypophosphite should be a pyrogenitor, and a phosphate a pyroannihilator. Phosphates have been designed as the durable cement for both organic and inorganic structures; and as when Roman cement formed from the phosphatic stone nodules of the London clay is mixed for use with too much silica, loses its cohesive force, and crumbles to decay along with the building it was intended to support,—so with organic structures, when the normal proportion of the phosphates is reduced by morbid vital action or defective supply, disintegration of the bloodvessels ensues, as manifested in many forms of disease, attended with considerable weakness; chronic albuminuria may not prove an exception, and the replacement of phosphates by means of saccharo-alkaline hypophosphites may produce a beneficial result.

It is not in the vast, the awful, and the overwhelming alone that we are to look for the beneficent designs of creation, but also in the minute details of organization; and not least, probably, in the wisdom which constituted the basis of the tissues a *monobasic* phosphate suitable for absorption and replacement, and that of the bones *tribasic*—forming a structure so durable that, unless destroyed by diseased vital action, ages pass away but sparingly decomposing it, and even when exposed to air disintegrates more slowly than many rocks. So important is the continuous supply of phosphorus for the maintenance of animal and vegetable life regarded by a French writer, Baron Ernouf, that he deeply laments its possible exhaustion, and predicts "that as its return to the earth is not at all in proportion to its removal in grain and other produce, a day will come when the sun shall shine on an unpeopled and desert globe." The great error, he thinks, is in human sepulture; his aim being to make the fathers' bones subservient to the children's bread. Though many tons of phosphates are now wastefully washed into the sea for sanitary purposes, there is no fear of the future providential supply of phosphorus,—relying on the fact that its elements will in the progress of science be discovered and applied. A Scotch professor of chemistry throws out the suggestion that phosphorus probably consists of *hydrogen* and *nitrogen*; and he draws this conclusion from its analogies. If his speculation be correct, it is possible that it may be in some degree spontaneously formed as a secretion designed to give vital stimulus to each organ, and that reparation chiefly depends upon artificial supply from the ingesta.*—*Lancet, Nov.* 30, *Dec.* 7 *and* 14, 1861, *pp.* 517, 544, 564.

* If any one wishes for my particular alkaline hypophosphites, they can be obtained from Mr. Charles Taylor, manufacturing chemist, 10 Cleveland-square, Liverpool; at 2s. per ounce.

152.—ON URANOPLASTICS.

By GEORGE POLLOCK, Esq., London.

It has fallen to my lot to have had the treatment of more than one case of complete congenital cleft of the hard palate, and complete closure has been the result in each. Several of the cases have been operated on in the hospital, so that their results are known to others as well as to myself. The experience gained in the treatment of such cases enables me to assert that the most favourable cases for operation are those in whom the cleft is complete or nearly so. Attention has already been drawn to this fact, and the explanation is simple. The more complete the cleft, the nearer the perpendicular are the sides of the palate, and, consequently, when the soft tissues are detached from the bones, the flaps formed fall inwards, and very readily meet in the median line. Indeed, in some instances so abundant has been the flap on each side that their margins have had to be somewhat freely pared to prevent the edges overlapping, for by their so doing the efforts to secure union would be frustrated.

The really troublesome cases in treatment are those in which, firstly, the cleft is confined to the posterior part of the hard palate, and in which, secondly, the opening in the palate is the result of exfoliation of bone.

The difficulties in the treatment of such cases arise from the circumstance of the arch of the palate being of the natural curve, or nearly so; the consequence is, that when a gap or opening exists in any portion of such an arch, and the soft tissues are separated by operation around either aperture, the edges of the flaps will rarely meet in the median line by the mere falling down of detached soft structures; lateral incisions then become necessary; and thus an additional interference with nutrition of the flaps is set up.

The difficulty of bringing the edges together, in certain cases of the character last referred to, is so great and so evident, that it would be unwise to attempt a closure by operation.

The complete congenital cleft offers the best prospect of success of closure by operation; and there is little credit due to the surgeon who overcomes a deformity so apparently difficult to deal with, but which really is by no means so troublesome to close as a slight posterior cleft. It must be understood that we do not allude to clefts confined to the soft palate; such clefts are not under consideration.

An error into which Professor Langenbeck has fallen, relates to the causes of failure in operations for cleft palate. He considers that his method of operating possesses an advantage not obtained by other surgeons, and this advantage he supposes to depend on the circumstance of his peeling off the periosteum, together with the soft structures which cover the sides of the hard palate. He states that it is much easier to separate the mucous membrane alone; but "if this membrane is separated to a great extent, gangrene ensues; and it is the reason that a complete cure of a total cleft of the hard palate has never been accomplished by that operation." He further entirely ignores the possibility of the success of any one but himself; for he continues,—"but that this (the separation of the periosteum) has, in fact, never been done, is evident not only from the express statement of those surgeons [named], but also from the fact of the partial or entire failure of their operations."

We need not stop to inquire by what means the Professor has established the correctness of his new theory; but it is desirable to test the truth of his statement, that it is easy to separate the mucous membrane from the periosteum, and that his own success is due to the periosteum being always separated with the mucous membrane in his operations.

It is impossible in the living subject to prove that, during an operation for closure of a cleft palate, the mucous membrane is readily separated from the periosteum, or that the latter is not detached from the bone; but in the dead subject it will be found equally impossible to separate from the bone the mucous membrane without the periosteum. In most cases which come under

the surgeon for operation, the patients are not far advanced in life. Up to the middle periods of life the under surface of the hard palate is rough, and often ridged with thin prominences of bone. In attempting to peel off the mucous membrane the periosteum is detached with it to a great extent without any care or trouble, but many little patches remain adherent to the depressions and furrows in which the vessels run and send to the bone its minuter nutrient branches. If any one curious on this question will endeavour to remove the mucous membrane from the palate in the dead subject, it will be found by no means an easy process, without, at the same time, removing the periosteum with it.

It will be quite evident from the results of such an examination that Professor Langenbeck has not effected in his operations more than has been achieved by other surgeons experienced in the treatment of clefts of the hard palate.

The true causes of failure in operations for closure of clefts running into the bone have been entirely overlooked by the Professor. In detaching the soft tissues the parts are often bruised, and then invariably some interruption to union will result. But the chief cause of failure is the difficulty of getting the edges of the soft flaps to meet without any traction by sutures. This difficulty never presents itself in complete congenital clefts: in such cases the flaps meet readily from their falling inwards and downwards when sufficiently detached. But such a difficulty is always more or less met with in clefts confined to the posterior portion of the bone, and in acquired openings in the palate; and when such a difficulty presents itself it can only be overcome by lateral incisions, parallel with the margins of the cleft or openings.—*Medical Times and Gazette, Feb.* 8, 1862, *p.* 144.

153.—LALLEMAND, PERRIN, AND DUROY ON ALCOHOL.

The following excellent summary of the work of MM. Lallemand, Perrin, and Duroy, is from the Report (by M. Rayer) presented to the French Academy on the occasion of the presentation of a prize to the authors:—

Alcohol is a body whose study is of great interest as regards physiology, medicine, and hygiene. It has, hitherto, been the subject of experiments; but its real action upon the organism had yet to be shown. MM. Lallemand & Co. first sought for a means by which they might be able to recognise the presence of very small quantities of alcohol in the tissues and fluids of the body. Having obtained such a test, they then proceeded to follow the course of the alcohol into the body, its absorption by the digestive organs, its circulation in the blood, its localisation in certain tissues and in certain organs, and then its elimination by different parts of the body. They examined with especial care the question, whether alcohol, in its passage through the body, retains its chemical composition, or whether it is converted into products of combustion; whether, in fact, it behaves as food or as a non-assimilable substance, foreign to the body. These points are all treated experimentally by M. Lallemand and his associates.

By these experiments they establish, in the first place: that alcohol mixed with water is rapidly absorbed by the stomach, passes into the blood, and reaches the lungs, which are the chief organs of its elimination. A few minutes after the ingestion of alcohol its presence is noted in the air exhaled from the lungs; and the exhalation may continue for many hours, according to the amount of alcohol ingested.

Four men, each of whom had taken 100 *grammes* of brandy, passed their breath during four hours (relieving each other) through an apparatus. The liquid products of the condensation of the vapour thus introduced into the apparatus was twice distilled over quick lime, and yielded 4 *grammes* of a limpid liquid, having a distinct alcoholic odour. It was, in fact, diluted alcohol, and was capable of burning when heated. The alcohol also passes off with the urine. Three *litres* of the urine of four men who had taken three

bottles of wine, containing 10 to 12 per cent. of alcohol, and about 120 *grammes* of *eau de vie*, were collected. The urine was carefully distilled, and yielded 2 *grammes* of highly concentrated and nearly pure alcohol. Alcohol was also extracted from the blood.

In ascertaining this fact, the authors were led to a remarkable discovery, viz., that the blood was not the part of the body which contained the largest amount of alcohol. 240 *grammes* of alcohol of 21° were injected into the stomach of two dogs (120 into each), and in an hour and a half 700 *grammes* of arterial blood were taken from the carotids of the animals. It was then distilled, and gave 5 *grammes* of concentrated and nearly pure alcohol. When different parts of the bodies of these animals were subjected to distillation, it was found that the nervous tissues of the liver contained a larger proportion of alcohol than the blood; and that in the muscles only a trace of it could be found: the blood, for instance, containing 1, the liver containing 1·48, and the brain 1·75 proportional part. Alcohol, therefore, accumulates by a sort of affinity in certain parts of the body. The fact well corresponds with the well-known pathological effects produced in the liver and the brain by alcohol. Our authors then endeavoured to ascertain whether alcohol was decomposed in the body; whether, in fact, it could be considered as a respiratory food. They sought for the products of its combustion, aldehyde and acetic acid. But all their experiments were negative; and they therefore concluded, that alcohol must be considered as a non-assimilable substance, acting as alcohol in the body, and as a local excitant of the tissues. Hence, then, alcohol ingested is absorbed. Introduced into the circulation it pervades all the tissues; it accumulates in the liver and the nervous centres; it remains a long time in the body; it is eliminated as alcohol by the lungs, the skin, and chiefly by the kidneys. The localisation of the alcohol in certain organs explains its pathogenic influence over certain constitutional and organic diseases of the liver, the nervous system, and the kidneys.—*British Medical Journal, Feb.* 8, 1862, *p.* 152.

154.—ON PODOPHYLLIN.

By the Editors of the LANCET.

[Podophyllin is a preparation from the root of the Podophyllum peltatum. It has been known for more than forty years, but has not come into general use until now. It rather resembles jalap, but is slower in its action.]

Our lists of materia medica are so rich in alvine evacuants that, were it nothing more, the podophyllum might well be disregarded. But it is now known to have very different and extremely desirable properties in the treatment of diseases, as we shall proceed to explain.

First, however, we may notice that the Podophyllum peltatum, May-apple, or mandrake (which latter name it shares in common with other quite different plants)—belongs to the natural order *Ranunculaceæ*. It is found in great abundance in the northern states of America, from New England to Georgia, propagating itself rapidly by its roots, so that it presents two favourable conditions—cheapness and unmistakableness. Its fruit is sub-acid and agreeable to some persons, and is eaten with impunity under the name of *wild lemons*. The leaves are said to be poisonous and narcotic, but do not appear to have been investigated.

The remedial and physiological properties of this root are thus described by Dr. King, of Cincinnati:—"When fresh it is an irritant poison, producing hypercatharsis, hyperemesis, gripings, and other unpleasant symptoms. The violence of its action is much modified by drying and long keeping. It is still a violent cathartic and emetic in doses of thirty to sixty grains. In moderate doses it is cathartic similar to jalap, but acting more *slowly*. As a deobstruent, it is one of the most valuable in the materia medica, acting upon all the tissues of the body. Small doses, repeated at short intervals, to fall short of catharsis, will, in many persons, produce ptyalism. Its cathartic

action often requires twenty-four hours, and is accompanied by much griping.

"In chronic hepatitis there is not its superior in the whole range of medicines, being vastly more useful than mercurials, rousing the liver to healthy action, and maintaining an increased flow of bile longer than any other agent. In alterative doses it has been found valuable in scrofula, syphilis, rheumatism, and other chronic diseases. In constipation it acts without inducing subsequent costiveness. It has also been found useful in amenorrhœa, dysmenorrhœa, worms, and affections of the bladder. The dose of the root in powder, as a cathartic, is ten to twenty grains, of the tincture ten to sixty drops. As a sialagogue or alterative, three to ten grains of the powder, and five to twenty drops of the tincture."

The interest of these facts lies in their being a correct representation of the properties of a vegetable in its crude state which yields a preparation of so much greater value and importance, as we shall proceed to explain, than we have in podophyllum. We may, however, remark, that an acrid principle, not, so far as we know, yet isolated, is contained in the root, and attaches to the infusion and the tincture, which produces such extremely unpleasant sensations in the fauces and throat as to be compared to the burning and smarting which follow the swallowing of sulphate of zinc, and often induces nausea and vomiting, with great and distressing depression. From these circumstances we doubt whether, with all its virtues, the Podophyllum peltatum would have found a place in our materia medica had not the preparation we are about to describe been devised—namely,

Podophyllin.—This substance, which, according to the testimony we shall adduce, must be a most curious and valuable remedial agent, is one of the concentrated preparations described generally in a former paper. Unfortunately the name has been given to two essentially different things, which it is necessary carefully to distinguish.

1st. The term Podophyllin has been applied to the resin extracted from the root, and purified by the separation of all the other principles.

2nd. To a mixture or compound of these prepared by the following process:—The root, coarsely powdered, is treated with alcohol by percolation until exhausted. The saturated tincture is placed in a still, and the alcohol distilled off until the solution is left of a dark colour and the consistence of treacle; this while hot is poured slowly into three times its volume of cold water, and stirred briskly during the process. It is then allowed to stand twenty-four hours. Some muriatic acid is added to the supernatant fluid, which completes the precipitation. The precipitate is then placed on a linen filter, and washed repeatedly with cold water; then between folds of bibulous paper, and dried at a temperature between 65° and 90° Fahr. It is of a greenish white colour. If a higher temperature is used for drying, it becomes of a darker colour. This is the podophyllin of commerce. That sold by some makers has an olive-green tint, which is said to result from the use of a solution of alum, to effect the precipitation, instead of water only.

It is probable that in a few instances the pure resin has been employed, and the results have not been satisfactory.

The podophyllin of commerce is not wholly soluble in alcohol nor in ether, but both these solvents dissolve parts of it. It is insoluble in water, dilute acids or dilute alkalies, but it forms a saponaceous compound with pure alkalies.

Some pharmaceutists consider that it is solely composed of two resins; whilst others maintain that besides the resin which forms the greater part of its bulk it contains four other distinct resinoids, an alkaloid, and a neutral substance. The pure resin (the podophyllin of Mr. John Lewis) is said, when given pure, to produce sickness, hypercatharsis, and great griping; while to the other principles it owes its cholagogue, eliminative, and alterative properties; and it is alleged that the latter greatly and beneficially modify the action of the pure resin, and that the full therapeutical advantages are only to be obtained by the use of the compound. If podophyllin is prepared in this country, it is necessary to be extremely careful to obtain the

root properly and recently dried. The fresh root being too acrid, probably loses some volatile and offensive matter by drying; whilst if kept too long, it undergoes such changes as not to yield all its active principles to alcohol.

Podophyllin has been very extensively employed in America since the year 1847. Dr. John King, of Cincinnati, however, claims to have been acquainted with it, and to have used it in his practice, as early as 1835. He gives the credit of the recognition of the properties of the podophyllum peltatum, and many other American indigenous plants, to Dr. Tully, formerly professor in Yale College, Connecticut. The practitioners calling themselves "Eclectics" have written on the subject so extensively, and attributed to it such remarkable and valuable therapeutic properties, that their testimony might seem to be overstated and almost fabulous, did we not find sober-minded and scientific physicians equally enthusiastic on the subject. The sum of the expressed opinion of both parties is, that it exercises an influence on the human constitution, and is remedial in as wide a range of diseases as mercury, without having any injurious effects whatever. The primary actions they attribute to it are—emetic, cathartic, cholagogue, generally eliminative and therefore diuretic, diaphoretic, emmenagogue, sialagogue, &c. Hence it is powerfully alterative, capable often of arresting fevers by a single dose; of removing congestions of the brain, lungs, liver, or spleen; of reducing glandular enlargements; of affording great relief in inflammations of almost every organ; of curing rheumatism, jaundice, croup, habitual constipation, amenorrhœa, and, in minute and long-continued doses, many skin diseases, and even syphilis and scrofula. It is regarded as almost a specific in all disorders of the liver; as indispensable in all fevers—intermittent, remittent, and continued, but particularly puerperal; as valuable in pneumonia, bronchitis, and phthisis. Not only, say they, is it capable of effecting everything that is expected by the agency of mercurial preparations, but it is beneficial far beyond them, and in diseases for which they are inapplicable. Apologizing for thus extolling a remedy so recently discovered, beyond one which has been in the hands of the profession for ages, and the reputation of which is established beyond controversy, they nevertheless maintain that their eulogiums are based upon sufficient and careful observation. Were we to quote the authorities for all this to be found in the medical literature of America, we should be compelled to extend this notice far beyond the limits allowable in the pages of a periodical; and we therefore deem it better, and we trust it will be regarded by our readers as more satisfactory, to produce the experience of practitioners on this side of the Atlantic, who have for a long time employed podophyllin, confining our quotations to one or two notes relative to the manner of its administration. First observing that a correspondent of an American journal states that he has treated with complete success one hundred and twenty cases of syphilis with this medicine alone. The general statement of its being curative of this disease is made by many authors; and, although we have no precise evidence on the point except the above, we have reason to believe that podophyllin has found its way into the hands of several irregular practitioners in this country, who use it as a substitute for mercury. We should therefore commend it to the attention of gentlemen who have opportunities to verify or refute the allegation. 1. It is said that the purified resin of podophyllum, improperly called podophyllin, is escharotic, producing, when applied to the skin, small pustules which are difficult to heal, internally giving rise to extreme griping and violent catharsis. 2. That chloride of sodium given with podophyllin increases its purgative action to an undesirable extent, and should therefore be avoided, except in cold, phlegmatic habits, where the action of the drug is feeble. 3. That lactic acid counteracts its operation; and that sugar, sweetmeats, pickles, and like substances, should be avoided when it is administered. On the other hand, many writers recommend it to be well triturated with four or five times its weight of fine sugar. 4. That its use should be avoided in inflammatory states of the stomach or bowels, particularly of the mucous lining of the primæ viæ. 5. That a warm infusion of ginger is the best means of relieving tormina and griping when caused by its use. 6. That for an overdose, sour milk,

whey, or any form of lactic acid, are the best remedies. Lastly: That, as a purgative, bitartrate of potass, twenty to thirty grains, is the best adjuvant; while, when given as a cholagogue or alterative, it is well to combine it with minute doses of extract of belladona, hyoscyamus, or cannabis indica.

The doses of podophyllin are from one to three grains as a cholagogue and cathartic; half to one grain as a moderate purgative; one-fourth to half a grain as an aperient; a sixth to a quarter of a grain, three times a day, as an alterative.

Having briefly stated the American evidence relative to the remedial properties of podophyllin, we proceed with the observations made upon its use in this country. The following is from Dr. GARDNER:—

"My attention was drawn to the subject of podophyllin in the year 1856, by a brief paragraph in some periodical—I think American—stating that Dr. Glover, of Massachusetts, had found it to be a remarkable cholagogue in doses of one grain. I wrote to Dr. Glover, who kindly replied, with such further information as he possessed. A very small quantity only was procurable in London; I therefore imported several ounces from New York, manufactured by B. Keith and Co., and immediately commenced experiments with it on the persons of the *employés* of the Apothecaries' Company, with which I was then connected, and on my patients, and furnished several practitioners with examples.

"From 1856 to the present I have constantly employed podophyllin in my practice, and the result of my experience is as follows:—

"1. I know no other substance which so certainly produces bilious evacuations when the liver is full of bile; I do not even except calomel; a full dose—namely, two grains (when pure)—producing effects very similar to those resulting from ten grains of calomel. I have seen jaundice, where the stools exhibited no trace of bile and the skin and eyes were of deep yellow, cured by a single dose, incredible quantities of bile being evacuated. With respect to this action of podophyllin, I think I am warranted in asserting, strange as it may appear, that if, after a free evacuation of bile, a second dose of two grains is given within two or three days, it produces no effect whatever, not even purging. This certainly happens in ordinary engorgement of the liver, if not in jaundice. The purgative effect in nearly all the cases is not direct—that is, it does not act on the bowels for ten or twelve, or even in some cases sixteen or twenty hours; the purging appears to result from the large amount of bile thrown into the bowels. Sometimes its action is without uneasiness, but generally there is a sense of tormina or twisting and spasmodic action in the upper region of the abdomen and about the navel.

"2. In the torpid liver of persons who have resided in tropical climates, a dose of one grain seldom if ever fails to rouse the secretory action of the organ and bring bilious-coloured stools; but it often requires twenty-four to thirty-six hours to act on these patients. It is only at long intervals that the dose requires to be repeated. These patients are, as is well known, accustomed to take blue-pill, and often suffer much from nervous irritability, ulcerations of the mouth, diarrhœa or costiveness, or both alternately, depression of spirits, and many other evils, which all pass away after a dose of podophyllin.

"3. In the constipation which often besets patients in phthisis—as I think, most frequently from fatty liver—the podophyllin is the best aperient I have found, though, for the above reasons, very long in acting on the bowels. I have given it in all stages of this disease with marked benefit, not obtainable from any other purgative.

"4. I have given it much in gout, deeming it an important point of treatment to secure free biliary evacuations, which it invariably does; but I have not trusted to it alone, as I think might be safely done; and the same in acute rheumatism. My mode of giving podophyllin has been to make it into a small pill with soft extract of henbane, or one-eighth of a grain of extract of belladonna, or more frequently with another concentrated vegetable resinoid, leptandin, either of which moderates the tendency to harshness of the podophyllin.

"5. In constipation without other disorder, I usually give one-sixth to one-fourth of a grain in a pill of compound rhubarb-pill. It acts thus very much the same as we expect a grain of calomel or five grains of blue pill with the compound rhubarb-pill to act; and I consider that it may in very numerous cases be substituted for mercurials with great advantage.

"6. Whenever I have deemed it desirable to evacuate or stimulate the liver, as in bronchitis, fevers, headaches, &c., I have used this medicine with highly satisfactory results.

"7. Aware of the eulogiums which have been bestowed on podophyllin as an alterative, anti-syphilitic, &c., I cannot either contradict or confirm them from my own observations; but I have met with two or three cases where it unequivocally produced ptyalism.

"Lastly. On my recommendation it has been introduced to the Jewish Hospital at Jerusalem, where liver disorders abound, and I have the general report that it is found to justify the most sanguine expectations of its remedial value; and I may add, from many of my friends who have employed podophyllin, I have received opinions confirmatory of my views."

We have been also favoured with the following:—

"Dr. Ward has administered podophyllin during the past year at the Seamen's Hospital, 'Dreadnought,' and in private practice in a considerable number of cases. The affections in which he has found it useful are cardiac, renal, and hepatic dropsies, an asthenic form of dropsy frequent amongst sailors, and not associated with organic disease; congested and indolent liver. He has given it in doses of half a grain or a grain. Its action has been slow, ten or twelve hours having usually elapsed before it has effected evacuation of the bowels. In the majority of cases it has not occasioned discomfort in the way of nausea, griping, or depression. It is less hydragogue than elaterium, but never fails in its cholagogue action; producing copious bilious stools, semi-solid at first, subsequently watery. In two other cases, Dr. Ward has found it to linger for a considerable period in the bowels, and cause much griping and irritation; but these results might be obviated by quickening its action by combination with some other drug. Dr. Ansell informs Dr. Ward that he has given podophyllin in doses of a quarter of a grain twice a day in cases of amenorrhœa with constipation, and has found it act as a powerful cholagogue cathartic and emmenagogue. The drug may certainly be regarded as a valuable addition to our materia medica."

Dr. Ramskill, Physician to the London and Epileptic Hospitals, has favoured us with the following valuable remarks and cases. It will be observed that his experience of the effects of podophyllin is remarkably confirmatory of the opinions of the American physicians:—

"Podophyllin may be given alone, but it is apt to gripe. At the London Hospital I invariably order a pill of equal parts of podophyllin and extract of Indian hemp. It is then slower of action than when given with hyoscyamus or conium, but less frequently gripes. At the Epileptic Hospital I have not, for more than a year, used any one other pill as a purgative for epileptic patients, except under special circumstances. The formula I use is extract of belladonna and podophyllin, of each one-fourth of a grain, made into a pill with extract of minespermium fenestratum. The latter drug is rich in berberine, and is used as a tonic adjuvant. Belladonna has itself a laxative tendency; but it is not used for that property. One or two such pills are usually sufficient, if continued for two or more successive nights.

"The cholagogue action of podophyllin is not confined merely to the period of its administration; large quantities of bile will continue to flow for several days after ceasing its employment.

"Latterly, at the London Hospital, I have administered podophyllin in about fifty cases a week. In three cases only has its action been seriously complained of, the complaint being its excessive action, with pain. The dose was, in fact, too large—namely, half a grain, all the three patients being women. Except under such circumstances, the evacuations from podophyllin are very rarely watery; they are always fæcal at first, followed by the peasoup-like evacuations we see follow from the continued use of col-

chicum, but of a brighter colour. With regard to the colour of fæces generally, I must say that a brown colour does not, in my opinion, prove the presence of bile, for the dark epithelium thrown off in the colon may cause this; neither does a pale clayey series prove that the liver does not secrete bile, for this may be limited in quantity and absorbed for well known uses in the economy. (Liebig.) But a quart or more of a golden yellow fluid, yielding the usual reactions to bile-tests, will often follow the use, and demonstrate the cholagogue power of the drug.

"Podophyllin seems to me to rank near to scammony as a purgative, but it is milder in its action. As a cholagogue it stands pre-eminent and alone—far before mercury or any other drug that I have ever administered. I am almost tempted to say, that there is no real cholagogue known in medicine except podophyllin. Will calomel or blue-pill procure in any case an unequivocal discharge of bile apart from its purgative or laxative action—*i. e.*, apart from its derivative influence, accompanied by increased secretion of the small and large intestines? Is not this the mode in which the biliary secretion is liberated by these mercurials? Podophyllin, on the contrary, in very small doses, will procure an abundant flow of bile, and often induce its discharge by vomiting, before, or even sometimes without, any purging. My idea respecting the *modus operandi* of mercurials is supported by very good authority —(*vide* Morehead, 'On the Diseases of India.')"—*Lancet, Feb.* 22, *March* 15, 1862, *pp.* 209 and 286.

155.—ON PODOPHYLLIN AS AN APERIENT.

[The following is reported from Dr. Andrew Clark's practice amongst the out-patients at the London Hospital.]

If after the regulation of the diet, the sufficient use of fluid, kneading and friction of the abdomen, daily exercise, and the due solicitation of nature at regularly recurring times, the constipation remains, you must have recourse to drugs. Now the drug best fitted for the purpose, is that which will act without irritation, slowly, moderately, and by the production of a formed stool, after the manner of nature. The objections to ordinary laxatives are almost innumerable: sometimes they act too freely to permit of subsequent spontaneous regularity for some time, distension being required to excite the contractility of the bowels; at other times the bowels are teazed by frequent ineffective actions and mucous discharges are induced. In some cases the muscular contractility is exhausted, and the patient is afflicted with flatulent distensions of the bowels. In other cases the head is distracted with uneasy sensations; or there is a general nervous restlessness, for relief from which the patient flies to larger doses of more powerful purgatives, till at last life becomes little else than continuous suffering, complaint, and misery.

Now there is in my experience no single remedy for constipation so free from these objections as Podophyllin. It is not a specific for constipation, and it is not in all cases free from some of the objections attached to other remedies. But in the great majority of cases of simple constipation, it fulfils the conditions required of a safe and effectual remedy, by operating slowly, easily, and after the manner of nature.

Podophyllin (made by Davenport, of Great Russell-street, Bloomsbury,) is an amorphous resinoid powder, obtained by evaporation from an alcoholic solution of the root of the May apple (Podophyllum Peltatum). In America it is used in ten-grain doses as a cathartic, of similar character to the resin of jalap; but for use as a natural laxative, the maximum dose is one grain. If more than this is given, it produces griping and loose stools. For most persons a grain is too large a dose. It is better to begin with half-grain doses made into a pill with extract of taraxacum, which must be taken during breakfast, that it may operate next morning after that meal. For the first few days the operation of the drug may be accompanied by a little griping and by unformed stools. Should these continue to the fourth day, administer only quarter-grain doses, and combine them with an equal quantity of ipecacuanha.

If after, the drug does not operate in the manner described, it is not likely to prove successful, and had better be relinquished. When the right dose is determined, its use may be continued without fear of injurious consequences; and instead of requiring to be increased, may after a time be diminished without affecting the success of its operation.—*Medical Times and Gazette, Jan.* 4, 1862, *p.* 10.

156.—ON THE HYPODERMIC ACTION OF ATROPIA.

By JOHN KENT SPENDER, Esq., Surgeon to the Eastern Dispensary, Bath.

It is now about three or four years since the hypodermic plan of administering medicines began to be pretty generally employed by the profession. It was recommended by high scientific and practical authority. Physiology sanctioned it by declaring that when a fluid material is introduced into the areolar tissue under the skin, it must necessarily all be absorbed without the intervention of any disturbing chemical or vital agency. Therapeutically, the method in question was announced as an ingenious novelty, ready of application, easy in performance, and so speedy in showing its remedial powers, as to distance all other ways of "exhibiting" drugs, just as locomotion by steam distances all other locomotive processes. Then the cases cited in illustration were nearly always unqualified cures; there was no drawback in the action of the medicine, and seldom anything to mar the permanence of its success. Surely, pharmacy and poly-pharmacy were coming to and end; and the physician would henceforth travel about with his injecting-needle and syringe just as he has been wont to do with his stethoscope, and combine the functions of doctor and nurse, by not only prescribing a dose for his patient, but by administering it too.

And yet, if I am to credit the report of some of my professional friends, scarcely anything but disappointment has resulted in their hands from this much-vaunted "hypodermic" method of giving drugs. It is alleged that the effect of any drug so given is extremely uncertain; that unexpected and unwelcome actions are sometimes developed; and that even when a definite and lasting curative agency is evolved, it is occasionally at the expense of severe physical suffering of some kind. It is further asserted, that in a large percentage of cases no such curative influence is observed, but the patient is placed in a worse state than before; since, in addition to his malady, he has had to encounter the unfavourable workings of a medicine which might have worked unmixed benefit if it had been administered in the ordinary way.

About a year and a-half ago, I had the pleasure of reading a paper on the hypodermic action of morphia. In my hands, morphia so given had procured immense and rapid mitigation of neuralgia in various forms; in some instances, however, the relief was very transient, and in nearly all at the expense of severe nausea and vomiting. These unfavourable accidents of morphia led me to try atropia, the alkaloid of belladonna, as likely to fulfil the sedative effects of the morphia in a less qualified manner. In some respects, the change has been advantageous. The removal of pain has been sometimes as speedy, and there has certainly been less disturbance of the abdominal viscera. But the impairment of the functions of brain and of muscle has been much more considerable. Illusions of the senses, delusions of the imagination, disorder of the intellect, and torpor of the muscles, are the characteristic toxical properties of belladonna: they are still more characteristic of the inorganic essence of the vegetable; but when the alkaloid itself is administered in the hypodermic method, it requires only a very minute dose indeed to develope those phenomena in a very emphatic way. And they *must* be developed in a certain measure if we wish to gain any of the desired therapeutical benefits.

One grain of pure atropia may be mixed with exactly four drachms of distilled water, and kept in solution by two or three drops of dilute acetic acid. This will form a standard solution for hypodermic use. Five drops will be the usual dose for an adult female—at least for the first time; and seven

drops for the other sex under the same circumstances. This quantity may, in each case, be cautiously increased until three or four drops are added to the original quantity. If the dose be administered at bed-time, the patient will usually soon fall asleep; he will awake two or three times in the night, and feel thirsty and giddy (one individual, speaking, apparently, from a practical acquaintance with the toxic qualities of alcohol, said he felt "very tipsy"); there is not much perspiration, in this respect contrasting strongly with morphia; the pulse is rather slow and feeble; and the appetite is impaired for two or three days afterwards.

The most unpleasant physiological results of the atropia are the dryness of the fauces, and the sensation of enlargement of the tongue. The pupils are generally a little dilated. The intellectual hallucinations differ greatly; sometimes a person says he is flying about the air—sometimes he is tumbling off a precipice—and often there are strange and fantastic dreams. There has never been any approach to delirium from the moderate doses with which I have employed the medicine. But the alleviation of pain has been always very decided, and more lasting, though generally not so instantaneous, as after the use of morphia. I have tried both remedies at different times in the same person, and requested a comparison of their respective effects. One man, a dispensary patient, declared that the morphia seemed to "sweat the pain out of him," and to make him inexpressibly warm and comfortable for a time; but the pain gradually returned. The atropia caused him to feel very "dreamy and thirsty," but the relief was far more durable.

Is it necessary to inject the remedy over the seat of pain? Foreign and American writers testify in the affirmative; but most British practitioners seem to think it perfectly immaterial. My own limited experience does not justify me in forming a positive opinion, but I incline to the belief that, unless the pain is very superficial, the question is of no practical importance.

A mode of administering remedies which, according to the testimony of numbers of medical men, is often of most notable value, should not be allowed to fall into desuetude on account of its special disagreeable concomitants. Our efforts should be directed, I think, to the removal or diminution of these physiological unpleasantnesses; and to the elevation of the hypodermic art.—*British Medical Journal, Nov.* 24, 1861, *p.* 554.

157.—ON THE ACTION AND USES OF ATROPIA.

By Dr. Fleming.

[A memoir on the action and uses of atropia is said to be forthcoming from the pen of Dr. Fleming. He gives atropia in solution thus:]

Atropia, 1 grain; distilled water, 5 drachms. Dissolve thoroughly with the aid of a few drops of diluted muriatic acid, and add of rectified spirit sufficient to make 10 drachms. This solution represents all the medicinal virtues of belladonna; it keeps well; and being of uniform strength, is superior to the very uncertain tincture and extract.

The above solution is so proportioned that ten minims are the commencing dose for the adult, given in a little water, once daily at bed-time, on an empty stomach. The dose is increased daily by two or four minims, until the physiological action of the drug is produced. This is attained with much precision and safety. Dr. Fleming insists on the stomach being empty, or nearly so. The same dose which produces distinct effects, if so taken, produces none on a full stomach.

The more prominent physiological effects of atropia in medicinal doses are dry throat and mouth, difficult swallowing, wide pupil, and dim sight. To these are gradually added frontal headache, giddiness, slight relaxation of the bowels, lessened urine, and increased frequency of micturition, wakefulness, and slight delirium. The following are among the most important of its applications in disease. In epilepsy it is sometimes curative, and rarely fails to afford material improvement. In chorea and neuralgia often useful,

but, on the whole, less so than other medicines. In asthma it often gives great relief; but its value is, perhaps, most strikingly shown in relieving constipation, and in subduing irritability, congestion, and inflammation of the urinary systems. Hence its utility in irritable bladder, incontinence of urine, spermatorrhœa, irritable uterus, dysmenorrhœa, and other affections of those organs. Additionally, Dr. Fleming has seen it act as a parturient, with a promptitude and efficiency which he characterises as unequivocally superior to the ergot of rye. To avoid delay in inducing this effect, the drug is introduced into the cellular tissue, the solution employed being made of the same strength, but without spirit.—*Medical Times and Gazette, Dec.* 14, 1861, *p.* 618.

158.—ON THE USE OF NUX VOMICA AS AN ANTIDOTE IN CASES OF POISONING BY ACONITE.

By Dr. D. D. Hanson, United States.

[Experiments have been made on animals to ascertain the applicability of aconite in cases of poisoning by strychnia. The following case confirms the opinion formed of the antagonistic action of these substances.]

The subject of it was a coloured boy, five years of age, who surreptitiously took a mixture of tincture of aconite and simple syrup. When seen by Dr. H. he was "comatose, the eyes half closed, expressionless, the pupils insensible to light, though not much dilated. The pulse was feeble and irregular, respiration requiring artificial aid to support it, and the muscles and ligaments so much relaxed that he could neither stand up nor sit unless supported. His respiration finally degenerated to a gasp, occurring five or six times the minute, then he would convulsively straighten out in the lap of his attendant, throw his head and shoulders back, and his hands over his head, as if, mechanically, to get a longer and fuller inspiration, then relax into the same state as before.

"No time was lost in getting his feet into hot water, sinapisms on the soles of the feet, calves, and over the abdomen and chest. I failed in my attempts to get an emetic dose of mustard into the stomach, from its bulk and difficult deglutition. Ipecac and antimony being the least bulky of anything at hand, I forced down a double dose; soon after I irritated the fauces with a feather. Fifteen minutes passing, and no signs of vomiting having appeared, I repeated the dose, and irritated the throat as before. No retching occurred from this at the expiration of half an hour from the first dose, the respiration grew more difficult, and the pulse became imperceptible at the wrist. He was sinking, evidently, and the emetics were aiding the poison instead of the patient, as the muscular fibres of the stomach were rendered insensible to expulsive stimuli by the depressing influence of the poison, and the difficult respiration and deglutition were referable to the operation of the same cause upon the diaphragm and pharynx. The case now appeared desperate, unless these tissues could be excited, and nux vomica was manifestly capable of producing this effect, as its full therapeutic action was the exact opposite of that now dominant from the poison. Impressed with this idea, I gave him three drops of the tincture of nux vomica; I then placed my finger upon the wrist, and awaited the result. My pleasure can be well imagined when, in a few minutes, I felt the heart's impulse returning with accelerated vigour as the tincture became more and more absorbed, and the respirations were correspondingly improved in steadiness and depth. At the end of twenty minutes I repeated the dose, soon after tickling the fauces with the feather. Retching was soon induced, and vigorous emesis followed. After this operation, young ebony opened his eyes, and, after satisfying himself that matters were progressing circumspectly, he coolly lay back in the lap of his attendant, with a quiet and steady respiration and pulse. I remained half an hour longer, when I considered him safe, and left him, with directions to take three drops once in three hours during the night, allowing him to sleep during the intervals if the breathing continued regular. The next day I found him sitting in

a chair, and apparently fully recovered, having rested well during the night, and taken light nourishment during the day. I left him two-drop doses of the tincture for meal-times during three days, to ensure perfect tone of the muscles.

"As corollary to this, I think it may be said that nux vomica is a complete antidote to aconite, and, conversely, that aconite is equally an antidote to nux vomica."—*American Journal of the Medical Sciences, Jan.* 1862, *p.* 285.

159.—THE TREATMENT OF SUSPENDED ANIMATION UNDER THE INFLUENCE OF CHLOROFORM.

By Dr. William Marcet, F.R.S., Assistant Physician to the Westminster Hospital.

It has frequently occurred to me that, in many instances, the final cause of death from chloroform was owing, not only to its anæsthetic properties, but also partly to spasm of the glottis. I do not mean, however, that the passage of the vapour of chloroform through the glottis and larynx has the power of causing an involuntary closure of the glottis; and I cannot agree with Dr. Black, of St. Bartholomew's Hospital, who states: "Any concentration of the vapour of chloroform which can be breathed is safe; any condition of dilution which causes the patient to cough or hold his breath is dangerous, and, if persevered in for half a minute, may be fatal." I believe a spasmodic closure of the glottis to take place occasionally from the action of the chloroform which has been absorbed into the blood, and that this obstacle to the admission of air into the lungs, taken in connexion with the narcotic or anæsthetic effect of the chloroform circulating in the blood, has been in some, perhaps many, cases the actual cause of death. Should this view be correct, it will follow that, if air be allowed to enter the lungs by means of tracheotomy, or by opening the glottis with a trachea-tube, or any other operation which will effect the same purpose, life will in such cases be saved.

It may be inferred that spasm of the glottis takes place occasionally under the influence of chloroform.

1st. Because several substances possessing anæsthetic properties are positively known, when present in the blood, to have given rise to closure of the glottis.

2nd. Because the symptoms of death from chloroform are consistent, more or less, with death from sudden asphyxia.

3rd. Because the post-mortem appearances after death from chloroform may be accounted for by assuming that death has taken place from asphyxia.

If it be admitted that death from chloroform be occasionally owing to spasm of the glottis, then the importance of performing tracheotomy in these cases, and adopting some means of allowing air to enter freely through the wound, will be readily understood. It must be remembered that the cases on record, of recovery from suspended animation owing to an over-dose of chloroform are very few, and, as a rule, it may be considered that, after the respiration has ceased, and the pulse become hardly perceptible at the wrist, death is inevitable. Under these circumstances, any means apparently available should be adopted. The operation of tracheotomy ought to be performed as soon as possible after the respiration has stopped, and the patient assumed that livid countenance known in these cases to precede death: the loss of every second diminishes the chance of saving life. Artificial respiration, if possible, must, however, not be neglected, and should be carried on before, during, and, if necessary, after the operation of tracheotomy has been performed. Artificial respiration is of great importance in connexion with death from chloroform, and I am at present busily engaged inquiring into the subject.—*Med. Times and Gazette, Nov.* 16, 1861, *p.* 498.

160.—ON THE LITHIA SPRINGS OF BADEN-BADEN.

By Dr. J. Althaus.

The salts of lithia, into the pharmaceutical properties of which Lipowitz and Mr. Alexander Ure were the first to inquire, have recently been prominently brought under the notice of the medical profession, by Dr. Garrod, who recommends them in cases of uric acid diathesis connected with gravel, and in cases of chronic gout. This recommendation is based upon the fact that lithia possesses a great affinity to uric acid, and that the urate of lithia is the most soluble of all the urates.

Lithia is especially found in the lepidolite, a kind of mica which occurs in the granite of Bohemia, and from which it is prepared in chemical manufactories. From one of these fabrics, about forty pounds of carbonate of lithia are annually sent to this country, where the salts of lithia are now so much in demand that they can scarcely be supplied in sufficient quantities. The amount of lithia which had been found in mineral waters up to the present time, was very trifling; and it was the great rarity of this substance which prevented Mr. Ure from following up his researches on it. Quite recently, however, Professor Bunsen, of Heidelberg. has shown, by a new method of investigation, that lithia is, indeed, a substance which is most universally encountered in nature. It is well known that many substances, when introduced into a flame, have the property of imparting to the spectrum of the flame peculiar and brilliant rays, which are especially striking when the heat of the flame is very great, and its light insignificant. Upon this fact the Professor of Heidelberg has based a method of qualitative chemical analysis which has considerably enlarged the domain of chemical investigations and has allowed to solve problems which were hitherto believed to be unapproachable. By means of this method, Professor Bunsen discovered in the mineral spring of Dürckheim two new metals, to which he had given the names of Cæsium and Rubidium, which are closely related to potassium, and are the most electro-positive substances known. Concerning lithia, the same philosopher found that its compounds gave rise to two quite distinct rays, one of which is of a feeble yellow, and the other of a very brilliant red. By this method 1-100,000th of a milligramme of carbonate of lithia may be distinctly traced, and one-fourth of a grain of the same salt causes the red ray to continue for a whole hour. The nature of the compound in which the metal exists has no influence upon the position of the rays in the spectrum, it being the same with the chloride, the bromide, the iodide, the carbonate, the sulphate, the phosphate, and the silicate of lithia; but the intensity of the rays increase in proportion to the volatility of the compounds. Professor Bunsen has by this means discovered lithia, not only in triphylline, triphane, and petalite, but also in a large number of feldspaths, in the granite of the Odenwald, in a common spring at Schlierbach near Heidelberg, in sea-water, in fucus which had been carried by the gulf-stream to the shores of Scotland, in the ashes of tobacco, of grapes, of vine-leaves, of grain of every description, of the milk of animals which had been fed upon grain, &c. Lithia has also been discovered in the ashes of the human blood and muscles. A very large quantity of it was found in two of the thermal springs of Baden-Baden, namely, the Fettquelle and the Murquelle, of which the former contains 0·2315 grains of chloride of lithia in sixteen ounces of water, and the latter 2·3649 grains of it. In one hundred pounds of the salt extracted from the Murquelle nine and three-quarter pounds of lithia are contained; that is, a quantity of this substance worth £90 sterling. This amount is not equalled by that contained in any other mineral spring which has yet been examined.

In consequence of the analysis of these springs, made by Professor Bunsen, they have, during the last season, for the first time been extensively used in cases of gout and lithiasis; and I am indebted to my friend, Dr. Ruef, of Baden-Baden, who has treated a large number of cases of that description with them, for the following particulars regarding the results of their administration.

The physiological effects observed after taking the waters are as follows:—At first it promotes digestion, and a feeling of well-being is induced; but after they have been taken for some time, and especially when in large doses, sickness, disposition to vomiting, and diarrhœa ensue, which in most cases, however, gradually disappear, but sometimes continue as long as the water is drunk. A constant effect is an increased elimination of urine, the quantity of which is often doubled or even trebled; it becomes turbid after some time, and large quantities of a reddish sediment are deposited in it. In some of the patients treated by Dr. Ruef, profuse perspiration came on after from five to ten days, and continued as long as the water was drunk; in the case of a lady who had not freely perspired for years, this perspiration continued two months after the cure had been finished. It therefore appears that the water is a diuretic as well as a diaphoretic.

Concerning its therapeutic action, in almost all cases the pain in the joints increased at first (especially in those patients who were in the end cured) to a rather high degree, but it never spread to healthy parts. In joints which were perfectly contracted, crackling, dragging and pulling was felt, as if the articulation was being torn asunder: but after such an attack of pain a sensation of easiness and decided improvement was felt, and the mobility of the limb was much increased. In one patient, a physician from Epernay, a regular fit of gout came on during the use of the water, under the continued use of which this patient so rapidly improved that he could walk about again after three days.

Gouty affections of the joints, of the sheaths of the nerves and the muscles, if not of very long standing, were cured after three or four weeks, and have remained so up to the present moment. In periodically-recurring headache on one side, which is often due to gout, the effects were also very beneficial. A lady who had been contracted for fourteen years, and who could neither stand nor walk, nor carry a spoon to the mouth, was by the use of this water so much improved that she was able to walk a little, and to stand and eat by herself; while no former medication had relieved her. This patient also suffered from dysmenorrhœa, the most prominent symptoms of which were severe abdominal pain, oppression and asthma, cold and paralysis of the left arm. She had only taken the lithia water for eight days when the catamenia appeared, and were unaccompanied by any unpleasant symptoms whatever; nor did these latter reappear afterwards. In a male patient whose finger-joints were infiltrated with urate of soda, which was visible in white specks through the skin, these infiltrations were removed and the swelling diminished.

The mode of administering the water was as follows:—For patients with whom large quantities of water do not agree, five grains of the carbonate of lithia were added to a bottle of the water of Murquelle, which contains five grains of chloride of lithia; and the water was then impregnated with carbonic acid in order to render the carbonate more soluble. Of this water a tumblerful was drunk three times a day; and if an increase of the dose appeared necessary, two or three grains of the carbonate were added to every glass. If patients are able to keep much water on the stomach, they may take six or eight tumblersful of the Murquelle, without any artificial addition of carbonate of lithia. Baths with water of the same spring were also given; and the Administration of the Spas of Baden-Baden are now occupied in preparing a mother-lye from the water of the Murquelle, in order to gain so much of the salt that it may next year be added to the baths. Of all the antarthritic remedies offered by the springs of Baden-Baden, none have proved so beneficial as this lithia spring. Dr. Ruef has, in some cases, at the same time employed the Russian vapour-baths prepared from the steam of the hottest spring of the place, which has a temperature of 155° F.—*Medical Times and Gazette, Nov.* 23, 1861, *p.* 527.

161.—ON SULPHURIC AND GALLIC ACIDS.

By Dr. Lumley Earle, Birmingham.

Sulphuric acid and gallic acid are at present in high repute, and justly so, as anti-hæmorrhagic medicines; but as far as I have seen the practice of others, they are never given together. Now I have tried both remedies, singly and in combination, and beg to testify to the great superiority which the two acids possess in checking hemorrhage when given together, than when only one is administered. The superiority is so striking that there seems something more than the mere fact of two acids being given instead of one. My experience has certainly been only in hemorrhages from the uterus, whether from disease or during pregnancy; but as these acids are quite as effectual in arresting hemorrhages from other organs, probably when given in combination the effect would be as striking as it certainly is in sanguineous discharges from the uterus. The doses in which they are given of course depend upon the amount of hemorrhage, or the obstinacy with which it continues.

For ordinary cases which present no unusual severity, the following mixture is sufficient:—

℞. Acid. gallici 3 ss., acid. sulphurici dil. 3 j; liq. opii sed., 3 ss.; inf. rosæ co., ℥ vj. M. Cap. coch. ij. mag. tertia vel quarta quâque horâ.—*Med. Times and Gazette, Jan.* 11, 1862, *p.* 49.

162.—NEW REMEDY—A NEW SALT OF IRON.

By T. L. Gentles, Esq., Sheffield.

In the *Chemical News* of Sept. 21st, p. 158, there is an extract given from the *American Journal of Pharmacy* in which Herr Emile Heydenreich claims to be the discoverer of a new salt of iron—viz., the phosphate of the sesquioxide, soluble in a solution of citrate of ammonia.

I think it but fair to my friend, Mr. J. Lightfoot, jun., of Accrington-lane, to state, that more than two years ago he prepared in his own laboratory an iron salt identical with this. Finding, like Herr Heydenreich, that in addition to this salt being soluble in a solution of citrate of ammonia, it was also soluble in hydrochloric and nitric acid, Mr. Lightfoot thought it might be of considerable service to the medical profession. It was accordingly tried, but did not realize all the expectations of its talented discoverer. Mr. Lightfoot again instituted experiments, and at last succeeded in making a magnetic phosphate of iron—a salt somewhat similar in its composition to the phosphate of the sesquioxide, but, unlike it, much more soluble in either hydrochloric or nitric acid. Pursuing his experiments still further, Mr. Lightfoot found that *the* acid which most freely and readily dissolved the salt was the citric. The proportion of magnetic phosphate of iron which this acid *in solution* will take up is more than double that which is taken up by the solution of citrate of ammonia proposed by Herr Heydenreich; the proportion of the former being, if I remember rightly, forty-eight per cent., while that of the latter is only twenty-three per cent. You have thus a more active preparation, and one in which you can place greater confidence.

I may add that this citric acid solution of the magnetic phosphate of iron has been extensively prescribed in Lancashire. Its action in albuminuria, diabetes, and various forms of heart-disease is most striking; while, as a tonic, it can scarcely be surpassed. It does not precipitate with sy. aurantii like most of the iron salts; thus forming one of the most agreeable chalybeates imaginable. In proof of its physiological effects upon the system, the two following cases are adduced:—

E. C., aged fifty-six, labourer, had for many years been afflicted with valvular disease of the heart, with hypertrophy of the organ itself. He suffered at times most frightfully from palpitation, and had not been able to do any

work for the last three years and a half; the least exertion produced the most distressing effects; even walking across a room would render him completely helpless for several minutes. He was given five minims of the magnetic phosphate of iron three times a day; this was continued for a week, at the end of which he expressed himself as being vastly better. The treatment was pursued for a month, when the patient was able to resume his work, a thing he had not done for more than three years, as previously stated. The violence of the heart's action was greatly lessened: the man could walk six or eight miles a day without inconvenience, and gained considerably both in strength and weight.

S. H., aged 26, married, but no children, had since last March suffered more or less from severe pain in each renal region, œdema of the lower extremities, and general debility. The specific gravity of her urine was 1008, and it was perfectly coagulable at a temperature of 200° Fahr. She was ordered the following mixture three times a day: solution of the magnetic phosphate of iron, six minims: iodide of potassium, one grain; water, one drachm. After the first eight doses of this medicine the quantity of albumen was diminished to nearly one-half, and on the seventh day from the commencement of the treatment there was not a trace of albumen to be found. The specific gravity of the urine was now 1021; the œdema of the limbs was fast disappearing; the patient had regained her appetite, and in three weeks from this date was completely convalescent. Instead of the pale, weak, and dropsical patient you saw a month ago, you had now before you a woman in the enjoyment of excellent health, able to perform her household duties, and expressing herself as "perfectly well."—*Lancet, Nov.* 30, 1861, *p.* 534.

163.—ON CODEIA.

By Dr. Aran.

[The following are extracts from a letter written by Dr. Aran to M. Berthé.]

I have at present only employed this alkaloid to obtain ease or sleep, but from ten or twelve cases I have been able to discover in this agent sedative and narcotic properties, which appear to me to place it in the first rank among the best remedies of this kind.

Inferior to morphia for calming pain for this reason only, that a larger dose of it is required, it has a marked superiority over morphia, in that it never occasions a heavy and agitated sleep, that it does not bring on perspiration or eruptions of the skin, nor troubles the digestion, that it produces no obstinate constipation, no desire to vomit nor vomiting, no anæmia; so that the Codeia appears to me destined to be of great service in nervous diseases of the stomach, and I can assure you that we have obtained with it a relief in cases of gastrodynia, which had defied all other means, belladonna included.

But it is especially as a means of procuring a calm and restoring sleep that the codeia seems to me called to occupy an important place in therapeutics. Those stubborn and harrassing coughs of bronchitis, and particularly of consumption, those violent pains of rheumatism, gout and organic affections, of cancer, for instance, which disturb the sleep and frequently deprive the patients even of moments of repose, are forgotten in the midst of the calm and agreeable sleep which the codeia procures.

I have witnessed two very conclusive cases of incurable tumours, for which no means of relief at all lasting had been found. One of these tumours, of an enormous size, almost filled the pelvis and produced on the passage of the sciatic nerve pains returning about eight o'clock P. M., with such an intensity as to force cries from the patient, who only became calm towards the morning when she fell asleep overcome with fatigue. Two centigrammes of codeia* produced the first day a calm so complete that the patient thought

* One centigramme is equal to one-sixth of a grain.

herself cured, and for the first time since a month she was able to take a somewhat copious meal, her appetite having returned for the first time.

During the twelve days I attended this patient, the pains were almost null, and as soon as they reappeared she mastered them by a few centigrammes of codeia; it became, however, necessary to increase the dose by degrees, and from two centigrammes the patient increased the dose to ten centigrammes.

To explain how so small a dose produced a calm so complete, I must add that her weak state rendered her very sensitive to the action of codeia, which, on the other hand, appears to me able to be given from the first in a much larger dose than morphia. The dose of five centigrammes of it in the syrup, for example, we have seen calm the pains of gastrodynia without leaving the slightest trace of narcotism, and one of our patients takes at present every day fifteen centigrammes of codeia without any bad effect.

This patient, who has long made use of opium and morphia, established between these agents and the codeia a difference quite to the advantage of the latter, which does not agitate her at all.—*Medical Times and Gazette, Dec. 7, 1861, p. 599.*

164.—CASE OF POISONING BY ALMOND FLAVOUR.

By Dr. T. Herbert Barker, F.R.S.E., Bedford.

In the evening of the 6th of December last, I received an urgent message to see a young lad residing near to my house. The messenger said he had been poisoned by drinking "almond flavour." I took at once with me a bottle containing ipecacuanha powder. The little patient, a youth of 6 years of age, was much prostrated, very pale; the eyes bright, prominent, and glassy, and the pupils dilated. The pulse was small and quick. He appeared to have considerable uneasiness in the throat; but fortunately the power of swallowing had not been lost. A brimming teaspoonful of the powdered ipecacuanha was immediately mixed with warm water and given to him, and the cold affusion had recourse to. Within three minutes from the first dose, as vomiting had not commenced, I mixed another teaspoonful of powdered ipecacuanha with warm water, adding to it a small quantity of mustard and common table salt. I then gave him brandy and water at frequent intervals. Within two minutes after the second dose, free vomiting commenced. The ejected matters smelled strongly of bitter almonds. The cold affusion was persevered with, and the frequent administration of warm brandy and water, as well as the application of ammonia vapour to the nostrils. Free vomiting continued, and the ejecta became less and less flavoured with bitter almonds. Vomiting, however, was encouraged until the fluid introduced into the stomach was returned free from odour. The pulse rapidly improved, and in half an hour he was apparently out of danger. I sent him to bed, after directing him to have a foot bath, and a teacupful of brandy-gruel. The next morning he was running about.

Remarks.—The composition of "almond flavour," spirit of almonds, or essence of peach kernels, is usually of one drachm of the essential oil of bitter almonds to seven drachms of spirits of wine. I am informed by a respectable chemist and druggist in Bedford that he believes that this is the usual composition of the "flavour;" but that there is no authentic standard formula for its preparation. Dr. Taylor remarks: "It is sold to the public in quantities of not less than a quarter of an ounce, at the rate of one shilling per ounce, for the purpose of giving a pleasant flavour to pastry. It may be as well to state that one ounce of this almond flavour is, at the lowest computation, equivalent in strength to 250 grains of the Pharmocopœial prussic acid. In some cases, it may happen to be nearly equal in strength to this poison, and yet it is sold without restriction, and is entrusted in private families in the hands of ignorant cooks to apportion the dose which may give the requisite flavour to food!" (*On Poisons*, 2nd edit., p. 687.)

From the quantity known to have been in the bottle beforehand, the

mother of my patient has no doubt that he had swallowed two teaspoonfuls of the liquid. From the severe effects produced, and the intensity of the odour of the vomited matters, I think it probable that he did not swallow a smaller quantity than that. He drank it from the bottle, which is rather a wide-mouthed one, and from which it would flow readily. The smell and taste are not disagreeable, the latter somewhat pungent. Two drachms of the "almond flavour" would contain fifteen minims of the essential oil. Taylor mentions a case under the care of Dr. Bull of Hereford, in which seventeen drops of the essential oil killed in half an hour. Dr. Bull saw the patient in about fifteen minutes. (*Op. cit.*, p. 688.) This was the smallest dose of the oil known to prove fatal. In a case recited by Mr. Chavasse, where half a drachm or thirty minims of the oil had been taken, recovery took place with difficulty.

Fatal cases not unfrequently occur from the administration of the essential oil of bitter almonds, in mistake for the oil of almonds. Two such cases have been recently recorded in the *Pharmaceutical Journal*. In both cases, the essential oil had been used instead of the oil of almonds, and mixed with the syrup of violets. In the one case, a teaspoonful of the mixture proved fatal in a quarter of an hour to a child fifteen months old. (*Pharm. Jour.*, May 1859, p. 583.) In the other case, a "small" dose of the mixture proved fatal in three-quarters of an hour to a child nine months old. (*Pharm. Journ.*, Jan. 1860, p. 389.)

There can be but little doubt that the life of this boy was saved by the speedy emetic effect which was fortunately produced at a very early period after the poison had been swallowed. He lived within a few yards of my house, the messenger brought a precise and intelligible account of the mischief, and I did not stop even to weigh or measure any emetic, but took down and ran off with an ipecacuanha bottle. I probably saw him within five minutes of the accident. In any similar case, I would recommend the fullest possible dose of an emetic to be administered as early as possible, and the emetic effect to be encouraged in every possible way. I have a high opinion of the cold affusion, and of the restorative effect of ammonia applied to the nose. These may be applied in the intervals between the administration of the emetic doses, but by no means in lieu of them. There can be no safety but in the *thorough emptying and washing out of the stomach*. If the power of swallowing has been lost, the stomach-pump must, of course, be used.—*British Medical Journal, Dec.* 21, 1861, *p.* 655.

165.—*Biniodide of Mercury Ointment in Goitre.*—[Mr. Thos. Stainthorpe, of Hexham, offers his testimony to the efficacy of this treatment of goitre (see page 185). He says:]

For about twelve years I have been in the habit of prescribing it. My plan, hitherto, has been of having it freely rubbed in the part, night and morning; after each application the enlargement was covered by oiled silk; in a few days the full effect is produced, when the biniodide ointment is laid aside and simple spermaceti ointment and oiled silk substituted. If the enlargement is not thoroughly dispersed, the same mode of treatment is resumed until the results are satisfactory.

I have never seen any injurious effects from the use of this ointment, but on the contrary have always had every reason to be satisfied with the beneficial effects. The proportions I usually apply are biniod. hydr. ʒj., auxungiæ ʒj.; in some rare cases it has been requisite to use it stronger.—*Med. Times and Gazette, Dec.* 7, 1861, *p.* 599.

166.—ON VESICO-VAGINAL FISTULA.

By I. Baker Brown, Esq., Senior Surgeon to the London Surgical Home.

[Mr. Baker Brown has now operated in a total number of forty-nine cases of vesico-vaginal fistula. Of this number forty-seven have been cured, and two died.]

As there may be many who have not witnessed the operation for vesico-vaginal fistula, I shall, previous to relating the cases, say a few words as to the present mode of operating on these cases. There have been many improvements and alterations suggested; but I think it will be agreed that the plan I at present pursue is about as easy and perfect as can possibly be.

The patient being placed under the influence of chloroform in the lithotomy position, or without, on her hands and knees, the bent speculum is introduced, and, if in the former position, the urethra held up by a pair of vulsellum forceps. The first step is to pare the edges of the fistula. This is done by knives made for the purpose, one for the right and the other for the left hand; the fistula being held meanwhile either by a pair of forceps, or transfixed by a curved needle. The latter is perhaps preferable, as, besides everting both edges of the fistula, it has the advantage of having a needle inserted ready to be armed in the second stage. This consists in passing needles, made on the same principle as Startin's; but instead of being of flexible material, they are made of rigid steel, and with various curves—fourteen in all. The needle is passed within a quarter of an inch external to the denuded surface, and thrust through the coats of the vagina and the muscular tunic of the bladder, avoiding its mucous lining, and out again on the opposite side at the same distance from the fistula. The wire having been previously passed, the needle is drawn out, the wire being held in by forceps to be fastened. This is extremely simple. I have used shots and buttons, and have invented clamps, &c., but I have come to the conclusion that nothing is so easy or effectual as simply to twist the wire round and round with the fingers, or if too far to reach thus, by Weiss' self-holding forceps. The ends of the wire are cut, and the operation is complete. No dressing is required. The patient is placed in bed on her side, with her knees drawn up. A male elastic catheter, without the stilette and with bag attached, is introduced, and the patient left quiet for ten or fourteen days; when, to take out the wires, one has only to cut them on one side of the twist, and draw them out with forceps in the ordinary manner.—*Lancet, Nov.* 22, 1861, *p.* 494.

A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE PRECEDING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

ALBUMINURIA IN FEVER.—When, during an early stage of the fever, the urine becomes scanty and bloody, or highly albuminous, there are three practical points to which attention should be directed. First, give a guarded prognosis, for the complication is a serious one. Second, give alcoholic stimulants more sparingly and cautiously than when the kidneys are unaffected, otherwise the renal congestion is increased, and with it its effects, stupor and other cerebral symptoms. Third, to lessen the kidney irritation counter-irritate over the loins by mustard poultices, or better, by dry cupping repeated once or twice a day. (Dr. G. Johnson, p. 30.)

CONTINUED FEVER.—In cases of continued fever, a large amount of ammonia is constantly being formed and given out from the body. This is invariably so in all cases in which the destructive metamorphosis of the tissues overbalances the assimilating powers, as seen after prolonged exertion; but it is especially the case in fevers. Dr. Richardson goes so far as to attribute to this super-alkalinity of the blood the special typhoid symptoms. He supports his hypothesis on the experiment of inducing the symptoms, or something resembling them, by the injection of ammonia into the veins of an animal. Very pleasing results ensue in such cases of fever from the administration of small doses of acids. The following is the best formula: ℞. Acidi hydrochlorici diluti ♏xx; syrupi ʒj; aquæ ad ℥j; alternâ quaque horâ sumat. In mild cases, the tongue begins to clean immediately, the thirst and diarrhœa much abate, and the repugnance to food is diminished. The hydrochloric acid is peculiarly suited to the purpose, being as it were a natural constituent of most of the tissues. It is a powerful arrester of decomposition of animal matters, both in and out of the body. The blood in cases of scurvy and purpura is very nearly chemically the same as in cases of low fever: everybody treats these chronic affections with acids, why not also treat in the same manner an acute affection which corresponds with them in one point at any rate. Hydrochloric acid is so large a constituent of the body, that it may almost be called a food rather than a medicine. As to the use of alcohol in fever it is well to be guided by the condition of the nervous system. A tremulous state of the muscles, marked especially by a quivering of the hands and fingers, is a good test of the necessity for it. If congestive pneumonia come on, do not hesitate to abstract a little blood locally; great relief will follow this, and as much relief seems to result from the same treatment when there is much abdominal congestion. This treatment appears inconsistent with the feeding and stimulation required. But "all blood is not blood," and the loss of a little of the half-dead circulating fluid of fever is no loss really, and is amply compensated for by the additional nutriment which a small blood-letting will enable to be absorbed. (Dr. T. K. Chambers, p. 25.)

DEBILITY FROM PURULENT DISCHARGES.—*Hypophosphites.*—Purulent and mucous discharges are remarkable for the amount of corpuscles they contain "loaded with phosphates." It is not surprising, therefore, that cases in which there is prolonged and excessive purulent or mucous discharge of any kind, should be attended with considerable debility, and that the accompanying fever should be more frequently a hectic response than inflammatory. In the treatment, one indication is to supply the waste of phosphates, and the alkaline hypophosphites may be given with great advantage, exactly as in cases of phthisis. (Mr. J. Taylor, p. 385.)

EPITHELIAL CANCER.—*Pernitrate of Mercury.*—The solution of pernitrate of mercury is easily made by dissolving red oxide of mercury in hot nitric acid. The local application of this agent is productive of the most striking effect in epithelial cancer. So far as the affection is curable by its perfect local eradication, the solution of pernitrate of mercury can effect a cure. Apply the solution abundantly over the surface of the growth. It gives great pain, but only for an hour or two. It has the effect of destroying a layer of the diseased growth, which comes away as a slough about the third day. The surface must be soaked with the solution about twice a week, until the whole cancerous tissue is destroyed. The wound heals with the greatest facility, leaving scarcely a small firm cicatrix. The remedy destroys the diseased parts, whilst it seems quite powerless over the healthy tissues, except that it appears to quicken their healing energies. (Mr. J. Gay, p. 31.)

Arsenical Mucilage.—The author has "not yet seen a single instance of the disease returning" in any case treated by this method. First, prepare the following mucilage: Arsenious acid and gum arabic powder, of each one ounce, mixed up with five drachms of water. With this, paint over every morning a small portion of the cancerous mass. As the part becomes deadened it must be allowed to slough off, aided by a simple bread poultice. When all the diseased part has been thus got rid off, a carrot poultice should be applied during the night and a weak black lotion during the day (calomel one drachm, lime water one pint), until the part is entirely healed. The following medical treatment is most advantageous:—In persons of general good health, bicarbonate of soda, fifteen grains, night and morning; in persons of leuco-phlegmatic habit, one soda powder every night on going to bed, and the following draught twice during the day:—Hydrochloric acid, one drop; compound tincture of cinchona, one drachm and a half; to one ounce and a half of water. Full diet, with malt liquor, I consider to be absolutely necessary, avoiding spirits and tobacco, but wine I recommend to those who are in the habit of taking it. It is by this mode of treatment I consider such a change is produced in the constitution as to arrest and even destroy a cancerous action, as I have not yet seen a single instance of the disease returning in any of the cases thus treated. There are several cases now under treatment, which can be seen by any medical man who may desire to visit the hospital. (Dr. W. Marsden, p. 33.)

GOUT.—*Lithia Water.*—It will be remembered that the use of lithia in cases of uric acid diathesis was recently brought under the notice of the profession by Dr. Garrod (see 'Retrospect,' vol. xli., p. 35). This recommendation is based upon the fact that lithia possesses a great affinity to uric acid, and that the urate of lithia is the most soluble of all the urates. Lithia has been discovered in the ashes of human blood and muscles, so that it is not an ingredient foreign to the body. Fortunately, a large quantity of it has been discovered to exist in two of the thermal springs of Baden-Baden—namely, the Fettquelle and the Murquelle. A constant effect of drinking these waters is an increased elimination of urine, the quantity of which is often doubled or even trebled; it becomes turbid after some time, and large quantities of a reddish sediment are deposited in it. Profuse perspiration, sickness, and diarrhœa also occasionally result, especially when the water has been taken for some time in large doses. In most cases of gout, the pain in the joints is increased at first, but most are cured within three or four weeks, if not of very long standing; and even in those cases in which the finger-joints are enlarged from infiltration with urate of soda, much bene-

fit results, the swelling diminishing considerably, or being entirely removed. For administration in this country the best way is to dissolve about five or six grains of carbonate of lithia in a tumbler full of water, and drink this three times a day; the dose may be increased if necessary to eight or ten grains. As the carbonate is more soluble in water impregnated with carbonic acid, soda water might be used. (Dr. Althaus, p. 281.)

MUSCULAR RHEUMATISM.—*Acupuncture.*—In cases of muscular rheumatism, in which the pain is frequently severe and constant, and the remedies in ordinary use very ineffectual, insert a number of acupuncture needles deeply into the affected part, and allow them to remain about an hour. The result is frequently a speedy and complete cure. If there is any rheumatic paralysis remaining, galvanism is the best remedy to employ. Rheumatic paralysis is owing to a peripheral affection of the motor nerves. (Dr. A. Leared, p. 35.)

REMITTENT FEVER.—*Anarcotine.*—Anarcotine (previously called narcotine) is obtained from the residue of opium left after separation of the morphia. It has not been used at all in England therapeutically, and yet it possesses properties well worthy our attention. Dr. O'Shaughnessy in the Indian Annals of Medicine states, that it is of such value in remittent fevers, that he gives it in the full expectation of correcting the next periodic return of the fever. It is, he considers, a more powerful antiperiodic than quinine. It produces a degree of general heat in the system when first administered. It does not produce any organic disturbance. It fails in only 3·6 per cent. of all cases treated by it alone. In small doses it acts as a tonic, increasing appetite, and improving the tone of the system generally. For this purpose, from half-a-grain to a grain of the sulphate, combined with a slight excess of sulphuric acid, is a sufficient dose. As an antiperiodic, from a grain and a half to six grains is the proper dose. (Dr. A. Garden, p. 29.)

RHEUMATIC FEVER.—*Stimulants.*—Cases of rheumatic fever are constantly occurring in weak, insufficiently-nourished people, complicated by a low form of pneumonia and by pericarditis. The quantity of effused fibrine into the pericardium is not very great, and the pneumonia is not very extensive and affects the surface of the lung chiefly. There is great prostration and profuse sweating. These cases require the free and timely use of supports and stimulants, combined with the administration of alkalies and opium, which latter drug may be given in comparatively large doses without exerting its ordinary action on the system. There is a type of rheumatic fever precisely the opposite of that described, but it is seldom seen in large towns. The patients are comparatively strong and well-nourished. The skin is dry. Fever runs high. The quantity of lymph in the pericardium is often enormous. In these cases take blood, mainly in the hope of favouring sweating and the action of the kidneys. Without venesection, alkalies and sudorifics would be employed in vain. (Dr. L. S. Beale, p. 37.)

SMALL-POX.—*Indian Remedy.*—The North American Indians have a remedy which is believed by them to be always and invariably successful in "killing the disease." This is an infusion of the root of the "*Sarracenia purpurea*," or pitcher-plant. The effect of the first dose is, to bring out the eruption. After a second and third dose, given at intervals of from four to six hours, the pustules subside, apparently losing their vitality. In a subject already covered with the eruption of small-pox in the early stage, a dose or two will dissipate the pustules, and subdue the febrile symptoms. The Indians keep a weak infusion of the plant constantly prepared, and take a dose or two during the day to "keep the antidote in the blood." (Dr. M'William, p. 38.)

TROPICAL DISEASES.—*Quinine.*—In nearly all tropical diseases, attention once directed to the point will rarely fail to detect a periodic type. For instance, in dysentery a patient will have a rapid succession of bloody stools, with aggravation of tenesmus, and great prostration, at some certain period of the twenty-four hours. In such cases, one or two ten-grain doses of quinine, given in the interval, will often ward off the attack, just as it would that of ague. The malarious type of dysentery must be regarded and treated

as essentially fever, with disease of the bowels superadded. One or two large doses of quinine must be given, and followed by as much ipecacuanha as the stomach can be got to bear. Opiates are also useful, and as much nutriment must be given as can be assimilated. Acute tropical capillary bronchitis is likewise essentially fever, and will frequently subside rapidly, if, after a smart emetic, frequent doses of quinine combined with ipecacuanha are given. The old plan of calomel and antimony too frequently resulted in death. (Dr. J. C. Cameron, p. 27.)

TYPHOID FEVER.—Typhoid fever is essentially contagious, and is propagated solely by contagion. The sewers and other places into which the virus from the bodies of patients passes, are the principal channels through which the fever is propagated; and *they propagate it solely in consequence of being the channels for the diffusion of this poison.* Typhoid fever is no more the offspring of common sewerage than mildew is the actual offspring of damp and decay. (Mr. J. Simon, Dr. W. Budd, p. 21.)

To Arrest Contagion from the Intestinal Discharges.—Place two ounces of caustic solution of chloride of zinc in the night-stool on each occasion before it is used by the fever patient. The discharges may thus be "entirely" deprived of their contagious powers. (Mr. J. Simon, Dr. W. Budd, p. 21.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CHOREA.—*Aniline.*—Aniline is one of the class of artificial alkaloids. It is contained in coal-tar, and may likewise be obtained from indigo by distillation. A sulphate of this alkaloid given in doses of one grain three times a day, and gradually increased, is of marked benefit in cases of chorea. The violence of the movements decidedly diminish and a gradual improvement ensues. When the dose is increased, some depression is produced, and frequently a peculiar blueness of the lips. (Dr. J. Turnbull, p. 42.)

DELIRIUM TREMENS.—The following will generally procure refreshing sleep and relief of nervous excitement. Make an infusion of two scruples of cayenne pepper in a pint of boiling water, strain it when cool, and make it into punch, adding sugar and lemon to suit the taste. It is very palatable, and will be taken by the patient ad libitum. (Mr. C. Ferneley, p. 53.)

EPILEPSY.—*Bromide of Potassium*—Bromide, like iodide of potassium, is sometimes of signal use in cases of epilepsy. Probably these cases depend on a local affection of the bones or membranes, which the remedy removes by its absorbent powers. Five grains may be given three times a day. (Dr. S. Wilks, p. 49.)

NEURALGIA.—*Valerianate of Ammonia.*—The valerianate of ammonia is a remedy of great use in some of the severer forms of neuralgia. If retained in a state of crystallization, it rapidly decomposes and is uncertain in its action. It should be kept in solution. The solution must be of the strength that twenty grains of the salt may be given as the smallest dose. It may be given in this dose every two hours, in infusion of valerian as a vehicle. (Dr. O'Connor, p. 48.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

DROPSY FROM HEART DISEASE.—*Elaterium.*—In these cases when the legs are enormously swollen, and the dyspnœa so urgent that the patient is unable to lie down, what can we do for the patient? The disease is mechanical, and the remedy must be likewise mechanical—we cannot raise the machine to the work, we must lower the work to the machine. Direct diuretics are useless, but by hydrogogues, and especially elaterium, floods of water may be draughted away. The patient is less exhausted by a good dose every other day, than by smaller doses more frequently repeated. Give a third or even half a grain about five o'clock every other morning. By ten or

eleven o'clock an immense watery defluxion will have taken place, leaving the patient exhausted, but wonderfully relieved. It is well, however, to give a much less dose at first, because different individuals tolerate it so differently. A stimulant may be given at the same time as the hydrogogue. Under this treatment patients will often even gain strength, the dropsy disappears, the œdema of the lungs vanishes, and the patient regains a fair state of health. (Dr. H. Salter, p. 53.)

PLUGGING THE POSTERIOR NARES.—Prepare a few slips of lint about a foot long and half an inch wide. Double about two inches of the first slip over the end of a strong director and pass it along the floor of the nasal cavity as far as the posterior nares, then pack the rest of the slip in, but the extremity must be left projecting out of the nostril for the purpose of more easy removal. (Mr. J. Smyly, p. 131.)

Push through the anterior nares backwards the finger of a thread glove or a piece of linen sewed in this shape, then pack it with lint or some such material and draw it forwards so as to press on the surrounding parts. The only disadvantage attending this plan is the difficulty of unpacking it. (M. M. Desnult and Larrey, p. 132.)

POPLITEAL ANEURISM.—*Flexion of the Limb.*—In a healthy arm or leg, the effect of forcible and complete flexion is the considerable retardation of the blood flowing through the main artery, and the almost entire extinction of the pulse. In cases of aneurism, the following plan of treatment may be adopted: Roll the leg in a flannel bandage from the foot upwards, stopping below the tumour so as not to compress this. Bend the leg upon the thigh and retain it in this position by bandages attached to the ankle and leg. Confinement to bed is not now necessary. The complete flexion cannot be borne continually, so that it is necessary to relax a little occasionally. On the fourth or fifth day the tumour will usually be found solidified. (Mr. E. Hart, p. 126.)

New Mode of Compression.—Place the patient in bed, on a firm mattress, in a half-sitting position. Let the thigh form an inclined plane in the opposite direction to that of the body, the leg and foot being placed horizontally on a soft pillow, in a frame or cradle raised some ten or twelve inches above the surface of the mattress, and the bed-clothes being kept from the foot by a foot-board. Now, apply a tourniquet to the groin, and another to the upper border of the popliteal space, care being taken not to make any painful amount of pressure on the nerve. From these arrangements the regulated but gradually increasing pressure on the inguinal artery, while reducing the stream of blood in that vessel, forces a larger stream into the internal iliac, and its large and ultimately anastomosing branches. The same applies to the pressure made on the upper part of the popliteal artery. Thus every part of the arrangement is made to harmonise. The pads used must not be broad, but elongated. (Dr. Bland, p. 130.)

STYPTIC.—*Sulphuric and Gallic Acids.*—The anti-hemorrhagic properties of these two acids are justly in high repute. They are never given together; yet when combined they act with far greater certainty and efficiency than when given separately. For ordinary cases give the following mixture: ℞. Acid. gallici ʒss., acid. sulphurici dil. ʒj; liq. opii sed., ʒss.; inf. rosæ co., ℥vj. M. Cap. coch. ij. mag. tertia vel quarta quâque horâ. (Dr. L. Earle, p. 283.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ARTIFICIAL RESPIRATION.—*New Instrument.*—First make a small pair of bellows capable of containing about forty cubic inches of air. A small valve should be made in the pipe, in order that the backward passage of air from the lungs may not be instantaneous. This may be effected by a small conical brass valve, moveable by the operator. The reason that instruments for effecting artificial respiration are not in much favour, is, that there has never been any effectual plan introduced for connecting the pipe of the bellows with the larynx so accurately that air does not return through the mouth in-

stead of being forced into the lungs to inflate them. This may, however, be effected by surrounding the canula introduced into the larynx with a piece of sheet caoutchouc accurately fitting to it, and surrounding it, except quite at the end. By means of a smaller tube passing along the concavity of the canula, and into the interior of the caoutchouc cavity, this elastic material may be expanded to fill accurately the interior of the cavity into which it is introduced. The price of the whole is about two pounds, and it can be carried in the coat pocket. (Dr. W. Marcet, p. 78.)

DEATH FROM CHLOROFORM.—*Tracheotomy.*—The cases on record are very few of recovery from suspended animation owing to an over-dose of chloroform, and as a rule it may be considered that, after the respiration has ceased, and the pulse become hardly perceptible at the wrist, death is inevitable. Now in many, if not in most cases, death is owing to spasm of the glottis, and tracheotomy gives us our best chance of restoring the respiration. It should, therefore, be performed in all cases immediately on the cessation of respiration, and when the livid tint of skin appears which is known to precede death. Do not hesitate a minute; the loss of every second diminishes the chance of saving life. Artificial respiration must also be employed; it is of the greatest importance in threatened death from chloroform. (Dr. W. Marcet, p. 280.)

HÆMOPTYSIS.—*Gallic Acid; Acetate of Lead.*—Gallic acid is a powerful styptic, given in ten-grain doses. It may be combined with antimony, opium, or digitalis, according to the case. The combination with antimony is practically useful when there is much excitement. Acetate of lead must be given in three-grain doses every half hour or hour; and the alimentary canal must be swept out with sulphate of magnesia afterwards to prevent any symptoms of lead poisoning. The lead is of but little use in smaller doses. (Dr. C. J. B. Williams, p. 80.)

Digitalis.—There are three forms of tubercular hæmoptysis of very different frequency and gravity. The afflux or active congestion of the first stage of tubercular deposit, and the obstruction and erosion of the second stage, affect chiefly the smaller vessels of the lungs. The large ulceration of the third stage occasionally lays open a blood-vessel of considerable magnitude. The first two varieties, which are also the commonest in a very large percentage of cases, cease spontaneously, and the endless list of drugs recommended by different writers (because *after* their administration the bleeding has ceased) are mostly worthless, and, if not so, are seldom or never required. It is in the terrible hemorrhage of the larger eroded vessels, and therefore of what is advanced phthisis, that we have the most searching test of the value of any drug of this kind. It is often impossible to predicate whether a slight oozing is not the commencement of a more considerable hemorrhage. Ipecacuan given in small and very frequently repeated doses, often produces protracted and severe exhaustion and nausea, and, moreover, it frequently utterly fails in moderating such a hemorrhage. The only remedy to be relied on is digitalis. Of this give from thirty to ninety minims of the officinal tincture daily, in four or six doses. Unless you can watch its effects, it is better not to administer it at all, as it is too much of an edge-tool to be carelessly handled. It is often not the robust, strong man who bears or requires the strongest dose. The patient must be kept quiet, and in the recumbent position, and when the pulse has become very slow, reduce the dose of the drug. Tincture of digitalis combines well enough with dilute sulphuric acid during the attack, and with tincture of iron during the convalescence. (Dr. W. Brinton, p. 66.)

PHTHISIS.—*Hypophosphites.*—The diversity of opinion amongst medical men respecting the merits of the hypophosphites in phthisis arises from their using different salts—*i.e.*, whether a hypophosphite or a phosphite, or a phosphate, more or less combined with a carbonate—the article of commerce being a combination of these. A hypophosphite is the most soluble in the animal secretions of all the oxides of phosphorus, and is at once admitted into the venous system. If a dose of *pure* hypophosphite of potash or soda be given, it excites (even two grains only) a feeling of nausea, a slight pain

within the chest much increased by repetition of the dose. Dr. Churchill asserts that the physiological action of the hypophosphites is two-fold: "They increase nervous force, and are the most powerful hæmatogues—possessing all the therapeutic properties of phosphorus, without the danger formerly attending its use. Experience has not confirmed Dr. Churchill's further eulogium, that the hypophosphites are curative in every stage of phthisis." The word *palliative* would be more in accordance with fact. In the earlier and middle stages of phthisis, the guarded introduction of an alkaline hypophosphite into the blood produces a glowing influence, as a respiratory excitant, expanding the chest; and, as a pyrogenic, increasing animal heat and nervous force, and removing erratic pains. It moreover increases appetite and cheerfulness, and controls expectoration, night sweats, and diarrhœa. It is well to combine the salt with a mild tonic, as tincture of gentian, or an anodyne, as a little morphia; otherwise a genuine alkaline hypophosphite is repulsive to the nerves of the stomach, and the patient refuses to take a second dose. (Mr. J. Taylor, p. 258.)

TRACHEOTOMY.—Between the lower edge of the cricoid cartilage and the upper border of the isthmus of the thyroid, a space exists in many subjects uncovered either by muscles or thyroid gland. It is true that sometimes a very small artery crosses the space from above downwards, and that occasionally a congeries of veins exists; yet upon the whole these obstacles are far less than are met with in the space below the gland, and nearer the sternum. Chloroform, as in other operations for dyspnœa, is inadmissible. The instruments required are a fine narrow scalpel with thin ivory handled-forceps, a stout hook, and proper canulas or elastic tubes. Assistants, with retractors, are required. After division of the integuments, keeping to the middle line, tear your way down to the trachea, using the nail of the left fore-finger and the handle of the scalpel. When the trachea has been fully exposed, seize the trachea with the hook, steady it, and raise it slightly whilst the requisite incision is made and the canula introduced. The hook used must be stronger than an ordinary tenaculum; it may be single or double. When foreign bodies are lodged in the lower part of the windpipe, we may, if there is no hurry from the presence of urgent symptoms, give chloroform and dissect carefully to expose the trachea low down to a considerable extent, in order to make an opening sufficiently large. But when the symptoms are pressing and desperate, we must act promptly, and must proceed regardless of hemorrhage. The upper part of the trachea must be selected, and the scalpel may even be directed upwards through the cricoid cartilage. We must trust to a forcible expiration for the expulsion of the foreign substance. (Mr. A. M. M'Whinnie, p. 135.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

APHTHOUS INFLAMMATION AND ULCER OF THE TONGUE.—Aphthous inflammation of the tongue is caused by anything which irritates the alimentary canal. Chlorate of potash taken internally, and used as a lotion to the part, soon cures the case. There is a form of simple ulcer of the tongue, neither syphilitic nor epithelial in its character, yet simulating, and frequently subjected to the same treatment as these: if thus treated it gets worse. If the patient be kept on simple diet, and chlorate of potash used freely, it is soon well. (Mr. Thos. Bryant, p. 81.)

CANCER OF THE TONGUE.—*Division of the Gustatory Nerve.*—Amongst the many sources of the peculiar painfulness of cancer of the tongue, irritation of the fifth nerve may be assigned as occasioning the pain of so much of the tumour as lies in front of the fauces, the tenderness of the ulcer, the pain in the region of the parotid and side of the head, and the excessive secretion of saliva. Division of the gustatory branch of the fifth nerve gives immediate relief to all these symptoms. The best plan of operating is, to cut through all the soft structures on the inside of the ramus of the jaw, by an incision commencing immediately behind the last molar tooth, and extending three-

quarters of an inch in a direction towards the angle of the jaw. The only structures which can be divided by such an incision are, the mucous membrane and a part of the mylo-hyoid muscle, with the gustatory nerve descending forward between them, about half an inch from the tooth, and nearly at a right angle with the incision. It is advisable to use a curved knife, and to cut quite to the bone, otherwise the nerve may escape division. This operation was first proposed by Mr. Hilton some years ago, but has never since then been performed. (Mr. C. H. Moore, p. 158.)

Removal of the entire Tongue by Operation.—This is most safely done by means of the ecraseur. First, cut a central opening between the lower jaw and the os hyoides, as deeply as can be done without fear of hemorrhage, and also with the object of having as little structure to crush through as possible. Through this opening pass a curved needle, sufficiently large to allow the chain of the ecraseur which is attached to it, to follow easily. The needle must be carried from the central line, obliquely towards the left side of the base of the tongue into the mouth, and out of the mouth, at a corresponding point on the right side, through the same external aperture by which it had entered. The chain must then be gradually tightened until the mass is gradually crushed through. A cord of twisted wire, or a piece of strong whipcord may be substituted for the chain. (Mr. T. Nunnely, p. 155.)

CLEFT PALATE.—The most favourable cases for operation are those in which the cleft is complete or nearly so, for when it is only partial, after the separation of the soft tissues from the bone, the flaps do not fall inwards, owing to want of parallelism between the sides. The use of lateral incisions interferes with the nutrition of the flaps, but sometimes they are absolutely necessary. As for the importance of detaching the periosteum with the flap so insisted on by Prof. Langenbeck, it is impossible to detach the mucous membrane without the periosteum. (Mr. G. Pollock, p. 269.)

The principal reason of failure in the treatment of bad cases of cleft palate is too much having been attempted at one time. Unless the opening in the hard palate is very small indeed, its closure must be the result of more than one operation. The periosteum must be separated along with the flaps made, for the purpose of strengthening them, and during their formation the knife must be kept well down upon the bone for this purpose, and a blunt instrument answers the purpose better than a sharp one. The steps of the operation necessary are these:—1. Pare the edges of the opening with a small scalpel. 2. Make an incision on each side, down to the bone, and parallel with the fissure in the palate. 3. Separate these flaps from the bone along with the periosteum. 4. Close the opening by sutures very neatly and carefully applied. (Mr. A. G. Field, p. 158.)

CONSTIPATION.—*Podophyllin.*—This is a greenish coloured amorphous resinoid powder, obtained by evaporation from an alcoholic solution of the root of the May apple (*Podophyllum peltatum*). Podophyllin is perhaps more free from the objections attached to aperients than any other remedy of this class. It operates slowly, easily, and after the manner of nature. In America it is used in ten-grain doses as a cathartic of similar character to the resin of jalap; but for use as a natural laxative, the maximum dose is one grain. It is better in most cases to begin with half-grain doses, made into a pill with extract of taraxacum, which must be taken during breakfast, that it may operate next morning after that meal. (p. 276.)

DIARRHŒA.—*Sulphuric Acid.*—Sulphuric acid is a much more rapidly efficient agent in the cure of diarrhœa than chalk, opium, copper, lead, pernitrate of iron, and others, but it is much less effective alone than in combination. When the stools are scanty and frequent, the administration of the acid is preceded by a dose of castor oil. When they are abundant and watery the acid may be begun at once:—℞. Acid. sulph. aromat. ℥xx.; tinct. camph. comp., ʒj.; ætheris chlorici, ℥x.; spirit. menthæ pip. ʒ ss.; syrupi rhœados ʒj.; decoct. hæmatox. ad ℥j. To be given every four, five, or six hours, according to the urgency of the diarrhœa. The object of the aromatics is the correction of the fæcal fermentation which is one of the chief conditions in the production of diarrhœa. (Dr. A. Clark, p. 86.)

Diarrhœa from Ulceration of Bowel in Phthisis.—The following is a "very capital mixture":—Extract of hæmatoxylon, ipecacuanha wine, and tincture of opium, made up with the ordinary chalk mixture. A mixture of nitric acid and ipecacuanha wine is also of great use. (Dr. Wilks, p. 244.)

Diphtheria and Cynanche.—*Guaiacum.*—Guaiacum is a specific in common sore throat; no matter what the stage of the complaint its action is always equally satisfactory. Guaiacum, along with chlorate of potash, is the remedy *par excellence* for diphtheria. For the attending debility our sheet-anchors are ammonia and bark. My prescription for all cases of diphtheria is the following: ℞. Pottassæ chloratis ʒiv.; tincture cinchonæ comp. ʒss.; tinct. guaiaci. comp. ʒss ad ʒvj.; mellis q. s.; aquæ ad ʒviij. M. Of this mixture from one tea to two tablespoonfuls may be given, according to age; but the interval between each dose must be regulated according to the severity of the case. In some cases of diphtheria a strong solution of nitrate of silver may be applied topically, to hasten the removal of the exudation, and to induce a more healthy action over the denuded surface. (Dr. J. W. Walker, p. 85.)

Hernia.—*Radical Cure of.*—The following plan of operating has been very successful, and is the same as Wood's, but slightly modified. The patient lying on his back, and his bowels having been that day opened, chloroform is administered if wished for. An incision, one inch and a half below the spine of the pubes, is then made for about three-quarters of an inch, the *integument* of the scrotum only being cut through; the lips of the incision are now successively seized hold of, and by a blunt-pointed pair of scissors a dissection is made beneath the integument, for a space of two inches around the external opening of the inguinal canal. The conjoined tendon is now to be felt for by the finger of the left hand, which pushes before it some fascia. The needle recommended by Mr. Wood is then carried on the finger to the external abdominal ring, through which it is to be passed for a distance of about half an inch, or it is thrust through all the soft parts above. When the point has emerged through the integument, the needle is threaded by a silver wire, and then withdrawn with the wire. The first and most difficult portion of the passing the wire is then accomplished. The finger now seeks for the external pillar of the ring, and on it the needle, bearing a loop of the wire already introduced (*i. e.*, the wire simply bent on itself), is carried about half an inch within the canal, and all the soft parts above it are transfixed. The extremity of the needle appears on the surface of the abdomen, bearing a loop of wire, and it easily glides down the *free* end of the wire when the loop is held. And again, upon the finger having found the internal pillar, the needle bearing the wire is carried and made to pierce the tissues above, and a little within the canal. The needle having been disengaged from the wire is lastly withdrawn. The free extremities of the wire are then passed through an aperture in an oval piece of thickish glass (three inches long by two broad), and the loop through another aperture a quarter of an inch distant, and having been pulled together as close as can be, they are twisted over each other, which securely fastens them. The testicles having been supported by some tow, and the parts washed, all is done. The wires must be unfastened on the eighth day and removed entirely on the next. (Mr. R. Davies, p. 146.)

Strangulated Hernia.—*Chloroform.*—Under the influence of chloroform, if the taxis is to succeed, the slightest manipulative efforts will suffice: by its use at first the danger of long-continued and repeated efforts at reduction is annulled, and the symptoms are not masked as when opium is used. Out of one hundred cases in which all other means had been employed and entirely failed, twenty were reduced under the influence of chloroform. Hence we may fairly conclude that we should primarily employ the means which have been proved to be so successful, and not waste time by the use of other remedies, when we possess one so incomparably their superior. (Mr. T. Bryant, p. 139.)

All the conclusions deducible from the statistics of operations point one way, and that is against the practice which demands more to be done in an operation than is absolutely essential. In strangulated hernia the mortality

is nearly three times as great when the sac is opened than when it is left intact. Practically a knowledge of the contents of the sac is of little use. Chloroform must always be first administered, and the patient be brought completely under its influence. In strangulated femoral hernia, unlike inguinal, but few cases are reducible by the taxis, and the use of chloroform cannot be so highly recommended. In "old" cases of femoral hernia so few cases are successfully treated by the taxis, and so many more get better under operation, that it becomes a question whether it would not be better to put aside the taxis altogether in these cases. If the opening of the sac is unavoidable, it is better only to open the neck of the sac just sufficiently far to allow of the division of the stricture, but not the exposure of the hernial contents. (Mr. T. Bryant, p. 142.)

Medical Treatment.—During the latter period of my practice I have followed the following plan of treatment in cases of strangulated hernia, and with one exception, with uniform success, (and in this the treatment was not properly carried out). Send the patient six pills, consisting of ten grains of calomel and three of opium, and direct that he shall take two immediately, and one every half-hour subsequently; and also that he shall drink a cup of strong coffee without milk or sugar occasionally. The hernia generally speedily becomes soft and reducible. (Mr. W. Blower, p. 150.)

UMBILICAL HERNIA.—*New Operation.*—Cut down upon and slit up the sac so as to expose the crevice-like opening into the abdomen. Clear away a good deal of the fat and cellular tissue so abundant outside the sac, otherwise it insinuates itself between the edges of the ring and prevents closure of the opening. The sac may be nearly all cut away likewise. Now, by means of a Price's needle introduce two, three, or even four sutures and draw the edges of the opening together. Sufficient distance is to be left between the edge of the opening and the entrance of the needle, and a considerable thickness of tissue must be included in the sutures. It is unnecessary to pare the edges of the opening. (Mr. R. Barwell, p. 149.)

TAPE-WORM.—*How to administer the Oil of Male-Fern.*—Always give the draught mixed up with an ounce or two of milk; it is thus more effectual, milk being the favourite food of the parasite. The draught is made by mixing a drachm of the oil with half an ounce of mucilage. It must be taken fasting. The mixture is carried speedily into the intestines, to feed, and at the same time to poison, the hungry parasite which nestles there. (Dr. Fleming, p. 88.)

AFFECTIONS OF THE URINARY ORGANS.

ALBUMINURIA.—At one time it was thought that death invariably resulted in all cases of albuminuria. This is now found not to be the case. In every instance in which recovery has taken place the urine has been bloody, and its specific gravity high, the system œdematous, and the general power good. In fact, in acute cases where congestion or subacute inflammation alone exists, we have much reason to hope for recovery. When the patient is strong venesection hastens recovery. The best treatment is a persevering use of the pulv. jalapæ comp., and warm baths taken about once a day to encourage free action of the skin. In no single instance in which the urine has slowly and insidiously become albuminous, in which the system seldom becomes œdematous, in which the urine never presents a bloody tinge, have I known recovery to occur. In such cases the kidneys are small, contracted, and granular. In the former class of cases the sp. gr. of the urine seldom descends below 1012, in the latter it is generally below it. (Dr. T. Williams, p. 92.)

In a case of albuminuria complicated with squamous skin disease, it was found that the arsenic given for the cure of the latter exercised a very beneficial effect upon the quantity of albumen in the urine. The urine is described as having a smoky appearance, and the renal anasarca was acute in character. The albumen ultimately almost disappeared, and the patient left the

hospital "comparatively well." The arsenic must have acted as a powerful tonic. (Dr. Farre, p. 95.)

DIABETES.—Patients sometimes, though rarely, get rid of their complaint, and if there is anything that can favour the production of such an issue, it is the keeping down their sugar to the utmost extent possible. By restoring the blood as near as possible to its natural constitution, we conduce to the healthy performance of the nutritive and other operations of life. (Dr. F. W. Pavy, p. 98.)

Diabetes is essentially a defective assimilation of the starchy and saccharine matters of food; the sugar filters through the system in obedience to the simple laws of physics. But unfortunately, the presence of sugar in such quantities alters the natural qualities of the blood, so as to unfit it for performing the healthy operations of life. By a *purely animal diet* the amount of sugar can be kept down to a thousand grains a day; when this is attained, the patient does not in any way know of the existence of his complaint, otherwise than by the necessity of restricting himself to the regimen essential to attain this object. With such an amount only of sugar escaping, the blood remains adapted for the healthy performance of all the functional operations of life. (Dr. F. W. Pavy, p. 97.)

A *gluten powder* is prepared by Mr. Blatchley, of London, which can be made into biscuits of an agreeable flavour, and being as free as possible from starch, is peculiarly suited to diabetic patients. These biscuits cannot be furnished so cheaply as the bran biscuits, and are therefore less fitted for hospital use. The biscuits can be bought ready made, or they may be prepared at home from the gluten powder. (Dr. J. Camplin, p. 98.)

HÆMATURIA.—By a look with a microscope, and one or two plain questions, you may determine the seat of any hemorrhage in the urinary tract. If we find blood-casts of the uriniferous tubes, we know that the hemorrhage is renal. If no casts are found, yet the hemorrhage is abundant, it is probably not from the ultimate structure of the kidneys. If the blood is passed by itself, it is urethral, or if it is confined to the first jet of the stream. If, again, it is confined to the last drop or two, it is probably urethral, and situated in the bulb. If the blood is never passed except by an act of micturition, and intermixed with urine, and there is a clear absence of casts, then it comes from above the urethra and below the ultimate kidney structure. (Dr. H. Salter, p. 89.)

HYDROCELE.—*Wire Setons.*—In the majority of cases iron wires passed through a hydrocele will cause merely adhesive inflammation, and thus effect a speedy and simple cure. But the result of extended investigations shows that, in a by no means small per centage of cases, notwithstanding every precaution, suppuration will take place. (Dr. J. D. Gillespie, p. 174.)

LITHOTOMY.—The principle is undoubtedly sound to keep the incision within the prostate in adults, but there is far more danger in attempting to drag a calculus through an opening of insufficient size. In fact, insufficient internal incisions are equally dangerous with those which are too free. Destructive inflammation of the delicate structures about the neck of the bladder is soon produced by the forcible dilatation, and the dragging downwards of the neck of the bladder, which insufficient incisions render necessary. The true cause of death in the majority of cases, is violence in opening up the internal part of the wound, and laceration of the tissues there, and not the passage of urine into the intercellular connexions about the neck of the bladder. The first is happily in the operator's power to avoid, the second would be often unavoidable with a stone much above the average size. (Mr. H. Thompson, p. 164.)

It is the best plan to use a staff of moderate curve. The curve should not exceed two and a half inches, the rest of the staff being straight. The urethra is not distorted by this instrument, as it must be by a staff of large and long curve, or with a straight staff. (Mr. Le Gros Clark, p. 168.)

OXALATE OF LIME IN THE URINE.—Oxalate of lime occurs in two forms in the urine; 1st, in octohedra; 2nd, shape of dumb-bells. The first is of little importance, and found in the most opposite conditions of the system.

It probably indicates that the process of oxidation is not fully performed. Dumb-bell crystals are, however, of considerable importance; unlike octohedra they are never formed in the urine after it has been voided. These crystals are formed, probably, in the secreting tubes of the kidney. These are liable to cause hæmaturia, and to form the nuclei of larger concretions. To diminish the tendency to the formation of these crystals, attention must be paid to the general health, and especially to the state of the digestive organs. Iron, quinine, and other tonics, may be given according to the general state of the patient; but air, exercise, short of fatigue, and a diet regulated as to quantity according to the proportion of nutrient material required by the system, are of the first importance. To get rid of the crystals already formed, let the patient drink large quantities of alkaline fluids, as Vichy water. (Dr. L. S. Beale, p. 90.)

STRICTURE OF THE URETHRA.—A case well illustrating the efficacy of complete division upon a grooved director of all the thickened constricting parts is related by Mr. Syme. He shows that the blade of the knife must be introduced beyond the diseased part, and the whole divided completely. The knife, if simply pushed along the groove, often fails to divide the thickened part. (Prof. Syme, p. 160.)

VARICOCELE.—The treatment of varicocele is now very simple and effectual. Introduce a needle beneath the vein or veins to be obliterated, and pass a figure of 8 ligature over its extremities, so as to include a portion of skin and the dilated veins. The circulation through the veins is, of course, arrested by the pressure. In a precisely similar way, apply another needle at about an inch distance. Now divide the veins subcutaneously. In a few days the veins will be effectually obliterated and the patient may resume his ordinary employments. If there is a pendulous condition of the testicle, all the skin intermediate between the two needles may be removed, and the subjacent enlarged veins also. The gap left is readily closed by bringing the two edges in contact by sutures, and no hemorrhage will ensue, in consequence of the vessels being closed by acupressure. Care must be taken during the operation to compress the same veins above and below the incision, and not to divide any that are not thus compressed. Great care must be taken to avoid the vas deferens. This may always be recognized by its peculiar hardness and irregular surface, and can be easily separated from the veins, before either the needle or the knife is passed beneath them. (Mr. H. Lee, p. 171.)

For the manner of ligaturing the veins subcutaneously pursued by Ricord, as modified by Mr. J. Tufnell, see woodcut and description of operation at p. 173.

AFFECTIONS OF THE BONES, JOINTS, ETC.

AMPUTATION OF THE FOREARM.—A plan followed with success is to make a circular incision through the skin, draw it well back, then make flaps of the muscles, and finally saw through the bones. (Mr. P. Hewett, p. 102.)

CONTRACTED JOINTS.—*Forcible Extension.*—The cases in which forcible extension must not be tried, are: 1. Those in which the inflammation, if it has existed, is apt to recur. 2. When the change has been of very long standing. 3. When there is a strongly-marked diathesis (strumous, syphilitic, &c.) 4. When the skin is involved and adherent over the joint, soundness of skin being an essential condition to success. In some cases in which the contraction is owing to rigidity of muscles, the contraction is reducible under chloroform with the use of very little force. (Mr. J. Paget, p. 123.)

DEFORMITIES OF THE FEET.—*Mechanical Treatment.*—Previous to 1832 most cases of deformity of the foot were treated by mechanical means only; since that period tenotomy has been employed in most cases, certainly with the effect of more rapidly curing the deformity, but as certainly with the effect of leaving a foot so weakened by the treatment as to be incapable, in

many cases, of performing its proper part in the motions of the body. Out of seven cases in which other tendons than the tendo-achillis were divided, and opportunity offered to examine after death, it was found that in every one of them the severed tendons were not united at all, or had become attached to the bone or surrounding parts so as to be of no possible use. To divide the peronei, the tibialis posticus, and flexor longus digitorum, is almost equivalent to producing complete paralysis of these muscles. Section of the tendo-achillis is sometimes, but not, as at present, indiscriminately required; and *division of the other tendons is hardly ever necessary.* We must in fact return to and rely solely upon mechanical treatment. In the mechanical treatment we must recollect that the deformity may result from excessive contraction of the prevalent muscles, or deficient action of their antagonists: and that our efforts in either case must be directed to applying a power in the direction of and as much like the deficient muscle or muscles as possible, and at the same time the *foot must be allowed its use.* These indications are thus carried out:—A broad and long piece of strapping-plaister, spread on a thick material, is made to adhere over the origin of the muscle and along its course. Upon this is laid a piece of sheet-tin, long enough to reach from the upper part of the leg to just above the ankle, and carrying at its upper part a wire loop; the piece of strapping is turned back over it, and lies with its adhesive side outwards. Another piece of strapping is placed on the foot upon the insertion and along the course of the tendon; the end of this strapping reaches to the bend of the ankle, and is folded over (the two sticky sides opposed), and an eyelet let into it. An assistant is now to hold the limb as near as possible in the right position, while it is being smoothly strapped from the toes to the knee, leaving out the end (with its eyelet) of the piece on the foot and the wire loop at the upper part of the leg. Between these two—the one representing the origin, the other the insertion, of the muscle—there is to be stretched an india-rubber spring, at a proper degree of tension. Thus, while there can be considerable force used in the direction of any muscles in the leg, the *point d'appui* is so supported by a loop of strapping, which takes its bearings from both the skin of the leg and the surrounding plaister, that no injurious pressure occurs anywhere. Some little contrivances are necessary also for changing the direction of the force. (Mr. R. Barwell, p. 120.)

Diseases of Joints.—The opinion is generally held that scrofula is almost universally the cause of disease of the joints. This is very far from being the case. Another opinion that long and perfect rest to joints induces disease leading to anchylosis is also incorrect. Again, when in disease of a joint, the articular ends of the bones can be felt grating upon each other, the surgeon is apt to consider this symptom as fatal to any probability of the repair in the joint. This also is a mistake. This grating occurs when, the cartilage being gone, the hard lamella of bone beneath it remains worm-eaten it is true, and cribriform in appearance, yet, if accurate apposition of the bones be effected and complete rest of the parts be maintained, quite capable of repair; and such bones do indeed, by anchylosis, become quite firmly united, no operation whatever being required. It is, however, a question if the removal, by absorption, of this cribriform hard layer of bones must not precede anchylosis. Diseases of joints in children follow a peculiar course. In adults the individual structures of a joint may be diseased separately; in children, this isolation of disease is rarely seen, and there is a tendency to diffuse disease contemporaneously in all the articular structures. The same tendency to diffuseness exists in all inflammations in children; this is especially seen in the chest, and the head. (Mr. J. Hilton, p. 102.)

Dislocation of the Shoulder-Joint.—The object of counter-extension is to prevent the yielding of the scapula to the tractive force employed to reduce the dislocation. Nothing does this so effectually as the fixing of the opposite scapula by counter-extension at the wrist. By simple traction at the two wrists, most dislocations may be reduced. The patient must be placed in a chair, and the traction must be steady and horizontal. In case the object is not effected by these means, the operator may place his foot on

the chair, and his knee in the axilla, and thus raise the head of the bone, using the arm as a lever. (Prof. N. R. Smith, p. 114.)

HIP-JOINT DISEASE IN CHILDREN.—When the early symptoms of disease of the hip-joint are present, as lameness, flexed thigh, slight local heat—do not hesitate, but tell the parents plainly that for the cure of the case it is necessary that the child should lie down for two or three months, with a splint on the limb to keep the joint quiet. In hip-joint disease, the only remedy required, and indeed the only remedy sufficient, is perfect rest, secured by the recumbent posture in bed. The best splint is made by adapting to the limb and hip a large piece of wet leather previously cut of the required shape. A bandage is placed evenly and tightly over the leather. When the leather has become dry, it is removed, lined with wash-leather, and an iron foot-piece and proper straps being affixed, it is complete. It must be worn constantly until all symptoms of inflammation have disappeared. (Mr. J. Hilton, p. 108.)

HOUSEMAID'S KNEE.—These cases may be cured in the following manner, without the least pain, confinement, or danger, the patient doing her usual work, except kneeling. Apply a simple plaster and bandage in the manner Scott recommends. A fortnight is sufficient for the cure, and perhaps two applications will be necessary. The following is the plaster: Cut a piece of leather of sufficient size to cover the knee above and below the joint, and to nearly meet behind; spread this with emplastrum plumbi. Cut some white curd soap into thin shavings, and about the same weight o ' emplastrum plumbi; add about a teaspoonful of olive oil. Melt together over a slow fire. Take half an ounce of muriate of ammonia (commonly called sal ammoniac); pound this in an earthenware mortar as fine as you can. Pour the melted soap plaster into the middle of the plaster spread on leather, so that it may cover the upper surface of the knee; let it be nearly an inch thick. Now, while it is liquid and hot, mix into it the powdered muriate of ammonia, and apply it as warm as the patient can bear. Apply a roller *secundem artem*. (Mr. R. W. Martyn, p. 113.)

INJURIES OF SKULL.—Never operate by trephining for any injury of the skull simply because the skull is injured, and you have reason to suspect depression of bone; wait until symptoms of compression actually occur. In many bad cases, by waiting patiently, by keeping the patients at perfect rest, by subduing inflammatory action, by assisting in the removal of fragments of bone when they become loose, we may procure recovery without any operation at all. (Mr. J. F. West, p. 124.)

LATERAL CURVATURE OF THE SPINE.—At the commencement of cases of lateral curvature, the best treatment is a combination of lying down, to the extent of four or six hours a day, together with light gymnastic exercises. This when curvature is threatened rather than positively formed, but if curvature has actually taken place, then mechanical support must be combined with the partial recumbency and light gymnastic exercises. The best medicinal treatment consists in the administration of hypophosphite of lime and preparations of iron. Five grains of the lime may be dissolved in a wineglassful of water, to which tinct. ferri sesquichl. may be added, and taken twice a day, at, or directly after meal times. Codliver oil may also be given. The best localities as a residence for such patients are high and dry places on a chalk soil. The best couch in such cases is one made by Ward, of Leicester Square. The inclination can be varied at any angle, from that of an upright chair to a horizontal couch. The muscular debility theory of curvature is very logical and attractive, but it can only apply to a very limited number of cases. No confidence can be placed in the curative effects of any system of special muscular exercises, whereby the muscles on one side of the spine are sought to be particularly developed. Such a result is practically impossible. Used judiciously, however, in conjunction with other means, muscular exercise is of great value. In delicate girls with slight curvature, the lightest form of instrument, such as that known as the England belt, is quite sufficient. The instrument may be made more powerful and a second plate may be added. (Mr. W. Adams, p. 115.)

NEW FRACTURE APPARATUS.—There is no form of injury to either the

shoulder or arm, including fracture of the clavicle, to which the apparatus to be described is not better suited than any that has been hitherto employed. The apparatus consists of a bar of iron one inch wide and an eighth of an inch thick, bent at the shoulder and elbow joints, and armed with five carefully covered pads, two of these being fixed, one at each extremity of the bar, and three of them capable of sliding into various positions. The upper or shoulder pad, with a basis of flat metal, is so bent as to fit accurately on the shoulder, extending over the scapula and clavicle, and by the assistance of a strap passing through the opposite axilla and buckled by its ends to the shoulder pad, arresting their movements. The other fixed pad is intended, by being secured round the wrist, to prevent all shifting of the instrument or motion in the injured limb. The other pads are made movable, first to facilitate the dressing of the wounds where these exist, and secondly to assist the close apposition of the limb to the splint. (Mr. C. Evans, p. 252.)

RECTANGULAR FLAP AMPUTATION.—Whenever amputation is performed by the rectangular flap operation of Mr. Teale, (see Retrospect, vol. xxxviii), a durable cushiony stump is obtained, capable of bearing a considerable amount of direct pressure on its face, and containing neither large nerves nor bloodvessels. This operation is especially useful in amputations at the lower third of the thigh and leg; it has only one drawback, that arising from the length of the long flap, necessitating that the bone must be sawn through higher up than in either of the ordinary modes (the circular and the transfixion methods). In other parts of the thigh and leg the short anterior and long posterior flaps obtained by the ordinary transfixion method answer, when properly shaped, everything which can be desired. The operation is done by clean sweeps of the knife in contrast to a somewhat tedious dissection, and the soft parts from behind make a better cushion than those from the front of the limb. (Mr. O. Pemberton, p. 100.)

SPREADING GANGRENE.—*Amputation.*—There can be no doubt that the practice of operating at an early period in spreading gangrene is not a safe, and often indeed, a disastrous measure. Nothing is gained, and a great deal is at stake by the surgeon's early interference in such cases. (Dr. L. Adams, p. 125.)

AFFECTIONS OF THE SKIN, ETC.

BURNS AND SCALDS.—*Sulphur Ointment.*—Sulphur seems to have almost a specific virtue in cooling and soothing the irritation of the part burnt. If nothing has been done previously, cover over all the injured surface with a pretty stiff ointment, composed of simple hog's lard and flowers of sulphur, spread in the form of a good thick layer on linen cloth. Under this simple treatment, burns and scalds of a slighter character heal in two or three days. Another good ointment is made as follows:—Place a mixture of common leeks and hog's lard in an earthen vessel, and allow it to simmer by the fire, until the leeks become quite soft and tender; strain through muslin. The ointment is of a lightish green colour, and has a slight garlic odour. (Dr. J. Y. Myrtle, p. 183.)

GOITRE.—*Biniodide of Mercury.*—In England the following plan suffices for the cure of goitre. Having made an ointment of the strength of ℈i. of biniodide of mercury to the ounce of lard, rub a little freely into the part night and morning. After each application the part must be covered with oiled silk. In a few days the full effect is produced, when the mercurial ointment may be laid aside and simple spermaceti ointment and oiled silk substituted. (Mr. H. M. Greenhow, Mr. T. Stainthorpe, pp. 185, 286.)

INDOLENT ULCER ON THE LEG.—First wash the leg well; after so doing, fill up the excavated ulcer with finely-powdered carbonate of iron, afterwards applying a large linen pad, without any moisture coming near; then envelope the whole limb in a starched bandage, allowing it to remain three weeks or so, according to the extent of ulcerated surface. The patient need not be confined to bed, or be enjoined absolute rest; he can even be allowed to walk a

little every day. The limb must be kept perfectly dry. (Mr. J. Mitchell, p. 185.

LUPUS EXEDENS.—The local application of a solution of pernitrate of mercury has affected a cure in some cases of this disease. (Mr. J. Gay, p. 31.)

PARASITIC SKIN DISEASES.—In parasitic diseases of the head in children, I find no application so satisfactory as the compound sulphur ointment of the Hospital for Skin Diseases: ℞. Sulph. sublim., ℥ss.; hydrarg. ammoniochloridi, hydr. sulph. cum sulph., āā ʒss. Leviga simul, dein adde olivæ olei, ʒiv.; adipis recentis, ʒxvi.; creasotonis ♏xx. Misce. If the disease has made some progress, it will not be cured under several months. (Dr. T. Hillier, p. 180.

VARICOSE ULCER.—Apply an ointment spread on lint, containing a very large quantity of an alkaline earth, such as chalk. This forms a good protection to the sore, and neutralizes the foul acid secretion which flows from it. Apply over this a "domette" bandage, evenly and neatly, and reaching from the foot to the knee. An ulcer thus treated generally heals with extraordinary rapidity, in spite of the patient walking about as usual. "Domette" is a mixture of flannel and cotton, used chiefly for making shrouds. (Mr. J. K. Spender, p. 184.)

SYPHILITIC AFFECTIONS.

INDURATED CHANCRE.—The local application of a solution of pernitrate of mercury, together with the administration of mercury internally, has been satisfactorily used in this disease. It is not necessary, were it possible, to continue the application until the whole of the induration is gone, but only until the wound shows unmistakable evidence of healing in. (Mr. J. Gay, p. 31.)

SYPHILIS.—*Calomel Vapour Bath.*—It is but seldom that patients can be induced to continue the internal use of mercury sufficiently long to be cured of real syphilis. The unpleasant effects produced pass away on his leaving off the remedy for a time, and, feeling better, he is convinced he has acted properly in discontinuing its use. You may patch up the disease by giving the remedy internally, but it will return over and over again. The plan of introducing the remedy by mercurial inunction is far more successful than any other plan, and the deleterious action of the drug upon internal organs is avoided. The amount absorbed into the blood produces its influence equally throughout the system, and is not conveyed direct to the liver, as when the medicine is administered internally. But mercurial inunction is a dirty remedy, and, requiring considerable labour and perseverance on the part of the patient, it is with difficulty that they can be induced to continue its use for any length of time. Fumigation of the surface of the body, by means of the vapour of calomel, possesses the advantages of inunction, without some of the objections to which it is liable. Calomel is a definite chemical compound, unaltered in composition either by heat or by moisture, and may be relied upon for producing its action as certainly as any medicine administered internally. So much is this the case, that for some years I have altogether discontinued the use of mercury internally in the treatment of syphilis. (Mr. H. Lee, p. 179.)

Conveyance of Syphilis by Vaccination.—There is no doubt but that syphilis is readily conveyed from one child to another by the process of vaccination. To avoid any chance of this: 1. Examine carefully the child from whom the lymph is taken. 2. Try to learn the state of the parents' health. 3. Choose, in obtaining the lymph, such children as have passed the fourth or fifth month, as hereditary syphilis, in general, appears before that age. 4. Do not use the lymph after the eighth day of the existence of the vesicle, as the lymph on the ninth and tenth days becomes dull by mixture with pus, which latter may be of an infectious nature. 5. In taking the lymph with the lancet, avoid hemorrhage, as there is less danger with pure and transparent lymph.

6. Do not vaccinate too many children from the same supply. (Dr. Pacchiotti, p. 176.)

SYPHILITIC TUMOURS AND ULCERS OF THE TONGUE.—Syphilitic tumours are formed in the tongue precisely as in other parts. They are composed of badly organised inflammatory products. Sometimes the tumour thus formed is of a diffused character. After a time ulceration takes place, the result of degeneration of the abnormal tissue. Now, these results are liable to be mistaken for cancer, which runs precisely the same course. The depth of the excavation, the irregularity of the surface, and the induration, raggedness, and eversion of the edges in cancerous disease, are the points upon which to ground the diagnosis. Under the use of iodine of potassium and iron, or bichloride of mercury and tonics, the syphilitic disease is curable; the cancerous is amenable to no treatment except removal. (Mr. T. Bryant, p. 81.)

AFFECTIONS OF THE EYE AND EAR.

ATROPINE PAPER.—Instead of using the ordinary solution of atropine to dilate the pupil, it is a good plan to soak fine green tissue paper in a solution of atropine; dry it, and cut it into small pieces one-fifth of an inch square. Each of these squares should contain as much salt as suffices to dilate a pupil. One may be placed on the sclerotic conjunctiva below the cornea without causing any irritation, and with the effect of dilating the pupil just as well as when the solution is used. (Mr. J. F. Streatfeild, p. 192.)

CATARACT.—The patient being recumbent, desire him to look upwards, the operator at the same time depressing the lower lid. By means of a pair of ordinary artery forceps, the teeth of which have been slightly rounded off, seize a fold of the loose ocular conjunctiva with a portion of the tissue beneath (including if possible a few fibres of the inferior rectus). The forceps must now be held by an assistant depressed on the cheek; it thus serves to depress the lid, and to firmly fix the eye, during the operation. (Mr. J. F. France, p. 190.)

DEEP ULCERS OF THE CORNEA. — *Paracentesis.* — Deep ulcers which threaten to perforate the cornea will often rapidly assume a healthy action after the tension of the cornea has been diminished, by letting off the aqueous humor; and sloughing ulcers will, under the same treatment derive similar benefit. To effect this object, pass a broad needle through the cornea at its lower margin, and then suddenly turn it on its edge, so as to allow the aqueous humour to run off. The needle must be rapidly withdrawn as soon as the iris approaches the cornea. (Mr. G. Lawson, p 191.)

OPHTHALMIA.—*Insufflation of Calomel.*—In the different forms of phlyctenular, or strumous ophthalmia, the insufflation of calomel is often of marked advantage during the presence of the disease, and also in preventing the constant recurrence of the affection. When it causes much irritation the red precipitate ointment must be substituted. (Dr. S. Wells, p. 189.)

PARALYSIS OF THE MUSCLES OF THE EYE.—*Electricity.*—In cases of recent squint depending entirely on peripheral causes, the use of electricity is sometimes productive of most beneficial effects. The continuous current has a deleterious effect on the retina. The extra current of an induction machine must be used; either a magneto-electric or volta-electric apparatus are suitable. (Dr. S. Wells, p. 187.)

MIDWIFERY, AND THE DISEASES OF WOMEN AND CHILDREN.

ANÆMIA.—We constantly notice in cases of anæmia in females that, though pallor is marked, evidencing the diminution of red corpuscles, yet there is no diminution in the bulk of the body. This plumpness, however is frequently false, and is owing to slight infiltration of the tissues with serum. In anæmia

there is much more deficiency of the nitrogenized constituents of the body than of the fatty or hydro-carbonaceous, and this is owing to the greater facility of absorption of fatty matters from the intestinal tract. In the treatment of anæmia our aim must be to introduce into the blood (a different thing from getting swallowed) the largest possible amount of—1st, nitrogenous food; 2nd, iron; 3rd, chlorine. To most patients the very sight of food is an abomination. Begin then, not with a regular diet table, but with a cup of milk with some lime-water in it every two hours, and a small quantity of beef-tea in divided doses, and pass very gradually to better articles of diet as the appetite bears or requires them. No form of iron agrees so well as the compound iron mixture of the London Pharmacopœia; it seems as if the carbonate and finely divided oxides were peculiarly easy of solution in the water saturated with salts and carbonic acid, which (and not pure water) we must remember is the solvent to be considered. In cases which improve very slowly, warm hydrochloric acid baths are of great service; the presence of more acid in the system perhaps favours the absorption of more iron. The pil. aloes c myrrha given every night, not as a purgative, but as a clearer of the intestinal, especially of the colonic membrane, is frequently found to be very useful. We must remember that in a state of debility the digestive mucous membranes are apt to be covered by a glairy mucus, which impedes absorption of nutritive material. It is, indeed, a universal rule that the greater the diminution of life the more secretion from mucous membranes, and the higher the vitality the less secretion is present. (Dr. T. K. Chambers, p. 38.)

Anæmia and Leucocythæmia.—One of the most excellent tonics is a combination of hypophosphite of potash with a little sugar or syrup, and a little tincture of gentian. The hypophosphites are almost as useful in anæmia as in phthisis. (Mr. J. Taylor, p. 258.)

Cauliflower Excresence of the Os Uteri.—Excision of the cervix uteri in cauliflower excresence of the os uteri is a comparatively safe and harmless operation, when performed by the ecraseur, modified to the circumstances of the operation. A rope made of annealed steel wire must be made of four or five smaller ropes, each composed of four wires of the size used for sutures. This rope must be made to encircle the cervix, by the aid of two tubes like those of Gooch's double canula. Both ends of the rope are then passed through the eye of the ecraseur, which is pushed up as far as the level of the encircling rope; when this is accomplished, the latter is properly fastened, and the tubes withdrawn. The screw must now be carefully worked. Intra-uterine polypi may be easily removed in the same way. (Dr. J. B. Hicks, p. 218.)

Child-bed Fever.—*Morphia.*—The following method has been pursued with very beneficial results: Give one-eighth of a grain of morphia two, three, or four times a day, according to the violence of the abdominal pain, and an hour after each dose a mixture of camphor (℞. Camphor. ʒss; gummi mimos ʒj; aq. chamomill. ℥iij.; liq. ammon. acet., sacch. albi, āā ℥j.) This treatment must be continued until the fever and pain are relieved, which usually occurs very speedily. (Professor Ritgen, p. 213.)

Craniotomy.—*New Instrument.*—A new instrument for perforating the skull in cases of craniotomy is figured at p. 207. The peculiarity consists in the large cone-shaped screw. This may be of use when the cranium is unusually ossified. (Dr. L. D. Harlow, p. 207.)

Dysmenorrhœa.—*Mechanical.*—In cases where the os is hard and densely fibrous, and simple dilatation quite insufficient, the os must be incised all round, and then a small leaden cylindrical plug introduced; this, by means of a dilator shaped like a pair of shoemaker's pliers, may be dilated so as to completely prevent reunion. (Dr. J. Coghlan, p. 195.)

Encysted Tumour of the Labium.—The methods of treatment in ordinary use consist of simple puncture, the seton, injections of iodine, and dissecting out the cyst. A fistulous opening generally follows the use of a seton, and simple puncture fails in producing a permanent cure. The best treatment is to puncture the cyst for the purpose of evacuating its contents then to slit

it up freely, dissecting away as much of the cyst as can be got hold of, and filling the wound with lint. The cavities heal up by suppurative action. (Mr. Tatum, p. 231.)

I usually dissect out the entire cyst—an operation of some delicacy, as its walls are very thin. (Mr. I. B. Brown, p. 232.)

FEVER FROM TEETHING.—*Hypophosphites.*—The demand for tribasic phosphate of lime in the construction of the teeth contributes to the disturbing influence called the fever of dentition; and this disturbance is often found to be most pyrexial in those children who have been ill-fed, or that have been too long suckled, both instances showing a want in the blood of a due proportion of phosphates. In these cases hypophosphite of potash may be given with marked success: in cases marked by sthenic symptoms combined with alkalies, and in cases marked by asthenic symptoms with tonics. It is delightful to witness the effect upon a fractious pseudo-inflammatory child. About eight grains of the pure alkaline hypophosphite must be put in a two-ounce mixture, with a little simple syrup. (Mr. J. Taylor, p. 258.)

HYDATIDS IN THE UTERUS.—You need not fear introducing the hand into the uterus, breaking down the mass of hydatids, and withdrawing the contents. If this is not done hemorrhage may continue to an alarming extent. The uterus will contract and the hemorrhage cease on the withdrawal of the foreign growth. (Mr. T. Tweedale, p. 237.)

HYPERLACTATION.—*Hypophosphites.*—"There is something remarkable in the permanence of the effect produced by the hypophosphites of potash and soda in prolonged lactation, beyond that experienced in phthisis." It is better to continue the hypophosphite with a little free alkali. (Mr. J. Taylor, p. 258.)

MILK.—*To Arrest Secretion of.*—Apply over the breast by means of a piece of flannel a saturated solution of camphor in glycerine. (Dr. Harris, p. 209.)

NÆVUS.—When the nævus is cutaneous the application of a little strong nitric acid or caustic potash generally suffices for the cure. When a deeper eschar is required the potash should be used. The nitric acid may be applied with a small glass rod, and any superfluous liquid absorbed by a piece of blotting paper. (Mr. Le Gros Clark, p. 239.)

Excision.—First pass two or more small hare-lip pins beneath the base of the nævus, letting the ends emerge about one-eighth of an inch on each side, beyond the extreme edge of the growth. Now remove the whole growth with the knife, and bring the cut edges together with figure of 8 ligatures passed over the needles. Excision of these growths is far preferable to the ligature, but for the hemorrhage: by this plan all danger of hemorrhage is avoided. (Mr. J. Gay, p. 242.)

OVARIOTOMY.—In performing ovariotomy, the following points amongst many others are important:—As soon as the tumour is freed from parietal adhesions it should be tapped by a large syphon trochar. As the fluid escapes and the cyst becomes flaccid, it is either to be fixed by a hook and withdrawn, or drawn over the canula and tied over it, to prevent any ovarian fluid from entering the peritoneal cavity. As the cyst escapes, flannels wrung out of water at 90° should be carefully wrapped round it to protect the peritoneal cavity. When a portion of intestine is found to be so firmly adherent that it cannot be separated, the adhering portion of cyst should be cut away, and left attached to the intestine, (the inner secreting membrane being, however, removed.) The pedicle must be compressed by the clamp close to its junction with the cyst before the cyst is cut away. The wound must be closed by hare-lip pins inserted at intervals of an inch, and they must be so managed that, when the opposed surfaces are pressed together, two layers of peritoneum may be in contact with each other on the inner surface of the wound. These two layers adhere together very rapidly, and so prevent morbid secretions from entering the peritoneal cavity. To secure the pedunclo permanently, it is often better to transfix it by a ligature, and tying each half separately, to fix the whole to the wound. The clamp may then be removed and the superfluous portions of cyst removed. A hare-lip may be used to fix it in position. It is important that the strangulating

ligatures be on a level with the skin, and surrounded by a dressing which separates the sloughing tissues from the raw surface. (Mr. T. S. Wells, p 222.)

OVARIAN CYSTS—*Trochar Syringe for the Injection of.*—A trochar may be used by which the surgeon may empty large ovarian cysts, inject solution of iodine, and remove it again if necessary, without any risk of admission of air. It is made by Weiss. See woodcut and explanation. (Mr. T. S. Wells, p. 228.)

PAINFUL MENSTRUATION.—*Veratrine.*—In the mechanical form of dysmenorrhœa an ointment, containing one-hundredth part of its weight of veratrine rubbed into the hypogastric region in quantities of half a drachm twice a day, will greatly relieve the pain. (M. Vannaire, p. 197.)

PLACENTA.—When the head and shoulders of the child have been expelled, let the left hand take charge of the child, and the right hand grasp the fundus with a firm steady downward moderate pressure. Whilst separating the child let the hand of an assistant be placed in a similar grasping manner. Again place the hand on the uterus and compress alternately the fundus and body of the uterus, and draw slightly on the cord until the placenta is either expelled or comes into the vagina. If relaxation of the uterus occurs, smart friction, "pinching and kneading" of the uterus are required. "I care not to wait for pain, I create it." (Dr. Mc Losky, p. 205.)

POLYPUS OF THE UTERUS.—It is the best plan to remove polypi from the uterine cavity, and not to wait until they have extended into the vagina. Many deaths arise from delay in their removal. An instrument may be advantageously used consisting of a rod and winch with double canula, made sufficiently strong to carry wire, and bear tension enough to cut through the neck of a polypus at once. It is possible to remove a polypus in a few minutes, whenever the os is sufficiently open to admit the canula. (Dr. W. T. Smith, p. 237.)

RETAINED MENSES FROM MALFORMATION.—The case here related shows that, by means of a curved trochar, the uterus may be punctured through the rectum, with relief to the sufferings produced by retention of the fluid. The patient will continue to menstruate per rectum. (Mr. I. B. Brown, p. 197.)

RETROVERSION OF THE GRAVID UTERUS.—*Position during Reduction.*—The best position is upon the left side, the pelvis well raised, the shoulders low, and the thighs as much flexed upon the abdomen as possible. If the misplaced organ cannot be restored to its proper position, as a last resource (puncture of the uterus being excepted), the hand may be introduced into the rectum; but in ordinary cases it is sufficient to pass the fore and middle fingers of the right hand into the rectum. Direct the fingers towards the left side of the displaced uterus and of the pelvic cavity; exert a well-sustained upward pressure with them, in the direction of the space between the right sacro-iliac synchondrosis and the pectineal eminence, and the tumour will most likely pass slowly from the true pelvis into the abdomen. (Dr. T. Skinner, p. 199.)

TUBERCULOSIS FROM UTERINE DISEASE.—An instructive case is recorded which occurred in a French Hospital. The collum uteri was swollen, and the os ulcerated and suppurating freely, while mucus flowed from the cavities of the organ. At last symptoms of phthisis ensued, viz., a distressing cough, hemoptysis, night sweats, and diarrhœa; and there was also a dull percussion sound below the right clavicle. The cervix uteri was cauterized twice a week, and the abnormal secretion and ulceration were cured by this means. The digestive powers immediately began to improve, the patient gained flesh and strength, the fever disappeared, the percussion sound lost its dulness, and the patient was discharged from the hospital as well as could be expected. (p. 238)

TURNING IN DRY LABOURS.—The difficulty of turning in "dry" labours, (when the waters have been long discharged), is well known to practical accoucheurs. This can be remedied in a great measure by injecting between the pains, from a pint to a quart or more of oil, directing the patient during

the process to change her position from back to side, or elbows and knees. The apparatus required is simple enough. An ordinary enema syringe and elastic catheter would do, if connected together by a piece of india-rubber tubing. After this simple operation the contractions of the uterus become less annoying, and the organ itself more pliable to the hand for the operation of turning. (Dr. Langer, p. 206.)

Ulcerations and Congestions of the Cervix Uteri and Vagina.—*Cauterization by Electric Heat.*—There is a peculiar benefit arising out of cauterization by heat, which places it in a different category from all other agencies of this kind, and especially adapts it for obstetric use, and this is, the contractility following its use, and the remarkable modifications of vitality which is caused in the structures treated by its means. Cauterization by electric heat is likely to prove a valuable means of treatment in the following affections: obstinate ulcerations and congestions of the cervix uteri, resisting all other treatment for a long time; vascular growths from the os and canal of the cervix, which often give rise to tremendous hemorrhages; also especially the vascular tumours or caruncles of the female urethra; warty excrescences; and a peculiarly obstinate form of patchy inflammation, apparently of scattered glands on the mucous surface adjoining the meatus, and which frequently give rise to intolerable pain and pruritus. It may also be useful in closing the minute holes left after the operation for recto- and vesico-vaginal fistulæ. The great obstacle to the success of the electric cautery has been the great size, the cost, and the complicated form of the battery required. But now one may be made but little larger than an ordinary glass tumbler. This battery consists of a cylindrical glass jar, with a wide mouth fitted with a cap of gutta percha. Within is a cylindrical roll of amalgamated sheet zinc, a quarter of an inch thick, six inches deep and about seven in circumference. Within this is a porous earthenware cylinder, containing a roll of platinum coil immersed in strong nitric acid. The peculiarity of this battery and the principal secret of its energy, lies in the size and form of the platinum roll. It is formed of a sheet of platinum folded upon itself, so as to present a very large negative surface. From this battery, the current is conveyed to the cauteriser. The cauteriser is of very simple construction. It consists of a straight tube of silver, ten inches in length, containing a piece of stout copper wire clothed with an insulating substance. These formed the two poles of the apparatus, and metallic connexion was made between them at the upper end by a fine platinum wire, while at the lower they were in communication with the battery by two wires secured by a clip. Under the platinum wire was introduced a piece of porcelain, which acted as the cauteriser, being made white hot on the completion of the galvanic circuit. (Mr. R. Ellis, p. 220.)

Uterine Douche.—*New Instrument.*—The apparatus is made entirely of india-rubber. A vessel holding about a gallon of water is placed above the level of the patient, from the bottom of which a pipe conveys the liquid to the uterus. A stopcock is placed in the pipe for the purpose of regulating the flow. No exertion or trouble is required in using this apparatus. It can be procured of Savory & Moore, of New Bond Street. (Dr. G. Hewitt, p. 208.)

Vaginodynia from Muscular and Fascial Contractions.—Cases are sometimes seen which have been mistaken and treated for various alleged affections of the uterus or its appendages. Pains are complained of and referred to the uterus, sacrum, or vagina. The uterus and ovaries on a careful examination are found healthy, but a tense corded transverse band is felt at some part of the vaginal wall, placed more or less deeply beneath the mucous membrane. It is extremely tender to the touch. Painful as these cases are, complete and instantaneous relief is afforded by division or rupture of the tightened contracted band. The best method of doing this is, after giving chloroform, to divide the band by means of a tenotomy knife. These contractions are either the result of chronic spasm of muscle, or of slow contraction of a portion of pelvic fascia from slight inflammatory action. (Dr. J. Y. Simpson, p. 215.)

Vesico-Vaginal Fistula.—*Operation.*—The steps of this operation are

as follows: First, Place the patient in the lithotomy position or upon her hands and knees. If in the former, chloroform may be administered. Introduce the bent speculum. Pare the edges of the fistula by knives made expressly for this purpose. Whilst this is being done it is necessary to hold the edge of the fistulous opening by a pair of forceps or transfix it by a curved needle. By means of a hollow-curved needle introduce wire sutures. These sutures must be passed through all the textures except the mucous lining of the bladder, this must be avoided. Instead of using any of the shots, buttons, and clamps, recommended, it is now found better simply to twist the wire together until the edges are brought accurately and firmly in apposition. Cut off the twisted wire, leaving sufficient to prevent the sutures giving way. Place the patient in bed, on her side with the knees drawn up. Insert and leave in the bladder a male elastic catheter, with bag attached to receive the urine as it drains away. (Mr. I. B. Brown, p. 287.)

MISCELLANEA.

ALCOHOL.—It has been lately discovered that when alcohol is absorbed into the blood, it is eliminated unchanged by the lungs and kidneys, and may be obtained by distillation from the urine. It is also found that, whilst in the body it is contained in very different proportions by the different tissues, most of all is contained by the nervous tissue, next by the liver, and next by the blood. The localization of the alcohol in certain organs explains its pathogenic influence over certain diseases of the liver, nervous system, and the kidneys. (MM. Lallemand, Perrin, and Duroy, p. 270.)

ATROPIA.—*Hypodermic Action.*—The severe nausea and vomiting so frequently resulting from the action of morphia, led to the trial of atropia, the alkaloid of belladonna. It is found that by the hypodermic injection of atropia the removal of pain has been sometimes as speedy, and there has certainly been less disturbance of the abdominal viscera, than when morphia is used. But the disorder of the functions of the brain is more marked—illusions of the senses, delusions of the imagination, and torpor of the muscles, are the characteristic toxical properties of atropia. The best solution of atropia for hypodermic use is made by dissolving one grain of pure atropia in exactly four drachms of distilled water, with the aid of two or three drops of dilute acetic acid. Five drops will be the commencing dose for an adult female, and seven for a man. If injected at bed-time sleep will soon follow, but the patient will awake two or three times in the night, and complain of feeling giddy and thirsty. There is never any approach to actual delirium, and the alleviation of pain *is always very decided*, and more lasting than after the use of morphia. It is of no practical importance to inject directly over the seat of pain, unless that be very superficial. (Mr. J. K Spender, p. 277.)

Internal Administration.—The following is the best form for internal administration. Atropia, 1 grain; distilled water, 5 drachms. Dissolve thoroughly with the aid of a few drops of diluted muriatic acid, and add of rectified spirit sufficient to make 10 drachms. This solution represents all the medicinal virtues of belladonna, it keeps well, and being of uniform strength, is superior to the very uncertain tincture and extract. Of the above solution ten minims is the commencing dose for the adult, given in a little water, on an empty stomach. In epilepsy atropia is sometimes curative, and seldom fails to afford relief. It is often useful in chorea and neuralgia. It often gives great relief in asthma; but its value is most strikingly shown in relieving constipation and in subduing irritability of the urinary organs. It is of much use in dysmenorrhœa. It has been known to act as a parturient, with a promptitude and efficacy superior to ergot of rye. When no spirit is employed in making the solution it may be introduced into the cellular tissue, if its action is required, without delay. (Dr. Fleming, p. 278.)

CHLORIC ETHER AND CHLOROFORM JULEP.—The following are the formulæ for these two useful preparations from chloroform. *Spiritus Formyli Terchloridi* (commonly called chloric ether):—Chloroform, ʒ v; rectified spirit of wine, sp. gr. 388 L. P.), Oj. Mix. Dose, ʒ ss to ʒ ij. *Mistura Formyli Terchloridi* (or chloroform julep):—Chloroform, ʒ ss; pure water, Oj. Mix thoroughly, with brisk agitation, for a minute or two, in a vessel capable of containing double the quantity. Dose, ℥ ss to ʒ ij. (Dr. T. Skinner, p. 253.)

CHLOROFORM.—*New Mode of Administering.*—Lay one single layer of a towel or a handkerchief over the patient's nose and mouth, taking care not to cover the eyes, and on this single fold pour the chloroform, drop by drop, until complete anæsthesia is produced. This method is perfectly safe, as the vapour is inhaled mixed with a sufficient quantity of air which is easily inspired through a single layer of any ordinary handkerchief. Anæsthesia is more speedily induced, and the quantity of chloroform required is much less. (Dr. J. Y. Simpson, p. 200.)

CODEIA.—This is only inferior to morphia for calming pain in that a larger dose of it is required, but it has a marked superiority over morphia in that it never occasions a heavy and agitated sleep, that it does not bring on indigestion, constipation, or vomiting. The last is a very troublesome symptom produced by morphia. Codeia produces a calm and restoring sleep, and must occupy an important place in therapeutics. Those stubborn and harassing coughs of bronchitis, and particularly of consumption, those violent pains of rheumatism, gout, and organic affections, of cancer for instance, are forgotten in the midst of the calm and agreeable sleep which codeia produces. (Dr. Aran, p. 284.)

IODIDE OF POTASSIUM.—It is a good plan to add ammonia to all medicines containing iodide of potassium. Five grains of the iodide with half a drachm of sal volatile are equal to ten grains of iodine alone. (Mr. J. Paget, Medical Times and Gazette, Jan. 4, 1862, p. 10.)

IRON-WIRE LIGATURES FOR ARTERIES.—Iron-wire may be made so fine, strong, and flexible as to admit of its being used instead of thread ligatures. It may be drawn into a knot with the greatest facility, and one end being cut off, the other must be left hanging out of the wound. If both ends are left, suppuration and irritation are sometimes excited. In using this wire as a ligature care must be taken not to pull it too tight, otherwise the wire will cut itself out. (Mr. T Nunneley, p. 253.)

NEW SALT OF IRON.—A magnetic phosphate of iron has been prepared by Mr. T. Lightfoot, jun., of Accrington-lane, Sheffield. This salt is very soluble in a solution of citric acid, and does not precipitate with syr. aurantii like most of the iron salts; thus forming one of the most agreeable chalybeates imaginable. It has been extensively prescribed in Lancashire, in albuminuria, diabetes, and various forms of heart disease, in which its action is most striking; while as a tonic in general debility it can scarcely be surpassed. The dose is four or five grains three times a day. It must be kept in solution with citric acid. (Mr. T. L. Gentles, p. 283.)

PODOPHYLLIN.—Podophyllin is a preparation from the root of the *Podophyllum peltatum*, or American May apple. It has been known for a long time in America. In a small dose, half a grain to a grain, it is a remarkable purgative and cholagogue; taken after breakfast one day it acts mildly and naturally by the same time next day. In doses of two grains it produces effects similar to those resulting from ten grains of calomel. Sometimes a single dose evacuates incredible quantities of bile. If, after this has taken place, another dose be given no effects whatever are produced. It may be given made into a small pill with soft extract of hyosciamus, or one-eighth of a grain of extract of belladonna, which prevents its griping. It may also be advantageously combined with compound rhubarb pill, one-sixth to one-fourth of a grain being given at a dose. Podophyllin ranks next to scammony as a purgative, but it is milder in its action. As a cholagogue it stands pre-eminent and alone, far before mercury, or any other drug. (p. 271.)

POISONING BY ACONITE.—*Nux Vomica.*—The actions of aconite and nux

vomica are antagonistic. In poisoning by aconite a few drops of tincture of nux vomica is one of the best antidotes which can be given. (Dr. D. D. Hanson, p. 279.)

PRUNUS VIRGINIANA.—This is the bark of the wild American cherry tree. It is considered one of the most valuable of their indigenous remedies. It is (1) powerfully tonic, (2) calmative of nervous irritability, (3) an arterial sedative. A saturated tincture is the best preparation. This, in doses of one drachm, affords relief in a most striking manner in the dyspepsia of highly nervous persons, and in that frequent complaint, "a sensation of sinking and pain at the pit of the stomach." In gastralgia, flatulence, and heartburn, it is beneficial. It may be used in the form of an infusion in cold water. When this is taken freely it sensibly diminishes the action of the heart. No satisfactory analysis has been made of this bark, but it contains amygdalin, and a peculiar principle not yet isolated. This bark is so abundant that it is not likely that any other will be substituted for it in the market. (Lancet, Feb. 8, 1862, p. 159.)

A COMMENTARY ON MIDWIFERY,

AND THE

DISEASES OF WOMEN AND CHILDREN, FOR THE LAST HALF YEAR.

It becomes a question of some importance in cases of Ovarian Cysts, and in Imperforate Vaginæ with distended uteri, and where relief downwards must be obtained, whether to make the puncture from the rectum or from the vagina. It generally happens that in cases of retained menses, from some congenital defect in the vagina, death follows sooner or later if the opening be made upwards from the vagina, and in such cases we believe that the safer method is to puncture the distended uterus from the rectum. Let us take a case of retention of the menses month after month for a year or two, from want of a vagina or natural outlet. What is the state of this short cavity? Is it lined with a mucous membrane, and so prepared to have communication with the external air, or does it more resemble any other shut sac, like the peritoneal cavity, which cannot bear atmospheric contact? We believe that a womb congenitally closed is not in a fit state to have free and unreserved communication with the external atmosphere; and moreover, we know that in such a case, if an artificial opening were made and kept open, it would not resemble the natural passage of the vagina and cervix uteri, which is in fact nearly a closed canal, except on certain occasions; whereas an artificial opening must always be more continually open, and therefore more completely subject to all the objections which atmospheric influence can produce. A womb which had never communicated externally, on account of some congenital obstruction, will not be lined with healthy mucous membrane, and we ought, therefore, if obliged for some temporary relief to open it, to do so through the rectum, if it so press backwards that we can readily do this. The contents of the uterine cavity will then be emptied into the rectum, and so the cavity will not communicate directly with the air, may gradually become more accustomed to act like a mucous membrane, and after a certain period of tuition, if needs be, an artificial vagina can be made, if there be room and opportunity. The same reasons will exist, in cases of ovarian cysts, to prefer the rectum to the vagina where an artificial opening downwards is deemed necessary. As a general rule we should say, that all openings into cavities not lined by mucous membrane are pernicious, if kept open, and that an opening into a cavity which is lined by old mucous membrane, such as opening the bladder from the rectum or even in the operation for stone, is not nearly so dangerous. Of course, in all these cases, we have but the choice of evils, and the most experienced practitioners will differ in opinion.

In the American Times for March 9, 1861, Dr. Percy reports a case in which, eight days and a half after the last intercourse, living Spermatozoa were found in the os uteri. These were examined by the microscope. From this fact it would appear that the ovum may be impregnated in such a case eight or ten days after intercourse, and thus apparently prolong gestation several days; and, secondly, a woman may be impregnated almost at any

portion of the period intervening between one menstrual period and another, i. e., if we proved that the ovum maintained its vitality long enough.

Dr. L. D. Harlow, in the American Journal of Medical Sciences for January, describes a new kind of instrument for Craniotomy, which we think much better than the spear-shaped instrument which we use in England. This instrument of Dr. Harlow, named by him the *Diaclast*, is figured at p. 207, and is shaped somewhat like a solid conical corkscrew, or more perhaps like a boy's top made into a screw, with a long handle. It is evident that this will be a most effectual instrument to enter and break up the bones of the skull, and be perfectly safe to the mother.

It is a great mistake in a case of Turning to delay the operation after the amnion has been ruptured. Indeed it is a comparatively easy operation if, at the same time you introduce the fingers to rupture this membrane, you make one operation of it, and continue the advance of the hand and arm till you can lay hold of the foot or leg before the womb has begun to grasp the child tightly. But sometimes you are called to a case where the water has already escaped some time, and the womb is violently contracting on the child, so as to make it difficult to push the hand high enough. Dr. Langer suggests that we syringe a sufficient quantity of oil into the womb by means of a gum catheter and a forcing-pump with a stop-cock; a pint or even a quart of warm oil may be injected with comfort to the patient, and the operation is considerably facilitated. But in such a case half the battle consists in being ready with your remedy, and it is very doubtful whether this recommendation will be followed, on account of the trouble and want of readiness of the instruments, especially if the case is urgent. Chloroform will have all the good effects of the oil in relaxing the muscular contractions, and with the arm and hand well smeared with lard, we need have little difficulty in accomplishing the operation.

It has been thought by some that if a woman die before labour from syncope, and without any blood escaping outwardly, she cannot have died from Internal Hemorrhage. It is known, however, that a woman may die from this internal hemorrhage without any external manifestation. Dr. J. G. Wilson, of Glasgow, gives us two cases of this description, one of which died, and a post-mortem showed a large collection of blood in a cup-like cavity of the placenta, and confined to this place by the adhesion of the placenta all round its margin to the womb—in fact, the placenta was entirely separated from the womb except at its margin. It was this circumstance which allowed a large collection of blood to take place and none to escape. So that when a woman is seized with this kind of sudden syncope, we must be prepared, with a knowledge of these cases, not only to form a correct diagnosis, but to expedite delivery, both for the sake of mother and child. No medicine, no plug, nor any of the usual remedies to check hemorrhage, will do in these cases. Nothing will secure the woman but the birth of the child and the contraction of the womb. We would therefore recommend the practitioner to dilate the os uteri by the fluid dilator of Dr. Barnes, as described in this volume, which can be done in two hours, and then either to introduce his hand and deliver at once by turning, if the life of the woman be in imminent peril, or, if the urgency of the case be not so great, to rupture the membranes and give a good dose of ergot of rye. Doctors Hardy and Mc Lintock mention two similar cases in their "Report on Midwifery and Puerperal Diseases," and another case is mentioned in the "New Medical and Physical Journal" for 1813, No. 38, p. 535. We have also had one case in our own practice, which proved fatal, the woman only just surviving the birth of the child. This subject of concealed hemorrhage in the womb before the birth of the child has not met with the attention which it deserves, and we would recommend the reader to refer to Dr. Wilson's paper in this volume, and to the papers of Dr. Bell of Glasgow, and Dr. Hicks of Guy's Hospital, as mentioned by Dr. Wilson.

It sometimes happens that Flooding after labour continues more or less, for hours and even days, without any relief from the usual remedies. We may think that we have caused the womb to contract, but it often dilates

again in our absence; we may have given all our usual styptics without effect, the powers of the woman decline every hour, and she is drifting into that state of syncope which will either end fatally, or this syncope itself will be the means of arresting the discharge, and from that period she may slowly rally. We have often attempted artificially to produce the state of nausea somewhat resembling syncope, which in such a case arrests the hemorrhage, by grain doses of ipecacuanha every half-hour or hour, sometimes in less doses, but oftener repeated. By this means we have often saved the loss of a large quantity of blood, and have thus been enabled to make a dose of brandy *tell* better on the circulation when we have been compelled to use it. The principle is that the state of syncope is friendly to the arrest of flooding; but it is a dangerous state when caused by loss of blood in such cases, and if we can imitate it by medicine it is a safer remedy. Ipecacuanha is certainly safer than antimony, and we would strongly recommend it in preference. But in our next case we intend taking a hint from Dr. Brinton's way of arresting hemorrhage from the lungs in the latter stages of phthisis. His plan is to give the tincture of Digitalis, from 30 to 90 minims in the course of the day, divided into four or six doses, till the pulse be reduced to 40, 50, or 60 in the minute. You may continue this drug for several days, or longer, watching its effects on the pulse, and combining an acid with it during the violence of the hemorrhage, and the tincture of the sesquichloride of iron during convalescence. In the large ulcerations of the third stage of phthisis, we know what copious bleedings will sometimes come on. In these hemorrhages nothing answers better than Dr. Brinton's treatment by digitalis, and we do not see why we should not transfer the plan to hemorrhages from the womb, when these occur some short time after labour, and contiuue from hour to hour with the prospect, in a day or so, of having a serious state of prostration and even fatal syncope.

Gallic acid is, of course, a favourite remedy with most practitioners for checking hemorrhage, and so is diluted sulphuric acid, with others. Dr. Earle recommends a combination of the two.

With respect to the Absorption of Pus or other poisonous matter by the veins of the womb, some light has been lately thrown on the subject by Dr. Wilks, in a most able and lengthy report on Pyæmia in Guy's Hospital Reports, vol. vii. It seems that when fibrin is absorbed or taken into a vein it generates fibrinous effusions elsewhere, and when pus is absorbed it causes purulent effusions only: each produces its like, in the same way as cancer-cells set up cancer-cells elsewhere, and a melanosis of the eye propagates its black tumours through the body, and an osteoid tumour gives the starting point for bony growths throughout the system. Where bone is diseased, it has been long thought that the Haversian canals are the channels through which the pus germs are conveyed into the system. Now these canals are permanently open, and incapable of closing like the ordinary blood-vessels; they are, therefore, peculiarly suited to carry pus germs in diseases of bones. The veins of the womb are likewise sometimes very open, and when the substance of the womb is flabby and powerless, either from flooding or from weakness, we can see how they will be, like the Haversian canal, the ready means of conveying pus germs and other poisons into the circulation, being very near the cava ascendens, and having no valves or other obstructions to their course. When pus or other poisons are taken into the circulation, the first effect is to cause coagulation either in the large vessels or in the capillary system of the viscera. At first, then, we have coagulation, and next suppuration in a part; but this may take place without the original pus itself being conveyed to the viscus, only, we suspect, the germs of pus or some other poison. For example, in the lungs hepatization takes place before pus is formed, so we may have a very minute portion of poison taken up by one of the veins of the womb, and carried with great rapidity to some distant organ, so as to produce the most dangerous effects.

We consider the remark made by Dr. Graily Hewitt, that Puerperal Fever oftener occurs in women who have flooded copiously, to be a true one. In women whose wombs contract well after labour we do not find this disease to

occur so often as in weakly flabby uteri. In these the parietes of the womb do not properly close the venous orifices, and the road to the heart is very direct and very near; there are no valvular obstacles to any poisonous matter which may get into them, and hence the fatality is often alarmingly rapid. What is the practical conclusion to be drawn from this remark? Always when you apprehend flooding after labour in weakly flabby subjects, and especially when the uterus contracts feebly before the birth of the child, give a dose of ergot of rye a little before the completion of labour, and as soon as the child is born and before you attend to it, get hold of the uterus during its last contractions and try to make it get rid of the placenta as soon as possible, and when this is done, don't be satisfied till you have got the fundus well contracted either by the hand firmly clasping it, or by the assistance of cold water and a well-applied bandage.

In the new Lying-in Hospital at Munich there seems to have been numerous cases of Puerperal Fever, and we cannot throw off the impression that the air of an hospital is very dangerous to lying-in women. Some writers assert that the mischief is caused by an epidemic poison in the air of a locality, but others assert, with more truth, we think, that the cases can be traced to the medical practitioner who is going about from one or more bad surgical cases to his female patients in labour. Dr. Uvedale West mentions a case in his own practice lately, which we think very decidedly confirms this view, and he himself also thinks so. This patient had been attended by a midwife, and was attacked by peritonitis and died. Dr. West had a post-mortem and found a large unilocular cyst of the ovary, the peritoneal covering being black and gangrenous. His attendance in this case and the post-mortem "were followed in his midwifery practice by an unusual proportion of febrile diseases of the puerperal state, caused either by direct contagion or by some epidemic influence." It seems that in such cases the practitioner cannot get rid of the poison which is about his person. Dr. West says that he "felt a wholesome dread of the possible consequences to his lying-in patients of his having conducted this unlucky autopsy," and "took every possible precaution by way of disinfection, such as ablutions, entire change of wearing apparel," and yet out of 48 cases, ten were attacked severely, although none died. M. Semelweiss maintains that the mischief arises from surgeons attending post-mortem cases, but we think that this is not sufficient to account for all the cases, as some arise where the surgeon has not been to a post-mortem examination. We consider that any bad surgical case, especially where there is pus, will convey the poison to the woman. The inner surface of the womb is always more or less affected, and may be looked upon as the starting point of the disease. The mucous membrane chiefly may be affected, pus will be found along the fallopian tube or tubes, and general peritoneal inflammation may ensue; or the inside of the womb is like a large wound covered with pus, which gets into one or more veins, causing pyæmia. In these cases we find coagula and pus in the veins at the insertion of the placenta, and even extending to the vena cava and the heart itself. We would again strongly recommend practitioners in midwifery to attend to two things:—1. Never meddle with a bad surgical case if it can be avoided. 2. Look well to the contraction of the womb after the birth of the child. It will be difficult for the dangerous symptoms of puerperal fever to take place when a womb contracts well after labour, but where it remains flaccid, and the mouths of the veins open, the patient is necessarily more liable to this frightful disease. We think that the subject of contraction of the womb after the birth of the child is too much forgotten or neglected, and we can assure our younger readers that this neglect will be a frightful source of trouble in their puerperal cases. To take pains to ensure the contraction of the womb *after* labour is almost as important as its contraction *before* the labour, whereas the one is always attended to, but the other often omitted. We have before mentioned the way in which the womb can generally be made to contract, viz., by grasping the fundus as soon as possible after the birth of the child, and even before the whole child has passed out, so as, if possible, to continue or prolong the last and violent efforts of the organ. This is just

the time when the contractile power is generally the most effective, and by prolonging the effort but a minute we do more to gain our end than by lengthy efforts afterwards. It has already been noticed that this kind of assistance by the practitioner is the more necessary as these puerperal cases will often occur in women who are weak and flabby, and who are consequently disposed to flood considerably. We should always, in such cases, be prepared with a good dose of ergot of rye, to be given just before the birth of the child, but this ought never to make us careless about grasping and even pulling downwards the flabby fundus uteri, which will be found high up and uncontracted. We have found it most useful to soak a napkin in cold water, fold it up a little and compress this on the fundus, pulling or pressing downwards and backwards towards the pelvic cavity; the cold napkin of course ought to be changed for others as soon as it loses it coldness. But remember that as soon as the child is born the intestines often fall down over, and in front of the womb, and if you now violently grasp and pull the fundus downwards you are doing so with the folds of the intestines under your hand. This would therefore be highly improper, and even dangerous to the woman. To avoid this, slip your fingers and bare hand upwards from the pubes along the surface of the womb so as to be sure that you have not got any of these intestinal folds under your hand; you may now grasp the womb confidently and with almost any degree of force.

In Leibig's Letters on Chemistry, p. 229, we have the following remarks on Cadaveric Poison:—"It is a fact that dead bodies in dissecting rooms frequently pass into a state of decomposition which is communicated to the blood in the living body. The slightest cuts with the scalpels used in dissecting often cause a very dangerous and even fatal disease. The observation of Majendie that putrid blood, brain, bile, or pus, when laid on fresh wounds, produce in animals vomiting, languor, and death, after a shorter or longer interval, has not yet been contradicted." This remark has long been ascertained to be true by all medical practitioners, but it has been practically and curiously confirmed in Vienna lately in the Lying-in Hospital, where, as far as we can learn, the physicians and not the surgeons, as in many other metropolitan hospitals, attend women in labour, thus reversing the practice which is common in the large provincial towns in England. In the Vienna Lying-in Hospital in 1846, the labours were attended by both physicians and midwives, but the fatality of the physicians' cases was four times greater than that of the midwives. This was ascertained to be owing to both Dr. Semmelweiss and the students attending post-mortem examinations, "that the cadaverous smell on their hands, in spite of repeated washings, did not disappear till after a considerable time, and that the pupils not unfrequently proceeded to the examination of women in labour immediately after dissecting a dead body." Dr. Semmelweiss then adopted a rule that every one should wash his hands in chlorine water before examining a woman, and immediately after this the physicians had no more fatal cases than the midwives. These facts are not sufficiently recognised by medical men, or at any rate, although well known they are not acted upon. What can be more dangerous than for a surgeon to go from a case of strangulated hernia, where the bowel has been gangrenous for a considerable period, to a woman in labour, or from any other kind of bad surgical case where purulent and gangrenous discharge exists? We maintain that operating surgeons ought not to attend labours at all, but that these ought to be attended by physicians and general practitioners, who seldom or never meddle with these bad surgical cases. In London we believe that it has long been the custom for operating surgeons to avoid the practice of midwifery, whilst most of the hospitals have physicians who devote their attention to this department of medicine. Further, we disapprove of the practice now becoming too common, of the physicians themselves operating in such cases as ovariotomy, &c. Let the departments of surgery and midwifery be more completely divided, and we shall hear of but very few cases of puerperal fever.

A short account of the Treatment of Puerperal Fever at Giessen, by Professor Von Ritgen, reminds us how erroneously this dreadful malady is some-

times treated in this country. Dr. Von Ritgen's practice consisted in giving morphia, ten grain doses of camphor, and one grain doses of quinine, as will be seen by referring to the article at page 213. In these poison cases we ought to remember that fever, and even inflammation, are favourable symptoms. Those cases are the most rapid and dangerous where the patients are struck down at once by the force and violence of the poison without having time given to the system to *get up* a fever or inflammation. It may seem paradoxical, but it is acknowledged by our best writers, that the fever and inflammation are *conservative processes* to throw off the poison through one structure or another. Those cases do the best where fever and inflammation are set up, and therefore we should be extremely cautious, even when those symptoms exist, how we attempt to check them by leeches, antimonials, low diet, or any other depleting means. In most of those so-called inflammations and fevers it is the poor and rapid pulse which alarms us, and not the full and bounding one. Let us, therefore, in our puerperal fevers, keep the patient *well up* from the first, notwithstanding what might appear to be fever and inflammation. And we would here recommend to our junior brethren in their cases of labour not to be afraid of giving the patient a mutton chop forty-eight hours after labour instead of keeping her on low diet for nearly a week.

Let us be careful how we manage such a case as the following:—A woman is seven or eight months advanced in pregnancy, she looks *puffy* about the hands and face; we examine the urine and find it albuminous; in due time she is attacked by convulsions and coma. What are you to do? Bring on labour with as much speed as you can. This generally occupies twelve or twenty-four hours and often some days. One of the best plans is that lately adopted by Dr. Barnes. We assume that the os uteri is undilated and the membranes unbroken. First, puncture the membranes with a knitting needle, or small male catheter, and then proceed to dilate, first, the vagina and then the os uteri, by means of a caoutchouc dilator distended with water. You can introduce this into the os uteri, and gradually dilate for half-an-hour or an hour at a time. You imitate with tolerable accuracy the method which nature herself adopts. Labour pains are gradually brought on and the dilatation of the os is completed in two, three, or four hours, so as to enable you to deliver by turning, or to leave the process to nature when she is doing her work well enough. To accomplish artificial delivery in two hours in a case of need, instead of having to wait twelve or twenty-four, is certainly an improvement in this kind of case where the life of mother and child depend on expedition.

Ovariotomy continues to be a common operation. In the Lancet for Nov. 30, 1861, p. 519, are reported thirteen cases, of which nine were successful and four fatal. Although every case differs a little from the rest, yet there is a general resemblance both in the cases and in the management of them. Adhesions more or less extensive were present in four of the successful cases, and in all the unsuccessful ones. Mr. Brown still prefers to secure the peduncle at the abdominal wound by the *clamp*—this clamp being an ordinary pair of carpenter's callipers. The advantage of this clamp is that it can be removed in a few days, and the peduncle gradually retracts within the abdominal cavity without any ligature having been used. On the other hand, it is said to put the parts too much on the stretch causing vomiting, retching, or straining. On an average two out of every three patients may be expected to recover, especially if the air of the apartment be kept good, and if small and separate wards be reserved for these cases. These kinds of operations, as well as all other capital surgical cases, do not bear congregating; they do much better when kept apart, and even alone. This remark seems to apply especially to all surgical cases in which wounds of the peritoneum are involved. In the London Surgical Home, under Mr. Brown's care chiefly, the success was thought to be increased by the preparatory treatment, which consisted of warm baths twice a week for three weeks prior to the operation, and the administration of a combination of the tincture of arnica and muriated tincture of iron, with attention to the general health. In a case under

the care of Mr. Hutchinson the tumour had to be tapped in three places owing to its multilocular character, and was gently drawn out of the wound as it emptied itself.

We are glad to see that the use of silver wire is increasing, in those cases where ligatures of deep parts, and in the cavity of the abdomen, are required. In our last Commentary we alluded to cases of lead bullets remaining for years in internal parts, and to a case where the veins of the spermatic cord were tied by a metallic ligature and the parts healed well with the ligature inside. These are perhaps different cases from those in which the ovarian peduncle or adhesions are tied in the peritoneal cavity, but still they confirm the principle that metallic ligatures cause comparatively but little if any suppurative inflammation. Let us be careful, when we strangle an ovarian peduncle, or a peritoneal adhesion, to do so with only just sufficient force to check hemorrhage, but not completely to devitalize the strangled portion. To strangle the part so completely as this would necessarily cause it to slough, and it would matter but little in such a case whether the slough were caused by wire or thread. This distinction is not sufficiently attended to. A wire ligature drawn so gently round an ovarian peduncle, or other internal bleeding part, as not to devitalize it, but only just sufficient to arrest hemorrhage, may prove a useful suggestion.

We ought to take particular notice of the experience gained by the leading operators on ovarian cysts. These gentlemen are candid in acknowledging where they fail as well as where they succeed. It is in this way that we shall arrive at great success in all capital operations. Mr. Spencer Wells has laid down some truly practical rules, many of which he has learned from his own experience. Some of these are as follows:—It is seldom right to recommend ovariotomy where only a single cyst exists, until simple tapping with iodine injection have both failed. In compound cysts, or semi-solid or solid tumours, where the life is severely threatened, it should be done before the general health gives way. As soon as the tumour is freed from peritoneal adhesions, it should be tapped by a large syphon trochar. As the fluid escapes and the cyst becomes flaccid, it is either to be fixed by a hook and withdrawn, or drawn over the canula and tied over it, to prevent any ovarian fluid from entering the peritoneal cavity. As the cyst escapes, flannels wrung out of water at 96° are to be carefully wrapped round it to protect the peritoneal cavity. As secondary cysts are drawn to the opening, they are to be emptied, either by the trochar being passed on through the canula which is still tied within the first empty cyst; or by opening this cyst, passing one hand within it, and breaking down secondary cysts, while the other hand withdraws the tumour as it is emptied. When there is a solid or semi-solid mass, so large that it will not pass easily through the opening, this must be carefully enlarged to the necessary extent.

If, as the tumour is withdrawn, omentum, mesentery, or intestine, are seen to be adherent to it, the adhesions must be carefully broken down with the fingers, or divided with the scalpel or scissors. When intestine is so firmly adherent that it cannot be safely separated, the adhering portion of cyst should be cut away, leaving it attached to the intestine, but removing the inner secreting lining membrane of the cyst. Any portion of omentum separated, should be carefully held to see that no bleeding vessel is returned. Any portion which appears to be much altered from the natural condition, or torn in the process of separation, should be cut away; and any bleeding vessel should be stopped by torsion, or by ligature. If by ligature, the ends should be brought out through part of the wound. The ligature should not include any portion of omentum—simply the bleeding vessels. When the whole of the tumour has been withdrawn, it will be found to be attached to one side of the uterus by a pedicle which varies very much in length and breadth, and always contains large blood-vessels. This pedicle is first compressed by the chain of an *écraseur*, or by a clamp, close to the junction of the pedicle and cyst. The tumour may then be cut away, great care being taken so to arrange the flannels that no ovarian fluid can enter the peritoneal cavity. The lips of the wound are then held apart, and the opposite ovary

is to be examined. If diseased, it must be withdrawn, its pedicle secured, and the diseased organ cut away. If healthy, it is left undisturbed. A careful examination is then made, to see if there be any bleeding vessel at the spots where adhesions have been separated. Any such vessel may be secured by torsion, or by the pressure of a needle passed across it. Any blood or ovarian fluid in the abdomen or pelvis must be carefully removed by soft sponges wrung out of water at 96°. The upper part of the wound is now to be closed by passing gilded harelip pins through the whole thickness of the abdominal wall, at intervals of an inch. Each pin should perforate the skin about an inch, and the peritoneum about half-an-inch, from the incision on either side; so that when the two opposed surfaces are pressed together upon the pin, two layers of peritoneum are in contact with each other. These two layers adhere together very rapidly; prevent pus or other secretions of the womb from entering the peritoneal cavity; prevent adhesion of omentum or intestine to any part of the inner aspect of the wound not covered by peritoneum; and secure such firm union that a ventral hernia cannot occur after recovery. The peduncle is next to be secured permanently. When the clamp does not drag on it too much, it may be left lying across the wound; but it will often be better to secure the pedicle permanently by ligature, and remove the clamp which had been used to secure it temporarily. Close below the clamp, the peduncle is transfixed by a needle which carries strong twine; and each ligature is so tied as to include a portion of peduncle of about a finger's breadth. One general ligature is then tied tightly around the whole as a security against bleeding from any vessel which may have been punctured. The smaller the portion included in each ligature, and the tighter it is tied, the more rapid is the subsequent process of separation. When the ligatures have been securely applied, the clamp is removed, and any superfluous portion of cyst is cut away; but care must be taken to leave enough beyond the ligature to prevent any danger of this slipping off. When the peduncle is so long that the stump (or portion strangulated by the ligature) can be fixed outside the abdominal cavity, it is to be brought out at the lower part of the wound, and fixed there by a harelip pin, which is to be passed through it as well as through both edges of the wound. It is important that the ligatures should be on a level with the skin, and that the stump should be surrounded by dressing which separates the sloughing tissue from the raw surface. When the ligatures are fixed at the level of the peritoneum, raw surfaces surround the sloughing stump, and a sloughing condition of the wound, or putrid infection of the whole system, may possibly result. When the peduncle is so short that the stump cannot be brought to the surface without great traction upon the uterus, the common practice has been to bring the ligature through the wound, and fix the free ends securely outside. In one case, they were carried through the inguinal canal beside the round ligament, and the abdominal wound was closed. In another, they were cut off short and left, the wound also being closed. It remains for further experience to determine which of these plans is to be preferred; or if only the vessels should be tied, instead of the whole thickness of the pedicle; or if acupressure can be applied successfully; or if it would be safer to trust to the *écraseur*.

In cases of ovariotomy the peduncle is often brought to the external wound and held there by a clamp, thus putting the parts on the stretch. Dr. Robert Tanner, of Ledbury, asks why we do not make use of the hot iron in the way that sowgelders do on inferior animals. These men make a small wound in the abdomen, draw the ovaries through it, and taking hold of the peduncles by a clamp they *sear off* the organs with a hot iron made rather sharp; they then thoroughly sear the maternal portion, and loosen the hold of the clamp a little to see if any oozing take place, if so, they sear a little more till all oozing cease, and then return the parts into the abdomen, closing the abdominal wound by a stitch. If this process would answer in the human female, it might be useful to apply Mr. Ellis's electric cautery, which we have no doubt could be done by a circular wire grasping the peduncle, almost like the écraseur: but there would be this objection which we have not seen

answered, viz., the production of a slough, more or less, which would cause pus to form on its separation, and we know that pus collected internally, in however small a quantity, is highly objectionable. It would be this formation of a slough which would be dangerous when the peduncle had been tied by a silver wire ligature, otherwise we think this kind of ligature would be an improvement, and we still think that if the parts could be so gently strangled as not to completely devitalize them and yet to check hemorrhage, the wire ligature might be safely employed.

We have two or three valuable hints to give about the operation of Vesico-vaginal Fistula. The patient is placed in the usual position as for lithotomy, and the parts exposed to view. It is better to hold the fistula by a curved needle instead of a pair of forceps; the needle not only serves this purpose but can be armed with the wire ligature to be used afterwards. Both this and the other needles, of which the operator ought to have a great variety ready, should be carefully passed through the coats of the vagina but *only through the muscular tunic* of the bladder. If you thrust them carelessly through the mucous coat as well, you will probably cause the whole operation to fail, as you lay the groundwork of fresh fistulæ. Formerly when the wire had been passed it was secured by shots, but Mr. Brown thinks that to twist the wire round and round with the finger, or, if too deep to reach, by Weiss's self-holding forceps, is a better plan. We cannot impress it too strongly on young operative surgeons to avoid transfixing the mucous membrane of the bladder, and to bring into juxta-position the corresponding tissues. In all these operations, and similar ones, where adhesion of *pared surfaces* is required, we should bear the same principle in mind, viz., to apply the edge of a serous membrane to a serous membrane and not to a mucous one, and a muscular texture to a muscular texture and not to a mucous or serous one. This seems but a little matter to attend to, but success will often accompany attention to many little things, instead of to one more important.

The young practitioner will not unfrequently meet with a Cyst on the inside of one Labium pudendi. He may mistake it for an abscess, or even for a hernia. He will soon diagnose it by its long existence, its moveable and cystic feel, absence of impulse on coughing, and by the absence of the soreness of purulent collection. He will naturally open it and there will escape a thick glairy liquid or a darker chocolate-coloured fluid. We think that the treatment of these cysts may be considerably simplified. In London they are generally treated, we believe, by simple puncture, the seton, injection of iodine, and dissecting out. Mr. Baker Brown dissects out more or less of the cyst, and Dr. Oldham often employs the seton in Guy's Hospital. We think that we possess a still more ready method of treating them, and one almost invariably successful. Put a bit of nitrate of silver, the size of a swan shot, or smaller, or larger, on a bit of paper near a gas light; take your silver probe, heat it in the gas light, and immediately plunge it into or upon the bit of nitrate of silver; it soon cools and adheres well to the probe. You now slit open the cyst, empty it of its contents, and immediately wipe out the whole interior with the charged probe, and apply some dry lint or cotton wool; adhesive inflammation is soon set up and the cyst disappears. We can carry out the same simple treatment in all small cysts on the eyelids, and in strumous cervical glands, which can thus be cured without leaving the ugly scars which are so often seen. In the same way, but only introducing the probe, more subcutaneously, as in subcutaneous nævi, we can radically cure enlarged bursæ. In these cases, however, the bistoury should be pushed through the sound skin at some distance from the bursa, so as to evacuate its glairy contents, and the charged probe should then be carried along the track which the bistoury has made, and the cavity of the bursa *wiped about* with the nitrate of silver.

It is almost gratifying to find that eminent men are sometimes wrong in their opinions, and it is highly creditable to them to acknowledge it. Our good opinion of them is not diminished by their failures because every candid man must himself acknowledge that he too often fails. An experienced

surgeon diagnoses a large Fibrous Tumour of the ovary, perfectly moveable and free from adhesions. This tumour being perfectly moveable, beginning from one side, and growing rapidly, was at once suspected to be ovarian, and capable of being removed by operation. We confess that we should easily have distinguished it from an ovarian tumour free from adhesions, by the following simple method. We should have placed the finger of the left hand on the os or cervix uteri, and with the other hand we should have pressed the tumour downwards towards the finger so placed. We should at once have felt that the body intervening between our two hands was a solid one, and that it was the uterus itself or some tumour intimately connected with it. No free ovarian tumour, without adhesions, would have the same feeling of a solid body between our two hands as would a solid and large uterus. It is not our purpose, however, to criticise, but to draw a lesson. Suppose that you make an exploratory puncture in such a case, and find a tumour of the womb instead of an ovarian body, what will you do next? Will you proceed to extirpate, or close the wound and acknowledge your mistake? We should not hesitate to close the wound at once and proceed no further. It is wonderful what ill-usage the womb will bear *from within*, and how little *from without* will prove fatal. You may excise, scrape, enucleate, and almost tear tumours from within the womb, taking care that you interfere as little as possible with its structure and never go through its peritoneal covering, and your patient may do well—but if you do this from the abdominal cavity you necessarily do it through the peritoneum, and your case will very probably be fatal.

At page 228 will be found a description of a Syringe used by Mr. Spencer Wells for Injecting Iodine into Ovarian Cysts, without the risk of admitting air. In some of the early cases too much iodine and spirit of wine were injected. It is now found that a scruple of iodine and half a drachm of iodide of potassium to the ounce of water, is better than a large quantity dissolved in alcohol. From one to two ounces of this solution may be left in a large cyst, but eight or ten ounces may be at first injected, so that all parts of the cavity may be washed with it, and the small quantity of one or two ounces left in will do more good than harm.

At one of the meetings of the London Obstetrical Society, Dr. Tyler Smith showed an instrument, by means of which Polypi could be readily taken from the uterus. It consisted of a rod and winch with double canula, made sufficiently strong to carry wire, and bear tension enough to cut through the neck of a polypus at once. This no doubt is a very good way, but we think that our own plan is better, and much more simple. You sometimes meet with a polypus with a very thick peduncle, and with other kinds of growths from the inside of the womb, which it would be difficult to strangle. We would recommend that the two first fingers of the left hand be pushed through the os uteri, and along the polypus or tumour to its origin; the two fingers by being a little separated and hooked will then be able, in most cases, to bring the peduncle considerably downwards, and we need not be afraid of using force almost enough to cause a degree of inversion of the womb. Now introduce a pair of long-handled but very small scissors, the points being curved almost at a *right angle*, so as to cut almost horizontally. These points may be made to cut almost downwards and inwards, by regulating the angle, and having them either sharp or blunt according to circumstances. A long small-pointed knife, with the end bent in a similar way, may be preferred by some. The common curved scissors are very ill-adapted to these cases, unless we can *invert* the womb and bring the growth down through the os uteri. Remember that when you have to introduce a pair of curved scissors through the os uteri, you will by no means cut horizontally but almost directly upwards, but when the scissors are bent at the angle we have suggested, a very limited range will be enough to cut through the largest peduncle, by taking plenty of time to *nibble* at it with the points of the instrument; and this *nibbling* process is sometimes much better than a more expeditious one, and more resembles the écraseur and even the ligature, but we think it much more easily done, and is equally efficacious. The

operator will be able to succeed in all or most cases when he can get his two fingers through the os uteri. Dr. J. B. Hicks has invented an instrument to remove these polypi, which seems likely to be successful where the peduncle can be secured. It consists in the adaptation of rope made of annealed steel-wire to the shaft of a screw-écraseur, whereby the flexibility of the rope is combined with nearly the strength of the chain. This annealed steel-wire has the greatest strength and flexibility. We have no doubt that such an instrument will be found very useful where the neck of the polypus can be properly secured, but not in other cases of tumours where there is hardly any peduncle at all, and where the ligature could not be properly secured. In these cases we think that the angular scissors or bistoury which we have suggested would answer the purpose better. In some malignant diseases about the os uteri it is not well to drag down the womb at all, in such cases we think Dr. Hicks's instrument will be very valuable. The part can be separated by pulling it down or displacing it, and we shall not have the hemorrhage which we might have by using the knife or scissors. The use of powerful styptics in some of these malignant cell-growths, is sometimes remarkable. The vitality of these cells is feeble, and their contents can be coagulated through their walls. Tannic acid is one of the best styptics for this purpose. It may be applied to the part in powder, or you may mix the tannic acid in a saturated solution of alum. Two drachms of tannic acid to half a pint of a saturated solution of alum, applied every day will reduce the more tender parts, but will not have the same effect on the parts which approach healthy tissue in character. Therefore it has the best effect on the external portions, and less effect on the deeper parts. Sometimes a lotion only half this strength is sufficient. The tannin when used thus, being insoluble, remains on the parts a considerable time. We think the use of this styptic might be extended to other growths of feeble vitality, and especially to fungoid bodies low down in the rectum.

Increased experience only makes Dr. Coghlan value still more his knife for dividing the cervix uteri in cases of Dysmenorrhœa. There is no doubt that when the cervical canal is very contracted and the discharge thick, the function of menstruation partakes slightly of the process and pain of abortion. In such cases to dilate the canal is attended with great benefit. We may do this by gradual dilatation by means of sponge-tents, by bougies somewhat resembling Mr. Wakley's instruments for dilating strictures of the male urethra, or by the more rapid process of incision. Dr. Simpson's instrument we have referred to before. This instrument is passed up the cervical canal and *cuts as it is withdrawn*, and is somewhat like the common hernia knife. Dr. Coghlan's instrument *cuts as it proceeds upwards;* it divides the canal on *both* sides, while Dr. Simpson's only divides *one* side. However narrow the canal may be, it divides and enlarges it as it ascends. We confess we see but few advantages in these things over the simple knife of Dr. Simpson, which is easily introduced without the aid of the speculum, and cuts as it is withdrawn. Dr. Coghlan's instrument is no doubt a very neat and useful one, but nothing can exceed the ease and simplicity of Dr. Simpson's knife. When the canal has been divided, you must not forget to introduce a metallic plug, so that the parts may heal while in a state of dilatation. The instrument devised for this purpose by Dr. Coghlan is figured at p. 195. where the reader will find a description of its use. In many cases we think this dilator may be very useful without any incision at all, and probably will be preferred by most practitioners to the cutting instrument in all cases.

It is sometimes curious to trace the connection between one disease and another. In a disease like phthisis, we all know how rapidly it becomes confirmed when the assimilating organs become more than usually at fault, but we have not paid sufficient attention to the fact, that in women the womb may be the real cause which first throws the digestive processes wrong, and that this is rapidly followed by confirmed phthisis. In women, therefore, if the digestive powers are impaired, examine the womb and you may find ulceration of the os or cervix, or a state of congestion, or even denudation of the internal mucous membrane: and by remedying these things, you may restore

the tone of the digestive powers produce a healthy manufacture of blood, and thereby avert the tubercular deposits which might have soon appeared.

To check the too copious secretion of milk, belladonna is certainly useful: as will be seen by referring to our former volumes. Camphor mixed with glycerine is found by Dr. Harriss to be preferable. He uses a saturated solution, gently applied over the breast with a flannel, several times a day. We have had no experience of this remedy.

In viewing the diseases of children generally, we must warn our younger friends not to look for those special and limited diseases which they will find more particularly developed as age advances. For example, in the adult and more aged patient you may have alone almost any one disease, such as pneumonia, bronchitis, inflammation, or congestion, or other diseases of particular and limited portions of the brain; but in children the whole system becomes more rapidly and generally affected. In them we have but few chronic and limited diseases, but more acute and diffused ones. In them the arterial system preponderates over the venous, and this is gradually reversed as age advances. This remark is not only applicable to internal diseases, but also to affections of joints, in which all the structures are more rapidly implicated than in later years: but although these little patients are much more liable to acute and dangerous diseases, they have also a greater power of repair, and when a disease or deformity does become chronic during the growth of the body, it is pleasing and astonishing to see what struggles the system makes to regain its original type and mould.

We have often noticed how variable is the pupil in diseases of the brain in children. As a rule, in effusions on or in the brain, the pupil will be dilated: but in some cases this will vary, owing probably to the disease implicating or not the structures adjoining the optic nerves. In some forms of apoplexy the pupils are actually contracted, as for example when there is fluid in the ventricles which descends to the base of the brain, and where there is a clot in the pons varolii. Dr. Wilks has noticed that even the position of a patient will alter the pupil. In a case of ventricular effusion with softening of the brain in an old man, when he laid on his right side, the right pupil was dilated, and when on his left side it was the left pupil which dilated, showing that simple gravitation of the fluid was sufficient to produce these effects. At the commencement, therefore, of diseases of the brain in children, dont depend too much on the pupils. You may have disease of the periphery or of the base, with contracted pupils at first; but let the disease advance, and the pupils will at last give you a more certain diagnosis.

The treatment of Nævi in children often requires great care, especially when existing about the eye, or on the face or scalp. Sufficient has been written on this subject to make us all learned enough, but have we practically paid attention to what we have read. The cutaneous nævus, when very superficial, may be treated by applying a little nitric acid or caustic potash, taking care to confine the application to the part diseased, and not to extend it more than one line beyond the affected part; using a paint brush or glass rod for the nitric acid, and having a bit of blotting paper ready to absorb any superfluous quantity. In using the potassa fusa to a child about the face, it is often difficult to make the little patient keep quiet. For this reason we generally put a small bit of the caustic on a bit of adhesive plaister, and apply it with accuracy to the diseased spot; the plaister keeps the caustic exactly where we require it, there is no rubbing required, the movements of the child cannot displace it, and in a few minutes, more or less according to the depth of the nævus, we take off the plaister. If we wish to be still more particular, we make a little hole in a bit of plaister, the exact size of the nævus, and put this plaister over the part so that we can see the nævus through the hole; on another bit of plaister we then stick the piece of potassa fusa and apply it to the hole, a little oil having previously been carefully wiped round the nævus on the sound skin, so as to prevent the caustic spreading under the plaister. But we are inclined to think that after all the best application is the fine point of a red-hot wire; it is not more painful than the caustic, and its destructive effects are carefully confined to the part

destroyed, without endangering the surrounding skin. This is especially important about the eyelids and face, in little children. If the nævus be in the subcutaneous structure, neither the nitric acid, the potassa fusa, nor the actual cautery will do. You must adopt one of the following methods. Melt a little nitrate of silver in a silver teaspoon, dip a silver probe in it, so as to charge the end of it to the size of a small or large shot, take a fine bistoury and introduce the point through the sound skin at a very little distance from the nævus, and push it into the diseased structure to break it up. Now withdraw the bistoury and immediately push the charged probe through the wound into the nævus and work it about well, then withdraw it, and cover the part with a little lint or plaister. The cure will be almost certain. We have adopted this same plan with a very fine probe, charged with a minute quantity of nitrate of silver, in those small cysts in the eyelids which have generally required excision, only that we push the bistoury directly into the cyst and do the same with the charged probe, and generally from the inside of the eyelid. Cysts of a larger size are not uncommon on the inside of the female labia pudendi which may readily be cured by this simple plan. The ligature is safe and efficacious when we do not care much about taking up a little skin, as on the scalp. Perhaps the best way is to push one or two needles through the structure and strangle it by twisting the ligature round its base under the pins, somewhat in the way of twisting the thread round the pins in cases of hare-lip operations. We have sometimes been struck by the way in which nævi of the scalp in children will cure themselves; we attribute this to the probability of these structures being sometimes fed by vessels passing from the internal to the external surface of the bones, and, as ossification advanced in the child, becoming strangled. Hence in young children we are never in a hurry to destroy nævi on the scalp till ossification of the bones of the head has been completed. Extirpation by excision is not often recommended, except when the nævus exists in muscular tissue. Mr. Gay's method of extirpation, however, is one of the best which we have seen. He passes a hare-lip needle through the skin, below the base of the nævus, so that the ends emerge about one-eighth of an inch on each side beyond the extreme edge of the growth. He then removes the whole of the nævus by incision close to the margin, and instantly brings the sides of the wound together by means of a figure of 8 ligature round the needle, as in hare-lip cases. If one needle be not sufficient more can be introduced, according to the size of the nævus and degree of hemorrhage. This simple operation will in many cases do away with the objections to excision on the score of hemorrhage which has been the great fear in these cases. One pin will obviously be insufficient when the nævus is large or long, just as in hare-lip one or more pins are required according to the length of the wound.

Mr. Ellis's Electric Cautery, which has been greatly improved lately, will be of great use in several obstetric cases, especially in those troublesome nævi or vascular tumours or caruncles which we so often find at and about the orifice of the female urethra. We have often failed in curing these little red bodies by the usual caustics, and generally now at once apply the point of a red-hot skewer, the other end being fitted into a bit of cork—a homely contrivance, certainly, but a very efficacious one, and which can be extemporised in the country, when you are not aware of the nature of the case to which you may be called. When you are consulted at home, however, you can now always have one of Mr. Ellis's instruments. When you apply red or white heat to one of these caruncles, you not only destroy it, as you would do with potassa fusa, but you induce a certain degree of contraction round it, of the contiguous structure, the same as in a burn of the skin. This, no doubt, is a valuable aid in preventing the return of the disease. You can also confine the destructive power to the exact spot which you wish to destroy. This cannot always be done with the other caustics, which spread and generally involve a portion of sound mucous and sub-mucous tissue which is not desirable. In all these cases use a *white* heat rather than a *red* one. The white heat causes comparatively little pain; the red heat is both more painful and less effectual, and it is perhaps from using only a red heat that

the practitioner often fails altogether. When you use the common wire or skewer, or any other metallic body heated in the ordinary way to a white heat, it often becomes only a red heat before you can apply it to the caruncle, and even then soon cools down considerably. But the heat of this electric cautery can not only be applied *white*, but kept so. This is a considerable advantage: and its use may be extended to the os uteri and rectum in some cases where we have hitherto used other caustics. In cases of small nævi about the eyelids in children, the only objection might be that more contraction might follow a small burn than by the usual methods of destruction, but we should not be much afraid of this if we confined the cautery to the nævus itself, and did not implicate any of the surrounding skin.

Dr. Simpson's present mode of exhibiting Chloroform is, to spread a single layer of handkerchief over the face and let the chloroform fall on it drop by drop. Its effects are more rapid with a less quantity. You may thus keep a patient affected for many hours with a very small quantity. It strikes us that in a case of labour this would not always be easily done, although much more easily applied than where a larger quantity is used and the handkerchief repeatedly taken off and re-applied. Our own plan is quite as simple and perhaps sufficiently efficacious in a tedious labour. Put a small quantity of chloroform in a tea-cup, apply the edge to the under surface of the lower lip, and now gradually approximate the cup to the mouth and nose, leaving more or less space for the mixture of atmospheric air. The nurse can manage this while you are otherwise engaged. You use very little chloroform, you can prolong its use for hours, the nurse can do it as well as yourself, and the effect is sufficiently good.

It is computed by Stockhardt that twenty or thirty ounces of phosphorus exist in the bones and other structures of the human body, and that it is the "cementing principle and bond of union to the other components of texture or tissue." Hence when displaced we see rapid disorganisations. For example, when a child is deprived of its phosphate of lime by eating a little alum in its bread, thus forming an insoluble phosphate of alumina, its bones have to depend chiefly on lime and gelatine, and therefore soften, constituting rickets. In children, therefore, we shall often impart vigour and increased health by giving hypophosphite of potash or soda. In women, too, who sometimes are attacked by a similar softening of the bones, implicating especially the pelvis, we may find a valuable remedy in the preparations of phosphorus.

The attacks of Epilepsy in the female are often quite unaccountable, indeed, the same may be said of this disease generally. It has for some time, however, been suspected that these attacks have much to do with some local cause, such as affections of bones or membranes, or the ovaries in the female, often, indeed, with some remote syphilitic affection, inherited or not. At any rate it is interesting to see the effects of the iodide or bromide of potassium in relieving, if not curing, the attacks. Dr Wilks mentions some interesting cases in which he has used, with much benefit, the bromide of potassium in five-grain doses three times a day. The bromide is thought to have some influence over the ovary, and has been also used in these epileptic cases by Sir Charles Locock with some success.

INDEX TO VOL. XLV.

PAGE.

Aconite, Dr. Hanson on nux vomica as an antidote to . . . 279
Adams, Dr. A. L., his case of sphacelus of the leg and foot . . . 125
" Mr. W., on the treatment of lateral curvature of the spine . . 115
Alcohol, MM. Lallemand, Perrin, and Duroy on 270
Albuminuria, Dr. Farre on the influence of arsenic upon . . . 95
" Dr. Williams' cases of recovery from 92
" of fever, Dr. Johnson on the 27
Albuminous urine, Mr. Johnson on 95
Almond flavour, case of poisoning by 285
Althous, Dr. J., on the lithia-springs of Baden-Baden . . . 281
Amputation by rectangular flaps, Mr. Pemberton on . . . 100
" of the forearm, Mr. Hewett on 102
Anæmia, Dr. Chambers on the treatment of 38
" use of the alkaline hypophosphites in 258
Anarcotine, Dr. Garden on the therapeutic value of 29
Aneurism, Mr. Hart on its treatment by flexion of the limb . . . 126
" popliteal, Dr. Bland's new mode of compression in . . 130
" of the femoral artery—embolia 125
Aortic disease of the heart, Dr. Hyde Salter on 53
Aperient, on podophyllin as an 276
Aphthous inflammation of the tongue, Mr. Bryant on . . . 81
Aran, Dr., on codeia as a sedative and narcotic 284
Arsenic, Dr. Bristowe's case of epilepsy treated by 51
Arsenical mucilage, its efficacy in the treatment of cancer . . . 33
" " formula for 35
Arterial murmurs in incipient phthisis, Dr. Kirkes on . . . 248
Artificial anus resulting from sloughing hernia 152
" respiration, Dr. Marcet on a simple method of performing . . 78
Ashton, Mr. T. J., on the treatment of hemorrhoids 152
Asthma, Dr. Kidd's views on the pathology of 68
" Dr. Salter on the pathology of 68
" with emphysema, Dr. Williams on 74
Atropia, Dr. Fleming on the action and uses of 278
" Mr. Spender on the hypodermic action of 277
Atropine paper, Mr. Streatfeild on the use of 192
Barker, Dr. T. H., his case of poisoning by almond flavour . . . 288
Barnes, Dr. R., his case of induction of premature labour . . . 201
Barwell, Mr. R., his new operation for umbilical hernia . . . 129
" on a new method of curing deformities of the feet . . 120
Beale, Dr. L. S., on dumb-bells of oxalate of lime 90
" on the treatment of rheumatic fever 37
Biniodide of mercury ointment in goitre 185
" of mercury ointment in goitre 286
Bladder, diseased, how to relieve pain in 176
Bland, Dr., his new mode of making compression in aneurism . . 30
Blood-letting, Dr. Chambers on 27, 41
Blower, Mr. W., on the medical treatment of strangulated hernia . . 150
Bright's disease, Dr. Williams's cases of recovery from . . . 92
Brinton, Dr. W., on the treatment of tubercular hemoptysis . . . 66

PAGE.

Bristowe, Dr, his case of epilepsy treated by arsenic 51
Bromide of potassium in epilepsy, Dr. Wilks on the use of . . . 49
Bronchial muscles, Dr. Hall on the action of the 75
Bronchitis, acute capillary, treated by quinine 27
Brown, Mr. I. B., his treatment of retained menses by puncture of uterus per rectum 197
" on ovariotomy and its results 226
" on removal of a fibrous tumour from the abdomen . . 217
" on vesico-vaginal fistula 287
Bryant, Mr. T., on aphthous inflammation and ulcer of the tongue . . 81
" on strangulated hernia 39
" on the treatment of nævus 238
" on tumours and deep-seated ulcerations of the tongue . 81
Buhl, Prof., on puerperal fever 214
Budd, Dr. W., on the alleged spontaneous origin of intestinal fever . . 17
Burns and scalds, Dr. Myrtle's treatment of 183
Calomel vapour-bath, Mr. Lee on the 179
Cameron, Dr. J. C., on the value of quinine in tropical diseases . . 27
Camplin, Dr. J., on gluten biscuits for diabetic patients . . . 98
Cancer, Dr. Marsden's treatment by the arsenical mucilage . . . 33
" epithelial, Mr. Gay on the use of pernitrate of mercury in . . 31
" of the tongue, Mr. Moore's cases of division of gustatory nerve in . 158
" of the tongue, successful removal of the entire organ for . . 155
Capsicum, its use in the treatment of delirium tremens . . . 53
Cataract, Mr France on the use of forceps in the extraction of . . 190
Cauliflower excrescence of the os uteri, Dr Hicks on . . . 218
Chambers, Dr. T. K., on the treatment of anæmia 38
" on the renewal of life in continued fever . . 24
Chancre, indurated, Mr. Gay on the use of pernitrate of mercury in . . 31
Child-bed fever, Prof. Ritgen's treatment of 213
Chloroform and chloroform-julep, Dr. Skinner on 253
" Prof. Simpson on a new mode of administering . . . 200
" Dr. Marcet on the causes of death from 280
Cholagogue and cathartic, on podophyllin as a 271
Chorea, Dr. Fraser on the employment of sulphate of aniline in . . 47
" Dr. Turnbull on the efficacy of sulphate of aniline in . . 42
Christophers, Mr. J. C., on the removal of nævi 244
Clark, Dr. A., on sulphuric acid mixture in diarrhœa . . . 86
Clark, Mr. Le Gros, on lithotomy 168
" on lithotomy in the female 168
" on the taxis in strangulated hernia . . . 145
" on the treatment of nævus 239
Cleft-palate, Mr. Field on the operation for 158
" Mr. Pollock on the treatment of 269
Club-foot, on the treatment of 120
Coagula of blood spontaneous expulsion of from the uterus . . . 235
Codeia as a sedative and narcotic 284
Coghlan, Dr. J., on dysmenorrhœa and sterility 195
Coloration of the fæces, Dr. Inman on 86
Concealed accidental hemorrhage at the full term of utero-gestation . . 203
Constipation, efficacy of podophyllin in 276
Continued fever, Dr. Chambers on the treatment of 25
Contracted joints, Mr. Paget on the straightening of 113
Copeman, Dr. E., on the treatment of pneumonia 63
Cornea, Mr. Lawson on paracentesis of the 191
Corneitis, on the insufflation of calomel in 189
Craniotomy, Dr. Harlow's new instrument for 207
Curvature of spine, Mr. Adams on 115
Davies, Mr. R., on the radical cure of reducible hernia . . . 146
Death after lithotomy, on the causes of 171
Debility after parturition, use of hypophosphites in 267
Deformities of the feet, Mr. Barwell on the treatment of . . . 120
Delirium tremens, Dr. Duchesne on the use of digitalis in . . . 52
" Mr. Ferneley on the use of capsicum in . . . 53
Dentition, use of hypophosphite of potash in the fever of . . . 265
Diabetic, Dr. Camplin on gluten biscuits for the 98
" Dr. Pavy on the diet proper for the 98

PAGE
Diabetes, Dr. Pavy on the dietetic treatment of 97
" Dr. Pavy on the influence of an acid in producing . . 96
" Dr. Pavy on the influence of alkalies on 96
Diaclast, description of Dr Harlow's 207
Diarrhœa, Dr. Clark on sulphuric acid mixture in 86
" in children, Dr. Wilks on the treatment of . . . 244
Digitalis, its use in delirium tremens 52
Diphtheria, Dr. Ormerod on the pathology of 83
" Dr. Walker on the treatment of 85
Diplopia, Dr. Wells on the treatment of 187
Dislocation of shoulder-joint, Prof. Smith's method of reducing . . 114
Dry labours, Dr. Langer on preparing for turning in . . . 206
Duchesne, Dr. R., on digitalis in delirium tremens 52
Dysentery, tropical, Dr. Cameron on the employment of quinine in . . 27
Dysmenorrhœa, M. Vannaire on veratrine ointment in . . . 197
" and sterility, Dr. Coghlan on 195
Dysmenorrhœal membrane, spontaneous expulsion of . . . 235
Dyspepsia, use of hypophosite of potash in 265
Earle, Dr. L., on sulphuric and gallic acids as anti-hemorrhagics . . 283
Electric cautery, Mr. Ellis on its use in uterine affections . . . 220
Electricity in paralytic affections of the eye 189
Ellis, Mr. R., on the electric cautery in affections of the uterus . . 220
" on ulcers of the os and cervix uteri 221
Encysted tumour of the labium, ablation of entire cyst . . . 232
" of the labium treated by free incision . . . 231
Epilepsy, its treatment by bromide of potassium 49
" suddenly and remarkably relieved by arsenic . . . 51
Epistaxis, Mr. Macnamara on plugging the nostrils in . . . 131
Evans, Mr. C., his new fracture apparatus 252
Extraction of cataract, on the use of forceps in 190
Eye-instruments, Mr. Streatfeild on the advantages of having them gilded 191
Fæces, Dr. Inman on coloration of the 86
Farre, Dr. F., on the influence of arsenic on albuminuria . . . 95
Feet, Mr. Barwell on the treatment of deformities of the . . . 120
Fever, Dr. Cameron on the value of quinine in 27
" Dr. Johnson on the albuminuria of 30
" continued, Dr. Chambers on the treatment of . . . 25
" intestinal, Dr. Budd on the origin of 17
" puerperal, Prof. Buhl on the pathology of 214
" puerperal, Prof. Ritgen's treatment of 215
" puerperal, Drs. Fox and Tyler Smith on 212
" puerperal, Dr. West on 209
" puerperal, in the Munich Lying-in Hospital . . . 210
" remittent, its treatment by anarcotine 29
" typhoid, Mr. Simon on the causation of 21
" typhus, Prof. Griesinger on 24
" typhus, Dr. Gull on 23
Fibrous tumour, Mr. Brown's case of removal of from the abdomen . 217
" tumours spontaneously expelled from the uterus . . . 232
Field, Mr. G. A., on cleft-palate 158
Fistula, vesico-vaginal, Mr. Brown's treatment of . . . 287
" vesico-vaginal, Dr. Sims on 229
Fleming, Dr., on the action and uses of atropia 278
" on the cause and cure of tape-worm 88
Fore-arm, Mr. Hewett on amputation of the 102
Fracture apparatus, Mr. Evans on a new 253
France, Mr. J. F., on the use of forceps in extraction of cataract . . 190
Gangrene of the leg and foot—amputation and recovery . . . 125
Gangrenous abscess of the lung, treatment of 63
Garden, Dr. A., on anarcotine in remittent fevers 29
Gay, Mr. J., on the excision of nævi 242
" on pernitrate of mercury in epithelial cancer, &c. . . 31
Gillespie, Dr. J. D., on the wire seton in hydrocele 174
Goitre, Mr. Greenhow on biniodide of mercury ointment in . . 185
" Mr. Stainthorpe on biniodide of mercury ointment in . . 286
Gout, Dr. Ruef on the efficacy of lithia water in 282

PAGE.
Gravid uterus, Dr. Skinner on retroversion of the 199
Greenhow, Mr. H. M., on the treatment of goitre 185
Griesinger, Prof., on typhus fever 24
Gull, Dr., on typhus fever 23
Hall, Dr. C. R., on the action of the bronchial muscles . . . 75
Hanson, Dr. D. D., on nux vomica as an antidote to aconite . . . 279
Hare-lip, Mr. Field on the operation for 158
" Mr. Pollock on the treatment of 269
Harlow, Dr. L. D., his new instrument for craniotomy . . . 207
Hart, Mr. E., on the treatment of aneurism by flexion of the limb . . 126
Haydon, Dr. N. J., his case of syphilis transmitted by vaccination . . 178
Head, Mr. West on some injuries of the 124
Heart-dyspnœa, Dr. Hyde Salter on 53
Hematuria, Dr. Salter on 89
Hemoptysis, Dr. Brinton on the treatment of 66
" Dr. Williams on the treatment of 80
Hemorrhage, uterine, Dr. Wilson's cases of concealed accidental . . 203
Hemorrhoids, Mr. Ashton on the treatment of 152
Hernia, Mr. Davies's operation for the radical cure of . . . 146
" strangulated, Mr. Blower on the medical treatment of . . 150
" strangulated, Mr. Bryant on 139
" strangulated, Mr. Bryant on opening the sac in . . . 142
" strangulated, Mr. Le Gros Clark on the taxis in . . . 145
" umbilical, Mr. Barwell's operation for 149
" umbilical, Mr. Bryant on 142
Herpes, circinatus, treatment of 180
Hewett, Mr. P., on amputation of the fore-arm 102
Hewett, Dr. G., on substances expelled from the vagina . . . 232
" on the uterine douche 208
Hicks, Dr. J. B., on cauliflower excrescence of the os uteri . . . 218
" his wire-rope écraseur for intra-uterine polypi . . 231
Hillier, Dr. T., on ringworm and the vegetable parasites . . . 180
Hilton, Mr. J., on diagnosis of disease of the hip-joint . . . 105
" on the influence of rest on accidents and surgical diseases . 102
Hip-joint, Mr. Hilton on diagnosis of disease of the . . . 105
Housemaid's knee, Mr. Martyn's treatment of 113
Hydatids, uterine, Mr. Tweedale's case of 238
" uterine, expulsion of 237
Hydrocele, Dr Gillespie on the wire seton in 174
Hydrocephalus, Dr. Wilks on the state of the pupil in . . . 244
Hypodermic action of atropia, Mr. Spender on the 277
Hypophosphites, alkaline, Mr. Taylor on 258
" chemical constitution of the 258
Indolent ulcers, Mr. Mitchell's treatment of 185
Injecting apparatus for the rectum and vagina, Dr. Simpson on a new . 216
Inman, Dr. T., on the coloration of the fæces 86
Intestinal fever, Dr. Budd on the alleged spontaneous origin of . . 17
Iodide of potassium, Mr. Paget on 310
Iron, Mr. Gentles on a new salt of 283
Iron-wire ligatures for arteries, Mr. Nunneley on 252
Johnson, Dr. G., on the albuminuria of fever 30
Johnson, Mr. M., on albuminous urine 95
Joints, Mr. Hilton on the influence of rest on diseases of the . . 102
" contracted, Mr. Paget on the straightening of . . . 113
Kidney, cases of recovery from Bright's disease of the . . . 92
Kirkes, Dr. W. S., on arterial murmurs in incipient phthisis . . 248
Labour, premature, Dr. Barnes' case of induction of 201
Lactation, prolonged, use of hypophosphite of potash in . . . 258
Lallemand, Perrin, and Duroy on alcohol 270
Langer, Dr., on preparing for turning in dry labours 206
Lawson, Mr. G., on paracentesis of the cornea 191
Leared, Dr. A., on acupuncture and galvanism in rheumatism . . 35
Lee, Mr. H., on the calomel vapour bath 179
" on the operation for varicocele 171
Leucocythæmia, use of the alkaline hypophosphites in . . . 258
Ligatures, iron-wire, for arteries, Mr. Nunneley on 252

PAGE.
Lithia water, Dr. Althaus on its efficacy in gout 281
Lithotomy, Mr. Le Gros Clark on 168
" Mr. Thompson on the causes of death following . . . 164
" and lithotrity, Mr. Thompson on 161
" and lithotrity, Mr. Thompson on 255
" in the female, Mr. Le Gros Clark on 168
Little, Dr., on the plaster of Paris splint 123
Lloyd, Mr. A., on oxalate of cerium in the vomiting of pregnancy . . 198
Lung, Dr. Williams on gangrenous abscess of the 62
Lupus exedens, Mr. Gay on the use of pernitrate of mercury on . . 31
Macnamara, Mr. R., on plugging the posterior nares . . . 131
McLosky, Dr. P., on the management of the placenta 205
M' Whinnie, Mr. A. W., on tracheotomy 135
M' William, Dr., on the Indian remedy for small-pox 38
Marcet, Dr. W., on a simple method of performing artificial respiration . 78
" on suspended animation from chloroform . . . 280
Marsden, Dr. W., his cases of cancer successfully treated . . . 33
Martyn, Mr. R. W., his treatment of "housemaid's knee" . . . 113
Milk, Dr. Harriss' mode of arresting the secretion of 209
Mitchell, Mr. J., his treatment of indolent ulcers 185
Moore, Mr. C. H., on division of the gustatory nerve for cancer of tongue . 158
Myrtle, Dr. J. Y., on the treatment of scalds and burns . . . 183
Nævus, Mr. Bryant on the treatment of 238
" Mr. Christophers on the removal of 243
" Mr. Le Gros Clark on the treatment of 239
" in the forehead of an infant, Mr. Gay's case of . . . 242
Neuralgia, Dr. O' Connor's cases treated by valerianate of ammonia . . 48
New salt of iron, Mr. Gentles on a 283
Nunneley, Mr. T., his case of removal of the entire tongue . . . 172
" on iron-wire ligatures for arteries 252
Nux vomica as an antidote in poisoning by aconite 279
Ophthalmiæ, Dr. Wells on the insufflation of calomel in . . . 189
Ormerod, Dr. E. L., on the pathology of diphtheria 83
Orthopnœa, or "upright breathing," Dr. Salter on 54
Ovarian cysts, Mr. Well's trochar-syringe for injecting . . . 228
" cysts and tumours, Mr. Wells on the treatment of . . . 222
Ovariotomy, Dr. Routh on 226
" Mr. I. B. Brown on 226
" Mr. Spencer Wells on 226
Ovum expelled from the uterine passages 232
Oxalate of cerium in the vomiting of pregnancy 198
" of lime, Dr. Beale on the dumb-bells of 90
" of lime deposits, how to discriminate 246
Paget, Mr. J., on the straightening of contracted joints . . . 113
Pain in diseased bladder, to relieve 176
Painful menstruation, use of veratrine in 197
" muscular and fascial contractions along the vagina . . . 215
Paracentesis of the cornea, Mr. Lawson on 191
Paralysis of the muscles of the eye, Dr. Wells on electricity in . . 187
Pavy, Dr. F. W., on the diet of diabetic patients 98
" on the dietetic treatment of diabetes . . . 97
" on the influence of alkalies on artificial diabetes . . 96
" on the influence of an acid in producing diabetes . . 96
Pemberton, Mr. O., on amputation by rectangular flaps . . . 100
Pepsine in the vomiting of pregnancy 198
Pernitrate of mercury, its use in epithelial cancer, &c. 31
Phthisis, incipient, on arterial murmurs in 248
" on the use of the hypophosphites in 258
Placenta, Dr. McLosky on the management of the 205
Placenta-prævia at eight months, Dr. Ramsbotham's case of . . . 206
Placentæ, expulsion of at an early period of gestation . . . 232
Plaster-of-Paris splint, Dr. Little on the 123
Pleurisy, Dr. Williams on the treatment of 65
" and pneumonia, Dr. Williams on the chronic results of . . 61
Plugging the posterior nares, Mr. Smyly on 131
Pneumonia, Dr. Copeman on the treatment of 63

PAGE.

Pneumonia, Dr. Williams on the treatment of 60
Podophyllin, Editor of Lancet on the medicinal properties of . . 271
" its extensive employment in America 271
Poisoning by aconite, use of nux vomica as an antidote . . . 279
" by almond flavour, Dr. Barker's case of 285
Pollock, Mr. G., on uranoplastics 269
Polypi, uterine, Dr. Tyler Smith's instrument for removal of . . 237
" uterine, removal of the cervix 222
Polypus, intra-uterine, removed by Dr. Hicks' wire-rope écraseur . . 231
Popliteal aneurism, its cure by flexion of the limb 126
" new mode of making compression in . . . 130
Pregnancy, on the treatment of the vomiting of 198
Premature labour, Dr. Barnes' case of induction of 201
Prunus Virginiana, medicinal properties of 311
Puerperal fever, Prof. Buhl on the pathological anatomy of . . . 214
" Prof. Ritgen's treatment of 213
" Drs. Fox and Tyler Smith on 212
" in the Lying-in-Hospital at Munich 210
" regarded as an epidemic, Dr. West on . . . 209
Pulmonary congestion of heart-disease and of asphyxia, differences of . 53
" hemorrhage, Dr. Brinton on 66
Quinine, its value in fevers and other diseases 27
Ramsbotham, Dr. F. H., his case of placental-presentation at eight months . 207
Rectangular flaps, Mr. Teale's mode of amputation by . . . 92
Remittent fever, its treatment by anarcotine 29
Respiration, artificial, Dr. Marcet's method of 78
Rest, Mr. Hilton on its influence in accidental and surgical diseases . . 102
Retained menses of two years' duration, Mr. Brown's case of . . 197
Retroversion of the gravid uterus, Dr. Skinner on 199
Rheumatic fever, Dr. Beale on the treatment of 37
" paralysis, Dr. Leared on galvanism in 35
Rheumatism, muscular, Dr. Leared on acupuncture in 35
Ringworm and the vegetable parasites, Dr. Hillier on . . . 180
Roberts, Dr. W., on the discrimination of urinary deposits . . . 246
Salter, Dr. H., on hematuria 89
" on regurgitant aortic disease of the heart . . . 53
" on the pathology of asthma 68
Scalds and burns, Dr. Myrtle on the treatment of 183
Sedative, Dr. Aran on codeia as a 284
Shoulder-joint, Prof. Smith's method of reduction of dislocation of . . 114
Skinner, Dr. T., on chloroform and chloroform-julep 253
" on retroversion of the gravid uterus 199
Skull, Mr. West on fracture of the 124
Simon, Mr. J., on the causation of typhoid fever 21
Simpson, Dr. J. Y., on a new injecting apparatus 216
" on a new mode of administering chloroform . . 200
" on painful muscular contractions of the vagina . . 215
Sims, Dr. M., on vesico-vaginal fistula 229
Small-pox, Indian remedy for 38
Smith, Prof. N. R., his new method of reduction of dislocation of shoulder-joint 114
Spender, Mr. J. K., on the hypodermic action of atropia . . . 277
" on the treatment of varicose ulcers without rest . 184
Spermatozoa in the vagina eight and a half days after intercourse . . 198
Sphacelus of the leg and foot, Dr. Adams' case of 125
Spine, Mr. Adams on the treatment of lateral curvature of . . . 115
Splint, method of making with plaster of Paris 123
Sterility and dysmenorrhœa, treatment by operation . . . 195
Strangulated hernia, Mr. Blower on the medical treatment of . . 150
" hernia, Mr. Bryant on 139
" hernia, Mr. Bryant on opening the sac in . . . 142
" hernia, Mr. Le Gros Clark on the taxis in . . . 145
Streatfeild, Mr. J. F., on eye-instruments 191
" on the use of atropine paper 192
Stricture of the urethra, Prof. Syme's case of congenital . . . 160
Strychnia as an antidote in poisoning by aconite 279
Styptic, Dr. Earle on sulphuric and gallic acids as a . . . 283

PAGE
Substances expelled from the generative passages of women, Dr. Hewett on 232
Sulphate of aniline, on its use in chorea 42
Sulphuric acid mixture for diarrhœa, formula for 86
" and gallic acids as anti-hemorrhagics, Dr. Earle on . . 283
Suspended animation from chloroform, Dr. Marcet on the treatment of . 280
Syme, Prof., his case of congenital stricture of the urethra . . . 160
Syphilis, Mr. Lee on the calomel vapour bath in 179
" appearing after vaccination from a syphilitic child . . . 178
" conveyed by vaccination to forty-six children . . . 176
Tape-worm, Dr. Fleming on the cause and cure of 88
Taxis in strangulated hernia, Mr. Le Gros Clark on 145
Taylor, Mr. J., on the alkaline hypophosphites 229
Thompson, Mr. H., on practical lithotomy and lithotrity . . . 161
" on practical lithotomy and lithotrity . . . 255
" on the cause of death following lithotomy . . 104
Tinea tonsurans, treatment of 180
Tongue, Mr. Bryant on tumours and ulcerations of the . . . 81
" Mr. Moore on division of the gustatory nerve in cancer of . . 138
" Mr. Nunneley's case of successful removal of . . . 155
" on aphthous inflammation and ulcer of the . . . 81
Tracheotomy, Mr. M'Whinnie on 135
Tuberculosis and uterine disease 238
Tufnell, Mr. J., on the radical cure of varicocele 172
Tumour, fibrous, removal of from within the abdomen . . . 217
" of the labium, encysted, entire ablation of 232
" of the labium, encysted, treated by free incision . . . 231
Tumours and deep-seated ulcerations of the tongue 81
Turnbull, Dr. J., on sulphate of aniline in chorea 42
Turning in dry labours, on preparing for 206
Tweedale, Mr. T., his case of uterine hydatids 238
Typhoid fever, Dr. Budd on the contagious propagation of . . . 17
" Mr. Simon on the causation of 21
Typhus fever, Prof. Griesinger on 24
" Dr. Gull on 23
Ulcerations of the cervix, use of the electric cautery for . . . 220
Ulcer of the tongue, Mr. Bryant on 81
" of the leg, indolent, new mode of treating 185
" of the leg, varicose, treated without rest 184
" of the os and cervix uteri, Mr. Ellis's table of the varieties and treatment of 221
Umbilical hernia, Mr. Barwell's new operation for 149
Urethra, stricture of, Prof. Syme's case of 160
Uric acid, deposits in the urine, discrimination of 246
Urinary deposits, Dr. Roberts on the discrimination of . . . 246
Urine, Dr. Beale on the dumb-bells of oxalate of lime in . . . 90
" albuminous, on a source of fallacy in examining . . . 95
Uterine disease and tuberculosis 237
" douche, Dr. Hewett's new apparatus 208
" hemorrhage, Dr. Wilson's cases of 203
" hydatids, Mr. Tweedale's case of 238
" polypi, Dr. Smith's instrument for removal of . . . 237
Uterus, puncture of per rectum for retained menses 197
Vaccination, case of syphilis transmitted by 178
" syphilis transmitted to forty-six children by . . . 176
Vagina, Prof. Simpson on a new injecting apparatus for the . . . 216
" Prof. Simpson on painful muscular contractions of the . . 215
Valerianate of ammonia, its use in cases of severe neuralgia . . . 48
Varicocele, Mr. Lee on the operation for 171
" Mr. Tufnell on the radical cure of 172
Varicose ulcers, Mr. Spender on their treatment without rest . . 184
Veratine in painful menstruation 197
Vesico-vaginal fistula, Dr. Marion Sims on 229
" fistula, Mr. I. B. Brown on the treatment of . . . 287
Vomiting of pregnancy, Dr. Morris on pepsine in 199
" of pregnancy, Mr. Lloyd on oxalate of cerium in . . . 198
Walker, Dr. J. W., on the treatment of diphtheria 85
Wells, Dr. S., on electricity in paralysis of muscles of the eye . . 187

PAGE.
Wells, Dr. S., on the insufflation of calomel in ophthalmiæ . . . 189
Wells, Mr. T. S., his trochar-syringe for injecting ovarian cysts . . 228
" on the treatment of large ovarian cysts and tumours . 222
West, Dr. R. U., on puerperal fever regarded as an epidemic . . 209
West, Mr. J. F., on some injuries of the head 124
Wilks, Dr. S, on bromide and iodide of potassium in epilepsy . . 49
" on the state of the pupil in hydrocephalus . . . 243
" on the treatment of diarrhœa in children . . . 244
Williams, Dr. T, his cases of recovery from Bright's disease . . 92
Williams, Dr. C. J. B., on asthma with emphysema 74
" on gangrenous abscess of the lung . . . 62
" on the chronic results of pleurisy and pneumonia . 61
" on the treatment of hemoptysis . . . 80
" on the treatment of pleurisy 65
" on the treatment of pneumonia . . . 68
Wilson, Dr. J. G., his cases of concealed accidental uterine hemorrhage . 203
Wire seton in hydrocele, Dr. Gillespie on the 174

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REGULAR TERM.

The Regular Term will commence on Wednesday, Oct. 15, 1862, and end early in March, 1863.

During the whole of the Session the Student will have the opportunity of attending, at least, two Clinical Lectures daily. In addition to these, during the regular term, three Didactic Lectures are given on every week-day, except Saturday. The Didactic Lectures are so arranged as not to interfere with attendance in the Hospital wards. Ample time is allowed for accompanying the Visiting Physicians, Surgeons, and Obstetricians in their daily rounds, attending clinical lectures, witnessing surgical and obstetrical operations, and following private courses, without compromising in any degree the regular didactic instruction. Clinical and Demonstrative teaching constituting the great feature of this College, the arrangements are such as to render the immense resources of the Hospitals available to the Student to the fullest extent.

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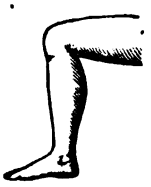

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